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Norbert Schwarz

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Zentrum für Umfragen, Methoden und Analysen e.V. (ZUMA)
Postfach 12 21 55
6800 Mannheim 1

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Judgment in a Social Context: Biases, Shortcomings, and the Logic of Conversation

Norbert Schwarz
Zentrum fuer Umfragen, Methoden und Analysen,
ZUMA, Mannheim, FRG
and
Institute for Social Research
University of Michigan, Ann Arbor, USA

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I. Introduction

The mainstream of social cognition research can be described as the application of principles and methods of cognitive psychology to social stimuli. While the information processing paradigm, to which social cognition research is committed (Ostrom, 1984), stimulated an enormous research productivity in social psychology, it has frequently been criticized as being asocial in nature. In the eyes of many critics, its concentration on individuals as isolated information processors fostered a neglect of the social context in which human judgment occurs, prompting Schneider (1991, p. 553) to ask, "Where, oh where, is the social in social cognition?". As Forgas (1981, p.3) observed, following the adoption of the information processing paradigm "social psychology found itself transformed into a field now mainly concerned not with human social action, but with human beings as thinkers and information processors about social stimuli." However, even the study of "human beings as thinkers and information processors" is likely to suffer from this neglect. On close inspection, it seems that much of what we consider to reflect biases in human judgment, artifacts in attitude measurement, and so on, may actually reflect researchers' ignorance regarding the conversational context of human judgment, rather than serious shortcomings on the side of our subjects. Accordingly, social cognition research may greatly benefit from a fuller consideration of the social context in which humans conduct much of their thinking about social as well as non-social stimuli. A promising starting point for this endeavor is provided by psycholinguistic work into the tacit assumptions that govern the conduct of conversation in everyday life.

The present chapter reviews these tacit assumptions and explores some of their implications for social cognition research. At present, social cognition researchers have primarily paid attention to conversational aspects of human judgment in exploring attribution processes (e.g., Hilton & Slugoski, 1986; Edwards & Potter, 1993) and the impact of different audiences on the encoding and recall of person information (e.g., Higgins, McCann, & Fondacaro, 1982; Higgins & Rholes, 1978). Given that excellent reviews of that work are available (see Hilton, 1990, 1991; McCann & Higgins, 1992), the present chapter focuses on the contribution of conversational processes to the emergence of biases and shortcomings in human judgment, drawing on research in judgment and decision making, attitude measurement, and questionnaire construction.

II . The Logic of Conversation

As Clark and Schober (1992, p. 15) noted, it is a "common misperception that language use has primarily to do with words and what they mean. It doesn't. It has primarily to do with people and what they mean. It is essentially about speakers' intentions." Determining a speakers' intention, however, requires extensive inferences on the part of listeners. Similarly, designing an utterance to be understood by a given listener requires extensive inferences on the side of the speaker. In making these inferences, speakers and listeners rely on a set of tacit assumptions that govern the conduct of conversation in everyday life. In their most widely known form, these assumptions have been expressed as four maxims by Paul Grice (1975), a philosopher of language. Subsequent researchers have elaborated on these assumptions, specifying their implications for speakers and listeners (see Clark & Brennan, 1991; Clark & Schober, 1992; Higgins, 1981; Higgins, Fondacaro, & McCann, 1982; Levinson, 1983).

The Logic of Conversation in Everyday Life

A maxim of manner asks speakers to make their contribution such that it can be understood by their audience. To do so, speakers do not only need to avoid ambiguity and wordiness, but have to take the characteristics of their audience into account, designing their utterance in a way that the audience can figure out what they mean – and speakers are reasonably good at doing so (Krauss & Fussel, 1991). At the heart of this process are speakers' assumptions about the information that they share with recipients, that is, the common ground (Schiffer, 1972; Stalnaker, 1978). Listeners, in turn, assume that the speaker observes this maxim and interpret the speaker's utterance against what they assume to constitute the common ground (e. g., Clark, Schreuder, & Buttrick, 1983; Fussel & Krauss, 1989a,b). Whereas the initial assumptions about the common ground are based on the participants' assumptions about their cultural and personal background, each successful contribution to the conversation extends the common ground of the participants, reflecting that "in orderly discourse, common ground is cumulative" (Clark & Schober, 1992, p. 19).

This cumulative nature of the common ground reflects, in part, the operation of a maxim of relation that enjoins speakers to make all contributions relevant to the aims of the ongoing conversation. This maxim entitles listeners to use the context of an utterance to disambiguate its meaning by making bridging inferences (Clark, 1977). Moreover, this maxim implies that speakers are unlikely to assume that a contribution to a conversation is irrelevant to its goal, unless it is marked as such. As Sperber and Wilson (1986, p. vi) noted, "communicated information comes with a guarantee of relevance" and if in doubt, it is the listener's task to determine the intended meaning of the utterance by referring to the common ground or by asking for clarification.

In addition, a maxim of quantity requires speakers to make their contribution as informative as is required, but not more informative than is required. That is, speakers should respect the established, or assumed, common ground by providing the information that recipients need, without reiterating information that recipients already have (Clark & Haviland, 1977; Prince, 1981). Finally, a maxim of quality enjoins speakers not to say anything they believe to be false or lack adequate evidence for.

Table 1: Rules of the Communication Game

Communicators should:

1. take the recipient's characteristics into account;
2. try to be understood (i.e., be coherent and comprehensible);
3. give neither too much nor too little information;
4. be relevant;
5. produce a message that is appropriate to the context and the circumstances;
6. produce a message that is appropriate to their communicative intent or purpose;
7. convey the truth as they see it;
8. assume that the recipient is trying, as much as possible, to follow the rules of the communication game.

Recipients should:

1. take the communicator's characteristics into account;
2. determine the communicator's communicative intent or purpose;
3. take the context and circumstances into account;
4. pay attention to the message and be prepared to receive it;
5. try to understand the message;
6. provide feedback, when possible, to the communicator concerning their understanding of the message.

Note. Adapted from McCann, C. D., & Higgins, E. T. (1992). Personal and contextual factors in communication: A review of the 'communication game'. In G. R. Semin & K. Fiedler (Eds.), Language, interaction, and social cognition (pp. 144-172). Newbury Park, CA: Sage. Reprinted by permission.

Table 1, adapted from McCann and Higgins (1992), summarizes the implications of these maxims in the form of "rules" that speakers and listeners are supposed to follow. These rules apply most directly to situations in which participants attempt to exchange information or to get things done. Obviously, conversations may be characterized by other goals (see Higgins, Fondacaro, & McCann, 1981), such as entertaining one another, in which case participants may not assume that the usual conversational maxims are observed. Given that the present chapter is concerned with conversational processes in research settings, however, the adjustments required by different conversational goals

do not need further elaboration. In general, research participants are likely to perceive the research situation as a task oriented setting in which participants attempt to exchange information as accurately as possible, thus rendering the assumptions underlying task oriented conversations highly relevant.

In summary, according to the tacit assumptions that govern the conduct of conversation in daily life, "communicated information comes with a guarantee of relevance" (Sperber & Wilson, 1986, p. vi) and listeners are entitled to assume that the speaker tries to be informative, truthful, relevant, and clear. Moreover, listeners interpret the speakers' utterances "on the assumption that they are trying to live up to these ideals" (Clark & Clark, 1977, p. 122)[1].

The Logic of Conversation in Research Settings

What, then, are the implications of these tacit assumptions for communicative processes in research situations, most notably psychological experiments and survey interviews? As many researchers have noted (e.g., Clark & Schober, 1992; Strack, in press; Strack & Schwarz, 1992), "conversations" in research settings differ from natural conversations by being highly constrained. Whereas speakers and addressees collaborate in unconstrained natural conversations "to establish intended word meanings, intended interpretations of full utterances, implications of utterances, mutually recognized purposes, and many other such things" (Clark & Schober, 1992, p. 25), their opportunity to do so is severely limited in research settings, due to the researcher's attempt to standardize the interaction. Most importantly, the standardization of instructions, or of the questions asked, precludes that the utterances can be tailored to meet different common grounds. Moreover, when research participants ask for clarification, they may often not receive additional information. Rather, the previously given instructions may be repeated or a well-trained interviewer may respond, "Whatever it means to you", when asked to clarify a question's meaning. In some cases, as when a respondent is asked to complete a self-administered questionnaire, there may also be nobody who can be asked for clarification. As a result, a mutual negotiation of intended meaning is largely precluded in many research situations.

Nevertheless, research participants will attempt to cooperate by determining the intended meaning of the researcher's contributions to the constrained conversation. To do so, they will rely even more on the tacit assumptions that govern the conduct of conversation in daily life than they would under less constrained conditions. And these assumptions grant them every right to do so. That communicators are supposed to design their utterances such that they will be understood by addressees

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[1] For an introduction to other aspects of conversational conduct, including nonverbal behavior, see Chapter 2 of Argyle, 1992.

implies an interpretability presumption, as Clark and Schober (1992, p. 27) noted. This presumption is emphasized by the fact that the researcher as communicator obviously does not foresee any difficulties with the comprehensibility of his or her utterances, or else he or she would have taken appropriate precautions. As a result, research participants will refer to the conversational maxims in inferring the researcher's intended meaning. Hence, they will assume that every contribution of the researcher is relevant to the aims of the ongoing conversation; that every contribution is informative, truthful, and clear; and they will refer to the context of the conversation to resolve any ambiguities that may arise.

Unfortunately, however, research participants are bound to miss one crucial point: Whereas the researcher is likely to comply with conversational maxims in almost any conversation he or she conducts outside of a research setting, the researcher is much less likely to do so in the research setting itself. In fact, the researcher may violate each and every maxim of conversation by providing information that is neither relevant, nor truthful, informative and clear -- and may have explicitly designed the situation to suggest otherwise. Research participants, however, have no reason to suspect that the researcher is not a cooperative communicator and are hence likely to work hard at finding meaning in the researcher's contributions.

The findings reviewed below suggest that this basic misunderstanding about the cooperative nature of communication in research settings has contributed to some of the more puzzling findings in social and psychological research and is, in part, responsible for the less than flattering picture of human judgmental abilities that has emerged from social cognition research.

III. Communicated Information Comes with a Guarantee of Relevance

One of the key assumptions underlying the conduct of conversation holds that all information contributed by participants is relevant to the goal of the ongoing conversation. As noted above, research participants have no reason to assume that this maxim of relevance does not hold in a formal research setting. Accordingly, they assume that all information offered to them by the researcher is relevant to their task -- and will work hard at making sense of it. This implicit guarantee of relevance contributes in important ways to several pervasive biases that have been documented in judgment research and underlies many apparent "artifacts" in attitude and opinion measurement that have captured the attention of survey methodologists.

A. The Conversational Relevance of "Irrelevant" Information: If the Experimenter Presents It, I Should Use It

Social psychologists have long been intrigued by subjects' readiness to rely on individuating

information of little diagnostic value at the expense of more diagnostic information. Prominent examples of this general bias include the neglect of information about situational factors in explaining the behavior of an actor and the underutilization of base-rate information. As most robust phenomena, both of these biases are likely to have many determinants, as Ross and Nisbett (1991) noted. Nevertheless, several studies indicate that the conversational guarantee of relevance contributes to a considerable degree to the size of the typically obtained effects.

1. The Underutilization of Base-Rates

Numerous studies have demonstrated a pronounced bias to rely on individuating information of little diagnostic value at the expense of more diagnostic base-rate information (see Nisbett & Ross, 1980, for a review). Although the initial conclusion that individuating information will typically overwhelm the impact of base-rate information (e.g., Kahneman & Tversky, 1973; Nisbett & Borgida, 1975) has been called into question by subsequent studies (see Ginossar & Trope, 1987, for a review), the frequently observed underutilization of base-rate information has continued to be a key topic in judgment and decision research. An analysis of the experimental procedures used indicates, however, that the often dramatic findings are, in part, a function of conversational processes rather than of features naturally inherent to base-rate or individuating information.

In what is probably the best-known demonstration of base-rate neglect, Kahneman and Tversky (1973) told their subjects that a target person described to them "shows no interest in political and social issues and spends most of his free time on his many hobbies which include home carpentry, sailing, and mathematical puzzles". These subjects predicted that the target person is most likely an engineer, independently of whether the base-rate probability for any target being an engineer was .30 or .70. An analysis of the instructions used in this study proves informative. Specifically, the instructions read (emphases added):

> "A panel of psychologists have interviewed and administered personality tests to 30 (resp., 70) engineers and 70 (resp. 30) lawyers, all successful in their respective fields. On the basis of this information, thumbnail descriptions of the 30 engineers and 70 lawyers have been written. You will find on your forms five descriptions, chosen at random from the 100 available descriptions. For each description, please indicate your probability that the person described is an engineer, on a scale from 0 to 100.
>
> The same task has been performed by a panel of experts who were highly accurate in assigning probabilities to the various descriptions. You will be paid a bonus to the extent that your estimates come close to those of the expert panel."

The first part of the instructions informs subjects that the individuating information was compiled by psychologists on the basis of respected procedures of their profession, namely interviews and tests. Given that lay-persons assume psychologists to be experts on issues of personality (rather than base-rates), this introduction emphasizes the relevance of the individuating information. Moreover, other experts -- most likely psychologists as well, given the present context -- are said to be highly accurate in making these judgments, thus further increasing the relevance of the individuating information. The subjects' task is then defined as determining a probability that matches the judgments of the experts. If these experts are assumed to be psychologists, subjects can infer that the experimenter wants them to use the same information that these experts used -- which is most likely the personality information compiled by their colleagues.

Finally, as the experiment proceeds, subjects are asked to judge several target persons for whom different individuating information is presented. The base-rate information about the sample from which the targets are drawn, on the other hand, is held constant. This further suggests that the individuating information is of crucial importance because this information provides different clues for each judgment and in the absence of this information all tasks would have the same solution. Thus, the instructions and procedures of Kahneman and Tversky's classic study allowed subjects to infer (however incorrectly) the experimenter's intention that they should base their judgment on the individuating information. It therefore comes as little surprise that subjects relied on it when making their judgments. After all, they had no reason to assume that the experimenter violated each and every of the Gricean maxims by providing information that is neither relevant, nor truthful, informative and clear.

To test this conversational analysis of the base-rate fallacy, Schwarz, Strack, Hilton, and Naderer (1991, Experiment 1) tried to undermine the guarantee of relevance that characterizes human communication in a modified partial replication of Kahneman and Tversky's study. Some subjects were told that the person description was written by a psychologist, replicating the instructions used by Kahneman and Tversky. This entitles the recipient to assume that the presented information obeys the normative rules of communication and reflects a particular communicative intention on the part of the experimenter. Other subjects were told that the (identical) description was compiled by a computer that drew a random sample of descriptive sentences bearing on the target person. Obviously, the co-operative principle does not directly apply to the resulting communication and the communicative intention cannot be unambiguously inferred. While the database from which the computer drew the sentences was said to have been compiled by psychologists, the collection drawn by the computer is of dubious relevance.

As expected, undermining the implicit guarantee of relevance greatly attenuated subjects'

reliance on the individuating information. Specifically, subjects in the replication condition estimated the likelihood of the target being an engineer as .76, despite a low base-rate of .30. However, when the same information was allegedly selected by a computer, their likelihood estimate dropped to .40. This attenuation indicates that subjects' reliance on individuating information at the expense of base-rate information reflects, in part, their assumption that the experimenter is a cooperative communicator who does not present information that is irrelevant to the task at hand. Accordingly, they tried to find "relevance" in the information provided to them, unless the implicit guarantee of relevance was called into question.

In a similar vein, Krosnick, Li, and Lehman (1990) observed that the utilization of base-rate information varied as a function of the order in which the base-rate and the individuating information were presented. In seven studies, using a variety of different problems, subjects were more likely to use base-rate information when this information was presented after rather than before the individuating information. On first glance, such a recency effect may suggest that the information presented last was more accessible in memory. However, recall data and other manipulations rendered this interpretation implausible (see Krosnick et al., 1990). Rather, the emergence of this recency effect could be traced to the operation of conversational conventions. As Krosnick et al. (1990, p. 1141) suggested, subjects who first receive base-rate information and are subsequently provided with individuating information may reason, "The first piece of information I was given (i. e., the base-rate) has clear implications for my judgment, so it was sufficient. A speaker should only give me additional information if it is highly relevant and informative, so the experimenter must believe that the individuating information should be given special weight in my judgment." This reasoning would not only imply the general guarantee of relevance addressed above, but would also reflect the conversational convention to present the more informative and important information later in an utterance, in part to direct listeners' attention to it (see Clark, 1985, pp. 222-224). In turn, listeners may assume that "information presented later is more important and should be the focus of their attention" (Krosnick et al., 1990, p. 1141). If so, the individuating information may be rendered particularly relevant in the conversational context if presented after rather than before the base-rate information.

Several findings support this assumption. In one of their studies, Krosnick et al. (1990, Experiment 4) observed that base-rate information had a more pronounced impact when the base-rate was presented after rather than before the individuating information. However, this recency effect was largely eliminated when subjects were informed that the order in which both pieces of information were presented was determined at random. Thus, subjects were likely to rely on the information presented last, unless the informational value of presentation order was called into question.

Moreover, this order effect only emerged when the base-rate information contradicted the implications of the individuating information. In this case, subjects gave more weight to whatever information was presented last, suggesting that the presentation order may carry information about the relative importance that the communicator wants to convey.

To provide direct evidence for this assumption, Krosnick et al. (1990, Experiment 7) asked subjects to complete the blanks in a transcript of a conversation concerned with buying a car. In the base-rate last condition, the crucial part of this transcript read,

> "My brother-in-law has had one problem after another with his Saab. _____ a car magazine survey found Saabs have a better repair record than Volvos. Considering all this, I decided to buy a _____. I think that is the better choice."

In contrast, in the base-rate first condition the transcript read,

> "A car magazine survey found Saabs have a better repair record than Volvos. _____ my brother-in-law has had one problem after another with his Saab. Considering all this, I decided to buy a _____. I think that is the better choice."

As expected on the basis of conversational conventions, most subjects completed the blanks in a way that implied that the speaker considered the second piece of information as more relevant than the first piece. Moreover, most of these subjects assumed that the speaker decided to buy the car favored by the second piece of information.

In combination, the findings of the Schwarz et al. (1991) and Krosnick et al. (1990) studies indicate that subjects based their judgment primarily on the information that corresponded to the inferred communicative intention of the communicator. In the Schwarz et al. (1991) study, subjects were more likely to rely on the individuating information if it was presented by a human communicator, who they could expect to comply with conversational norms, rather than drawn by a computer. Similarly, in the Krosnick et al. (1990) studies, subjects gave differential weight to base-rate and to individuating information depending on its apparent importance to the communicator, as conveyed by the presentation order chosen. In both cases, it was not the nature of the presented information per se that determined its impact, but rather its perceived relevance in a given conversational context. The same theme is echoed in research on another well-known judgmental bias, namely the fundamental attribution error.

2. The Fundamental Attribution Error

Numerous studies in the domain of person perception have documented a pronounced readiness to account for an actor's behavior in terms of his or her dispositions, even under conditions

where the actor has responded to obvious situational pressures (see Jones, 1990; Ross & Nisbett, 1991, for reviews). Following a classic study by Jones and Harris (1967), this so called "correspondence bias" (Jones, 1990) or "fundamental attribution error" (Ross, 1977) is typically investigated in an attitude-attribution paradigm. In most studies, subjects are provided an essay that advocates a certain position and are asked to infer the author's attitude. Depending on experimental condition, they are either informed that the position taken in the essay was freely chosen by the author or was assigned by the experimenter. Whereas the content of the essay is presumably diagnostic for the author's attitude under free choice conditions, it is not under assignment conditions. Nevertheless, subjects typically attribute attitudes to the author that reflect the position taken in the essay, even under conditions where this position was assigned.

Whereas findings of this type are usually interpreted as evidence for a pervasive "dispositional bias", subjects seem quite aware that the essay is of little diagnostic value under no-choice conditions. For example, Miller, Schmidt, Meyer, and Colella (1984) observed that a majority of their subjects explicitly reported that the essay written under no-choice conditions did not provide useful information about the author's true attitude. Nevertheless, the same subjects proceeded to make attitude attributions in line with the assigned position advocated in the essay. As Wright and Wells (1988) suggested, a violation of conversational norms on the side of the experimenter seems to contribute to this finding. Specifically, Wright and Wells (1988, p. 184) noted that "the direction and content of the essay in the critical no-choice condition are irrelevant to the correct solution of the attribution task because the external constraints are sufficient to account for the essayist's behavior. " The experimenter nevertheless provides subjects with an essay, thus violating the maxim of relevance. However, subjects have no reason to expect that this maxim is violated and are thus likely to assume "that the experimenter believes that the essay has some diagnostic value (otherwise, why were they given the essay?)" (Wright & Wells, 1988, p. 184). Accordingly, they take the essay into consideration in making attitude attributions, resulting in an apparent dispositional bias.

To test this conversational account, Wright and Wells conducted an attitude-attribution study in which subjects were exposed to a pro or a con essay, allegedly written under choice or no-choice conditions. However, in addition to the standard procedure, their study involved a condition designed to undermine the implicit guarantee of relevance. Subjects in this condition were told that "the information packages and questionnaire items being given to subjects (...) were being randomly selected from a larger pool of information packages and questions. Thus, their pool might not include sufficient information for them to answer some of their questions. Moreover, their information package might contain some information that was not germane to some of their questions" (Wright & Wells, 1988, p. 185). As expected, this manipulation significantly reduced the emerging

dispositional bias relative to the standard conditions in which subjects could assume that all the information provided to them is relevant to the task at hand. Moreover, the impact of undermining the guarantee of relevance was limited to the theoretically relevant no-choice conditions, and the above manipulation did not affect subjects' inferences from essays written under free choice. Hence, undermining the guarantee of relevance did not result in generally more cautious judgments. Rather, it set subjects free to rely on the information that they themselves considered diagnostic, without having to find "relevance" in the information provided by the experimenter.

Similar processes are likely to contribute to some extent to findings that suggest that the impact of highly diagnostic information is "diluted" by the addition of less diagnostic information (e. g., Nisbett, Zukier, & Lemley, 1981; see Tetlock & Boettger, 1989). To the extent that information presented by the experimenter comes with a guarantee of "relevance", subjects are likely to consider it in forming a judgment -- not because they would find the information per se utterly informative, but because the sheer fact that it was presented to them indicates that it is somehow "relevant".

3. Conclusions

As the reviewed studies indicate, some of the biases that have received considerable attention in social cognition research may be less likely to reflect genuine shortcomings of the judgmental process than has typically been assumed. In fact, subjects often seem quite aware that the normatively irrelevant information is of little informational value. Nevertheless, they typically proceed to use it in making a judgment. As the above studies suggest, however, this may often reflect a violation of conversational norms by the experimenter, rather than any inherently flawed reasoning by subjects. Subjects have no reason to assume that the experimenter would intentionally provide information that is uninformative and irrelevant to the task at hand, thus violating the tacit rules that govern the conduct of conversation in everyday life. Accordingly, they proceed on the basis of the assumption that the experimenter is a cooperative communicator and work hard at making sense of the information provided to them. Once the implicit guarantee of relevance is called into question, however, the impact of normatively irrelevant information is largely reduced.

This analysis suggests that the typical procedures used in social cognition research are likely to result in an overestimation of the size and the pervasiveness of judgmental biases. This analysis does not imply, however, that violations of conversational norms are the sole source of judgmental biases. Like most robust phenomena, judgmental biases are likely to have many determinants (see Ross & Nisbett, 1992). If we are to understand their operation in natural contexts, however, we need to ensure that their emergence in laboratory experiments does not reflect the operation of determinants that are unlikely to hold in other settings.

B. Making Sense of Ambiguous Questions

Whereas the preceding research examples pertained to the impact of explicit assertions of little informational value, subjects' judgments have been found to be equally biased by the presuppositions conveyed by a researcher's question. This misleading impact of questions has received considerable attention in research on public opinion measurement and eyewitness testimony. As the studies reviewed in this section illustrate, however, the biasing effects of questions are again mediated by researchers' violations of conversational norms and respondents' erroneous assumption that the questioner is a cooperative communicator.

1. Answering Questions About Fictitious Issues

Public opinion researchers have long been concerned that the "fear of appearing uninformed" may induce "many respondents to conjure up opinions even when they had not given the particular issue any thought prior to the interview" (Erikson, Luttberg, & Tedin, 1988, p. 44). To explore how meaningful respondents' answers are, survey researchers introduced questions about highly obscure or even completely fictitious issues, such as the "Agricultural Trade Act of 1978" (e.g., Bishop, Tuchfarber, & Oldendick, 1986; Schuman & Presser, 1981). Presumably, respondents' willingness to report an opinion on a fictitious issue casts some doubt on the reports provided in survey interviews in general. In fact, about 30% to 50% of the respondents do typically provide an answer to issues that are invented by the researcher. This has been interpreted as evidence for the operation of social pressure that induces respondents to give answers, which are presumably based on a "mental flip of coin" (Converse, 1964, 1970). Rather than providing a meaningful opinion, respondents are assumed to generate some random response, apparently confirming social scientists' wildest nightmares.

From a conversational point of view, however, these responses may be more meaningful than has typically been assumed in public opinion research. From this point of view, the sheer fact that a question about some issue is asked presupposes that this issue exists – or else asking a question about it would violate the norms of cooperative conduct. Respondents, however, have no reason to assume that the researcher would ask meaningless questions and will hence try to make sense of it (see Strack & Martin, 1987, for a general discussion of respondents' tasks). If the question is highly ambiguous, and the interviewer does not provide additional clarification, respondents are likely to turn to the context of the ambiguous question to determine its meaning, much as they would be expected to do in any other conversation. Once respondents have assigned a particular meaning to the issue, thus transforming the fictitious issue into a better defined issue that makes sense in the context of the interview, they may have no difficulty in reporting a subjectively meaningful opinion. Even if they

have not given the particular issue much thought, they may easily identify the broader set of issues to which this particular one apparently belongs. If so, they can use their general attitude toward the broader set of issues to determine their attitude toward this particular one.

A study by Strack, Schwarz, and Wänke (1991, Experiment 1) illustrates this point. In this study, German college students were asked to report their attitude toward an "educational contribution". For some subjects, this target question was preceded by a question that asked them to estimate the average tuition fees that students have to pay at U.S. universities (in contrast to Germany, where university education is free). Others had to estimate the amount of money that the Swedish government pays every student as financial support. As expected, students' attitude toward an "educational contribution" was more favorable when the preceding question referred to money that students receive from the government than when it referred to tuition fees. Subsequently, respondents were asked what the "educational contribution" implied. Content analyses of respondents' definitions of the fictitious issue clearly demonstrated that respondents used the context of the "educational contribution" question to determine its meaning.

Thus, respondents turned to the content of related questions to determine the meaning of an ambiguous one. In doing so, they interpreted the ambiguous question in a way that made sense of it, and subsequently provided a subjectively meaningful response to their definition of the question. Accordingly, it comes as no surprise that responses to fictitious issues do not conform to a model of mental coin flipping as Converse and other early researchers hypothesized, but do show a meaningful and systematic pattern, as Schuman and Kalton (1985) observed. What is at the heart of reported opinions about fictitious issues is not that respondents are willing to give subjectively meaningless answers, but that researchers violate conversational rules by asking meaningless questions in a context that suggests otherwise. And much as has been observed in response to useless information presented in psychological experiments, survey respondents work hard at finding meaning in the questions asked.

2. Leading Questions

In a highly influential program of research, Loftus and collaborators (e.g., Loftus, 1975; see Loftus, 1979, for a review) demonstrated a pronounced impact of the presuppositions conveyed by leading questions on subjects' memory. For example, subjects were shown a brief film clip and subsequently had to answer questions about what they saw. For some subjects, the questions included, "Did you see the children getting on the school bus?", although no school bus was shown in the film. One week later, these subjects were more likely to erroneously remember having seen the school bus presupposed in the leading question than subjects who were not exposed to the question (Loftus, 1975,

Experiment 4). Findings of this type have typically been interpreted as indicating that "a presupposition of unknown truthfulness will likely be treated as fact, incorporated into memory, and subsequently 'known' to be true" (Dodd & Bradshaw, 1980, p. 695).

Not surprisingly, such biasing effects of leading questions received considerable attention in applied research into eyewitness testimony. Several studies suggest, however, that the applied implications of this line of work may be more limited than has been assumed. In most experiments, the leading question is asked by the experimenter and subjects have no reason to assume that the experimenter may lead them astray by knowingly introducing unwarranted presuppositions, thus violating conversational norms. In an actual courtroom setting, on the other hand, people may be quite aware that communicators may follow their own agenda, may be motivated to introduce misleading information, and may be all but cooperative. Hence, the impact of leading questions may be restricted to conditions under which the questioner is assumed to be a cooperative communicator.

In line with this assumption, Dodd and Bradshaw (1980) observed biasing effects of leading questions about an observed car accident when the source of the question was the researcher, but not when the source was said to be the defendant's lawyer (Experiment 1) or the driver of the car who caused the accident (Experiment 2). Thus, the otherwise obtained biasing effects of leading questions were "canceled by attributing the verbal material to a source that may be presumed to be biased" (Dodd & Bradshaw, 1980, p. 701), calling the source's cooperativeness into question. Similarly, Smith and Ellsworth (1987) only obtained a biasing effect of leading questions when the questioner was assumed to be familiar with the event that the subject had witnessed. When the questioner was assumed to be unfamiliar with the event, the presupposition was discounted and no impact of the leading question was obtained.

Whereas Loftus's research program focused mainly on the impact of leading questions on reconstructive memory, other researchers explored the impact of leading questions on impression formation. Their findings reiterate the same theme. For example, in an exploration of incrimination through innuendo, Wegner, Wenzlaff, Kerker, and Beattie (1981) observed that media questions of the type, "Is Jane using drugs?", may quickly become public answers. Again, recipients infer that there must be some evidence that triggered the question in the first place -- or why else would someone raise it? Here, as well as in Loftus's research program, the impact of the presupposition conveyed by the question rests on the implicit assumption that the communicator is cooperative, as a study by Swann, Giuliano, and Wegner (1982) illustrates. In their study, subjects observed how a questioner asked a respondent a leading question of the type, "What would you do to liven things up at a party?" As expected, subjects considered the question to provide conjectural evidence that the person asked is an extrovert -- unless they were told that the questions had been drawn from a

fishbowl, thus undermining the implicit guarantee of relevance[2].

In combination, these studies indicate that theoretical accounts of the processes underlying the impact of leading questions have to take the assumed cooperativeness of the questioner into account. By themselves, the implications conveyed by the leading question are not sufficient to affect subjects' judgments or recollections. Rather, subjects only rely on the conveyed presuppositions if they can assume that the speaker has access to the relevant knowledge and is a cooperative communicator who complies with the Gricean maxims. Only under those conditions can they expect the communicator to provide information that is informative, truthful, relevant, and clear. From this perspective, the robustness of leading question effects under laboratory conditions is not surprising: As the preceding sections of this chapter illustrated, subjects typically assume that the experimenter is a cooperative communicator and are hence likely to rely on the implications conveyed by the experimenter's questions. Moreover, the experimenter is presumably a particularly knowledgeable source -- after all, who would be more likely to know what was presented in the stimulus materials? By the same token, however, leading questions may provide less of a problem in natural settings, in which "there is often a basis to believe that the interrogator does not know the facts and is likely to have reasons to mislead" (Dodd & Bradshaw, 1980, p. 696).

C. Formal Features of Questionnaires

Whereas the preceding examples pertained to explicitly presented verbal material, research participants do not only apply the tacit assumptions of daily conversations to the verbal content of questions. Rather, they also assume that any other aspect of the question asking process is "relevant" to the task at hand, including formal features of the questionnaire. As a result, formal features of questionnaire construction, such as the specific numeric values presented as part of a rating scale or the range of response alternatives presented as part of a behavioral frequency question, may strongly influence the obtained responses, as the following examples illustrate (see Schwarz & Hippler, 1991, for a more extended review).

1. The Numeric Values of Rating Scales

According to measurement theory, a 7-point rating scale is a 7-point rating scale, independent of how the seven points are graphically represented in the layout of the questionnaire. What

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[2] Similar considerations apply to the inferences drawn from explicit assertions or denials; see Gruenfeld & Wyer, 1992; Harris & Monaco, 1978; Wegner et al., 1981.

psychologists care about is the wording of the question and the nature of the labels used to anchor the endpoints of the scale (see Dawes & Smith, 1985, for a review), but not the lay-out in which the scale is presented. For example, a 7-point scale that ranges from 1 to 7 should result in the same data pattern as a 7-point scale that ranges from -3 to +3, or a scale that presents seven unnumbered boxes, as long as the same verbal endpoint labels are used.

Empirically, however, the specific numerical values used may strongly affect the obtained responses, as Schwarz, Knäuper, Hippler, Noelle-Neumann, and Clark (1991, Experiment 1) observed. As part of a larger survey, a representative sample of German adults was asked, "How successful would you say you have been in life?". This question was accompanied by an 11-point rating scale, with the endpoints labeled "not at all successful" and "extremely successful". In one condition the numeric values of the rating scale ranged from 0 ("not at all successful") to 10 ("extremely successful"), whereas in the other condition they ranged from -5 ("not at al successful") to +5 ("extremely successful"). The results showed a dramatic impact of the numeric values used, as shown in Table 2.

Table 2: The Impact of Numeric Scale Values on Reports Along Rating Scales

	0 to 10 Scale		-5 to +5 Scale		
Scale Value	Percentage	Cumulative	Scale Value	Percentage	Cumulative
0	-	-	-5	1	1
1	-	-	-4	-	1
2	2	2	-3	1	2
3	5	7	-2	1	3
4	7	14	-1	1	4
5	20	34	0	9	13
6	14	48	+1	9	22
7	20	68	+2	23	45
8	20	88	+3	35	80
9	6	94	+4	14	94
10	3	97	+5	4	98
Undecided	3	100	Undecided	2	100
N	480		N	552	

Note. Percentages rounded; $Chi^2(10) = 105.1$, $p < .0001$. Data based on a quota sample of 1032 German adults, randomly assigned to conditions (Source: IfD 5007, Juli 1988). Adapted from Schwarz, N., Knäuper, B., Hippler, H. J., Noelle-Neumann, E., & Clark, F. (1991). Rating scales: Numeric values may change the meaning of scale labels. Public Opinion Quarterly, 55, 570-582. Reprinted by permission.

Whereas 34 percent of the respondents endorsed a value between 0 and 5 on the 0 to 10 scale, only 13 percent endorsed one of the formally equivalent values between -5 and 0 on the -5 to +5 scale. Coding both scales from 0 to 10, this pattern resulted in mean ratings of $\underline{M}$ = 6.4 on the 0 to 10, but $\underline{M}$ = 7.3 on the -5 to +5 version of the scale. In addition, an inspection of the distributions along both scales indicated that the responses were dislocated towards the high end of the -5 to +5 scale, as compared to the 0 to 10 scale. This is also reflected in markedly different standard deviations, $\underline{sd's}$ = 1.03 and .56 for the 0 to 10 and -5 to +5 scale, respectively.

Subsequent experiments (Schwarz et al., 1991) indicated that the impact of numeric values is mediated by differential interpretations of the ambiguous endpoint label "not at all successful". When this label is combined with the numeric value "0", respondents interpret it to refer to the absence of noteworthy success. However, when the same label is combined with the numeric value "-5", they interpret it to refer to the presence of explicit failure. This differential interpretation reflects that a minus-to-plus format emphasizes the bipolar nature of the dimension that the researcher has in mind, implying that one endpoint label refers to the opposite of the other. Hence, "not at all successful" is interpreted as reflecting the opposite of success, that is, failure. In contrast, a rating scale format that presents only positive values suggests that the researcher has a unipolar dimension in mind. In that case, the scale values reflect different degrees of the presence of the crucial feature. Hence, "not at all successful" is now interpreted as reflecting the mere absence of noteworthy success, rather than the presence of failure. This differential interpretation of the same term as a function of its accompanying numeric value also affects the inferences that judges draw on the basis of a report given along a rating scale. For example, in a follow-up experiment (Schwarz et al., 1991, Experiment 3), a fictitious student reported his academic success along one of the described scales, checking either a "-4" or a formally equivalent "2". As expected, judges who were asked to estimate how often this student had failed an exam assumed that he failed twice as often when he checked a "-4" than when he checked a "2", although both values are formally equivalent along the rating scales used.

In combination, these findings illustrate that "even the most unambiguous words show a range of meaning, or a degree of 'semantic flexibility', (...) that is constrained by the particular context in which these words occur" (Woll, Weeks, Fraps, Pendergrass, & Vanderplas, 1980, p. 60). Assuming that all contributions to an ongoing conversation are relevant, respondents turn to the context of a word to disambiguate its meaning, much as they would be expected to do in daily life. In a research situation, however, the contributions of the researcher include apparently formal features of questionnaire design, rendering them an important source of information of which respondents make systematic use (see Bless, Strack, & Schwarz, in press; Schwarz & Hippler, 1991, for more detailed discussions). Far from demonstrating superficial and meaningless responding, findings of this type

indicate that respondents systematically exploit the information available to them in an attempt to understand their task and to provide a meaningful answer.

At the same time, these findings emphasize that researchers must be sensitive to the informational implications of their research instrument to use them to their advantage. For example, the present findings suggest that rating scales that provide a continuum from negative to positive values may indicate that the researcher has a bipolar conceptualization of the respective dimension in mind, whereas scales that present only positive values may indicate a unipolar conceptualization. If so, the choice of numeric values may either facilitate or dilute the polarity implications of the endpoint labels that are provided to respondents. Accordingly, researchers may be well advised to match the numeric values that they provide to respondents with the intended conceptualization of the underlying dimension as uni- or bipolar.

2. Frequency Scales

A related line of research explored the impact of response alternatives on behavioral frequency reports (see Schwarz, 1990; Schwarz & Hippler, 1987, for reviews). In survey research, respondents are typically asked to report the frequency with which they engage in a behavior by checking the appropriate value from a set of frequency response alternatives provided to them. Again, the range of response alternatives may serve as a source of information for respondents. Specifically, respondents assume that the researcher constructed a meaningful scale that reflects his or her knowledge about the distribution of the behavior. Accordingly, values in the middle range of the scale are assumed to reflect the "average" or "typical" behavior, whereas the extremes of the scale are assumed to correspond to the extremes of the distribution. These assumptions influence respondents' interpretation of the question, their behavioral reports, and related judgments.

Question interpretation. Suppose, for example, that respondents are asked to indicate how frequently they were "really irritated" recently. Before the respondent can give an answer, he or she must decide what the researcher means by "really irritated". Does this refer to major irritations such as fights with one's spouse or does it refer to minor irritations such as having to wait for service in a restaurant? If the respondent has no opportunity to ask the interviewer for clarification, or if a well-trained interviewer responds, "Whatever you feel is really irritating", he or she might pick up some pertinent information from the questionnaire. One such piece of information may be the frequency range provided by the scale.

For example, respondents who are asked to report how often they are irritated on a scale ranging from "several times daily" to "less than once a week" may relate this frequency range to their general knowledge about the frequency of minor and major annoyances. Assuming that major

annoyances are unlikely to occur "several times a day", they may consider instances of less severe irritation to be the target of the question than respondents who are presented a scale ranging form "several times a year" to "less than once every three months". Experimental data support this assumption (Schwarz, Strack, Müller, & Chassein, 1988). Respondents who reported their experiences on the former scale, subsequently reported less extreme examples of annoying experiences than respondents who were given the latter scale. Thus, the type of annoying experiences that respondents reported was determined by the frequency range of the response alternatives in combination with respondents' general knowledge, rather than by the wording of the question per se. Accordingly, the same question combined with different frequency scales is likely to assess different experiences.

Theoretically, the impact of the response alternatives on respondents' interpretation of the question should be the more pronounced the less clearly the target behavior is defined. For this reason, questions about subjective experiences may be particularly sensitive to the impact of response alternatives because researchers usually refrain from providing a detailed definition of the target experience so as not to interfere with its subjective nature. Ironically, assessing the frequency of a behavior with precoded response alternatives may result in doing just what is avoided in the wording of the question.

Frequency estimates. Even if the behavior under investigation is reasonably well defined, however, the range of response alternatives may strongly affect respondents' frequency estimates. This reflects that mundane behaviors of a high frequency, such as watching TV for example, are not represented in memory as distinct episodes (see Bradburn, Rips, & Shevell, 1987; Schwarz, 1990, for reviews). Rather, the various episodes blend together in a generic representation of the behavior that lacks temporal markers. Accordingly, respondents cannot recall the episodes to determine the frequency of the behavior but have to rely on an estimation strategy (see Menon, in press, for a more detailed discussion). In doing so, they may use the range of the scale presented to them as a frame of reference. This results in higher frequency estimates along scales that present high rather than low frequency response alternatives.

The results of a study on TV consumption, shown in Table 3, illustrate this effect (Schwarz, Hippler, Deutsch, & Strack, 1985, Experiment 1). In this study, 37.5 percent of a quota sample of German adults reported watching TV for 2.5h or more a day, when presented the high frequency response alternatives shown in Table 3, whereas only 16.2 percent reported doing so when presented the low frequency response alternatives.

Table 3: Reported Daily TV Consumption as a Function of Response Alternatives

Reported Daily TV Consumption			
Low Frequency Alternatives		High Frequency Alternatives	
Up to 1/2 h	7.4%	Up to 2 1/2h	62.5%
1/2 h to 1h	17.7%	2 1/2h to 3h	23.4%
1h to 1 1/2h	26.5%	3h to 3 1/2h	7.8%
1 1/2h to 2h	14.7%	3 1/2h to 4h	4.7%
2h to 2 1/2h	17.7%	4h to 4 1/2h	1.6%
More than 2 1/2h	16.2%	More than 4 1/2h	0.0%

Note. N = 132. Adapted from Schwarz, N., Hippler, H.J., Deutsch, B., & Strack, F. (1985). Response scales: Effects of category range on reported behavior and comparative judgments. Public Opinion Quarterly, 49, 388-395. Reprinted by permission.

Not surprisingly, respondents' reliance on the frame of reference suggested by the response alternatives increases as their knowledge about relevant episodes decreases (Schwarz & Bienias, 1990), or the complexity of the judgmental task increases (Bless, Bohner, Hild, & Schwarz, 1992). More importantly, however, the impact of response alternatives is completely eliminated when the informational value of the response alternatives is called into question. For example, telling respondents that they participate in a pretest designed to explore the adequacy of the response alternatives, or informing student subjects that the scale was taken from a survey of the elderly, wiped out the otherwise obtained impact of response alternatives (Schwarz & Hippler, unpublished data). Again, these findings illustrate that respondents assume the researcher to be a cooperative communicator, whose contributions are relevant to the ongoing conversation, unless the implicit guarantee of relevance is called into question.

Comparative judgments. In addition, the frequency range of the response alternatives has been found to affect subsequent comparative judgments. Given the assumption that the scale reflects the distribution of the behavior, checking a response alternative is the same as locating one's own position in the distribution. Accordingly, respondents extract comparison information from their own location on the response scale and use this information in making subsequent comparative judgments.

For example, checking 2h on the low frequency scale shown in Table 3 implies that a respondent's TV consumption is above average, whereas checking the same value on the high frequency scale implies that his or her TV consumption is below average. As a result, respondents in the Schwarz et al. (1985) studies reported that TV plays a more important role in their leisure time (Experiment 1), and described themselves as less satisfied with the variety of things they do in their leisure time (Experiment 2), when they had to report their TV consumption on the low rather than

on the high frequency scale (see also Schwarz & Scheuring, 1988). Moreover, these frame of reference effects are not limited to respondents themselves, but influence the users of their reports as well. For example, in a study by Schwarz, Bless, Bohner, Harlacher, and Kellenbenz (1991, Experiment 2) experienced medical doctors considered having the same physical symptom twice a week to reflect a more severe medical condition when "twice a week" was a high rather than a low response alternative on the symptoms checklist presented to them.

3. Conclusions

Findings of the type reviewed in this section are usually considered measurement "artifacts". From a conversational point of view, however, they simply reflect that respondents bring the assumptions that govern the conduct of conversations in daily life to the research situation. Hence, they assume that every contribution is relevant to the goal of the ongoing conversation -- and in a research situation, these contributions include apparently formal features of the questionnaire, such as the numeric values presented as part of a rating scale or the response alternatives presented as part of a frequency question. As a result, the scales used are all but "neutral" measurement devices. Rather, they constitute a source of information that respondents actively use in determining their task and in constructing a reasonable answer. While research methodologists have traditionally focused on the information that is provided by the wording of the question, we do need to pay equal attention to the information that is conveyed by apparently formal features of the questionnaire.

IV. Making One's Contribution Informative

So far, the reviewed research illustrated how many apparent biases and shortcomings in human judgment or artifacts in opinion measurement may, in part, be traced to the implicit guarantee of relevance that characterizes human communication. However, the maxims of cooperative communication do not only determine recipients' use of the information provided by speakers. Rather, they also determine what information the recipient of a question is expected to provide in turn. Specifically, cooperative speakers are supposed to provide information that is relevant to the goal of the conversation. This not only implies that the provided information should be substantively related to the topic of the conversation. Rather, it also implies that the provided information should be new to the recipient (Clark & Clark, 1977). Hence, the utterance should not reiterate information that the recipient already has, or may take for granted anyway. Accordingly, determining which information one should provide requires extensive inferences about the information that the recipient already has to identify what is, or is not, "informative".

As was the case for information (inadvertently) provided by the researcher, conversational

rules that govern the selection of information to be provided by research participants underlie many apparently surprising findings in social and psychological research, ranging from the impact of open versus closed question formats to children's performance on Piagetian conservation tasks or the use and disuse of easily accessible information in making a judgment.

A. What Is Informative? The Impact of Open versus Closed Question Formats

To answer a question, speakers have to determine what information they are to provide. Suppose that you are asked to report what you have done today. Most likely, you would not include in your report that you took a shower, that you dressed, and so on. If these activities were included in a list of response alternatives, however, you would probably endorse them. This thought experiment reflects a set of standard findings from the survey methodology literature (see Schwarz & Hippler, 1991, for a review).

Experimental studies on the impact of open- and closed-response formats have consistently demonstrated that open- and closed-response formats yield considerable differences in the marginal distribution as well as the ranking of items (e.g., Bishop, Hippler, Schwarz, & Strack, 1988; Schuman & Presser, 1977). On the one hand, any given opinion is less likely to be volunteered in an open-response format than to be endorsed in a closed-response format, if presented. On the other hand, opinions that are omitted from the set of response alternatives in a closed format are unlikely to be reported at all, even if an "other" category is explicitly offered, which respondents in general rarely use (Bradburn, 1983; Molenaar, 1982). Several processes are likely to contribute to these findings.

Most importantly, respondents are unlikely to spontaneously report, in an open-answer format, information that seems self-evident or irrelevant. In refraining from these responses they follow the conversational maxim that an utterance should be informative, as discussed above. This results in an underreporting of presumably self-evident information that is eliminated by closed-response formats, where the explicit presentation of the proper response alternative indicates the investigator's interest in this information. Moreover, respondents may frequently be uncertain if information that comes to mind does or does not belong to the domain of information the investigator is interested in. Again, closed-response formats may reduce this uncertainly, resulting in higher responses. Finally, a generic "other" response provides little information and would be considered inadequate as an answer in most conversations. Hence, it is rarely checked.

In addition, the response alternatives may remind respondents of options that they may otherwise not have considered. The methodological literature has typically focused on this latter possibility, implying that closed response formats may suggest answers that respondents would never

think of themselves. This assumption is to some degree supported by the observation that less educated respondents are more likely to refuse to answer in an open-response format, but to provide an answer in a closed-response format, than well educated respondents (Schuman & Presser, 1981). However, a conversational analysis suggests that the obtained differences are more plausibly traced to the clarification of the questioner's interest that is provided by a closed-response format. Most importantly, the assumption that respondents may lack the information required for an answer, and hence pick one from the response alternatives, may hold to some degree for complex knowledge questions, but does not hold for questions about daily activities, such as, "What have you done today?" Nevertheless, the same differences are obtained for questions of this type, and they are most pronounced for activities that the questioner may take for granted anyway, such as taking a shower or having breakfast (see Schwarz, Hippler, and Noelle-Neumann, in press, for a more extended discussion).

B. Repeated Questions and Changing Interpretations

That speakers are supposed to provide new information rather than to reiterate information the recipient already has, has important implications for the interpretation of questions that are highly similar in content or even repeated literally. Unless there is reason to believe that the questioner did not understand the answer already given, the person asked is likely to interpret the second question as a request for new information. The studies reviewed in this and the following section illustrate these shifting interpretations and their impact on respondents' reports.

1. Experimenters' Questions and Children's Cognitive Skills: The Piagetian Conservation Task

Much as researchers' violations of conversational norms have contributed to overestimations of adults' cognitive biases, they have also contributed to underestimations of children's cognitive skills. Research on the conservation task introduced by Piaget (1952; Piaget & Inhelder, 1969) may serve as an example. In a typical study (e. g., McGarrigle & Donaldson, 1974), a child is shown two rows of objects, equal in number and aligned in one-to-one correspondence. When asked, "Is there more here or more here, or are both the same number?", the child usually answers that both rows are the same in number. Next, the experimenter rearranges the objects in one of the rows to extend the row's length. Following this transformation, the previously asked question is repeated. Many young children now respond that there are more objects in the longer row, suggesting that they did not master number conservation.

Given that only the perceptual configuration of the crucial row has changed, explanations of this phenomenon have typically focussed on children's susceptibility to perceptual influences.

However, a conversational analysis of the procedures used proves informative. Why would a speaker ask the same question twice within a very short time span, unless he or she inquired about some new aspect? And what would that new aspect most likely be, following the deliberate and intentional transformation performed by the questioner? As McGarrigle and Donaldson (1974, p. 347) noted, in early stages of language acquisition, children use the behavior of a speaker "to arrive at a notion of speaker's meaning and this knowledge is utilized to make sense of the language around" them, much as adults use the context of an utterance to disambiguate its meaning. From this perspective, "it could be that the experimenter's simple direct action of changing the length of the row leads the child to infer an intention on the experimenter's part to talk about what he has just been doing. It is as if the experimenter refers behaviorally to length although he continues to talk about number" (McGarrigle & Donaldson, 1974, p. 343).

To test this assumption, the authors conducted a study in which they varied the apparent intentionality of the transformation. Whereas they replicated the above standard procedure in one condition, a "naughty teddy bear" appeared in the other and tried to "spoil the game" by rearranging the objects, increasing the length of one row. The results provided strong support for a conversational account: Whereas only 13 out of 80 children showed number conservation when the experimenter manipulated the length of the row, 50 out of the same 80 children showed number conservation when the change was due to the apparently unintended interference of "naughty teddy" (see Dockrell, Neilson, & Campbell, 1980; Light, Buckingham, & Robbins, 1979, for conceptual replications and Donaldson, 1982, for a review). These findings suggest that the children used the behavioral context of the question to infer the speaker's meaning: When "naughty teddy" changed the arrangement of the objects, the experimenter may indeed want to know if teddy took an object or if the number remained the same. But repeating the previously answered number question makes little sense when the experimenter changed the arrangement himself, leading children to infer that the experimenter apparently wants to talk about what he did, thus changing the reference of the question from number to length.

That the changes in children's interpretation of the question are indeed driven by conversationally inappropriate question reiteration has been nicely demonstrated by Rose and Blank (1974). In their study, children were again shown two rows of equal length with the same number of objects, but only some of the children had to make an initial judgment at this point. Subsequently, the experimenter changed the arrangement, increasing the length of one row. As usual, many of the children who had already given a judgment when the rows were equal now reported that the longer row has more objects. However, children who had not previously been asked were considerably more likely to respond that the number of objects in both rows is the same (see also Siegal, Waters, &

Dunwiddy, 1988). Clearly, it is not children's confusion of number and length per se that leads them astray. Rather, having already answered the question, the children assume that the experimenter must have something else in mind when the question is reiterated. Hence, they respond to their new interpretation of the reiterated question, unless asking for the same information twice makes sense, as was the case in McGarrigle and Donaldson's (1974) study.

2. Are "Happiness" and "Satisfaction" the Same Thing?

Much as children have been found to change their interpretation of the same question if reiterated within a short time span, adults have been observed to change, or not to change, their interpretation of highly similar questions as a function of the conversational context in which the questions are posed. For example, Strack, Schwarz, and Wänke (1991, Experiment 2) asked German students to rate their happiness and satisfaction with life as a whole along 11-point scales (11 = "very happy" or "very satisfied", respectively). In one condition, both questions were presented at the end of the same questionnaire and were introduced by a joint lead-in that read, "Now, we have two questions about your life." In the other condition, only the happiness question was presented at the end of the questionnaire, introduced by a parallel lead-in, "Now, we have a question about your life." The subsequent rating of life-satisfaction, however, was presented as the first question in a new and ostensibly unrelated questionnaire about characteristics of research participants, attributed to a different researcher and presented in a different graphic lay-out.

How would these manipulations affect respondents' interpretation of the two related concepts of "happiness" and "satisfaction"? In general, happiness and satisfaction are perceived as closely related concepts and both judgments have typically been found to be affected by the same variables in studies of subjective well-being (see Schwarz, 1987; Schwarz & Strack, 1991b). However, when both questions are presented as part of the same conversational context, interpreting them as nearly identical in meaning would result in considerable redundancy. Hence, respondents may infer that the researcher intends both questions to tap different aspects of their subjective well-being and may, accordingly, draw on different information about their life in making their judgments. Note, however, that this does not apply when the questions are asked by two different communicators. In this case, both communicators may simply use somewhat different words to refer to the same thing and providing identical responses would not violate the norm of nonredundancy, given that each response is directed to a different recipient. As a result, the answers given to both questions should be more similar when the questions are asked by different researchers rather than by the same researcher.

Strack et al.'s (1991, Experiment 2) findings confirmed this expectation. When both questions were asked in ostensibly unrelated questionnaires, subjects' mean reports of happiness ($\underline{M}$ = 8.0) and

satisfaction ($\underline{M}$ = 8.2) did not differ and both measures correlated $\underline{r}$ = .96. When both questions were presented as part of the same conversational context, however, subjects reported significantly higher happiness ($\underline{M}$ = 8.1) than satisfaction ($\underline{M}$ = 7.4) and the correlation between both measures dropped significantly to $\underline{r}$ = .75. Apparently, respondents inferred from the conversational relatedness of both questions that the researcher must have distinct concepts in mind, as asking the same thing twice would make little sense. Accordingly, they presumably based their responses on different aspects of their life under this condition, a process that is shown more clearly in the studies reviewed in the next section.

C. Avoiding Redundancy in Answering Questions of Differential Generality: Its Impact on the Use of Highly Accessible Information

In many studies, respondents are asked to answer several related questions that may vary in generality. For example, they may be asked how satisfied they are with different specific domains of their life, as well as how satisfied they are with their life as a whole. Current theorizing in social cognition suggests that answering a specific question increases the accessibility of information used to answer it (see Bodenhausen & Wyer, 1987; Higgins, 1989; Martin & Clark, 1990, for reviews). Hence, this information should be most likely to come to mind when a related general question is asked later on. Depending on the nature of the conversational context, however, using the primed information in answering the question may violate the conversational norm of nonredundancy, as Strack and Martin (1987) pointed out, following related suggestions by Bradburn (1982) and Tourangeau (1984). As a result, conversational processes may determine if easily accessible information is, or is not, used in making a judgment, thus determining the emergence of priming effects. As the studies reviewed in this section illustrate, the combined operation of cognitive accessibility and conversational norms may strongly influence the relationship obtained between two judgments, or between a behavioral report and a judgment, leading to dramatically different substantive conclusions in social research.

1. General and Specific Judgments: Conversational Norms and the Emergence of Assimilation and Contrast Effects

To explore the relationship between related judgments of differential generality, Schwarz, Strack, and Mai (1991) asked respondents to report their marital satisfaction as well as their general life-satisfaction, varying the order in which both questions were asked. The first column of Table 4 shows the resulting correlations between marital and life satisfaction. When the life-satisfaction question preceded the marital satisfaction question, both measures were moderately correlated, $\underline{r}$ =

.32. Reversing the question order, however, increased the correlation to $\underline{r} = .67$. This reflects that answering the marital satisfaction question first increased the accessibility of marriage related information in memory. As a result, respondents were more likely to consider marriage related information in evaluating their life as a whole (see Schwarz & Strack, 1991b, for a judgment model of subjective well-being). This interpretation is supported by a highly similar correlation of $\underline{r} = .61$ when the general question explicitly asked respondents to include their marriage in evaluating their overall life-satisfaction.

In a third condition, however, Schwarz et al. deliberately evoked the conversational norm of non-redundancy. To do so, both questions were introduced by a joint lead-in that read, "We now have two questions about your life. The first pertains to your marital satisfaction and the second to your general life-satisfaction." Under this condition, the same question order that resulted in $\underline{r} = .67$ without a joint lead-in, now produced a low and nonsignificant correlation or $\underline{r} = .18$. This suggests that respondents deliberately ignored information that they had already provided in response to a specific question when making a subsequent general judgment, if the specific and the general questions were assigned to the same conversational context, thus evoking the application of conversational norms that prohibit redundancy. In that case, respondents apparently interpreted the general question as if it referred to aspects of their life that they had not yet reported on. In line with this interpretation, a condition in which respondents were explicitly asked how satisfied they are with "other aspects" of their life, "aside from their relationship", yielded a nearly identical correlation of $\underline{r} = .20$.

In addition, respondents who were induced to disregard their marriage in evaluating their life as a whole, either by the conversational context manipulation or by explicit instructions, reported higher mean life-satisfaction when they were unhappily married, and lower mean life-satisfaction when they were happily married, than respondents who were not induced to exclude this information. Thus, contrast effects were obtained when conversational norms elicited the exclusion of the primed information from the representation formed of one's life in general, whereas assimilation effects were obtained when the activated information was included in this representation (see Schwarz & Bless's, 1992, inclusion/exclusion model of assimilation and contrast effects for a more detailed theoretical discussion).

Table 4: Correlation of Relationship Satisfaction and Life-Satisfaction as a Function of Question Order and Conversational Context

Condition	Number of Specific Questions: One	Number of Specific Questions: Three
General-specific	.32*	.32*
Specific-general	.67*	.46*
Specific-general, with joint lead-in	.18	.48*
Specific-general, explicit inclusion	.61*	.53*
Specific-general, explicit exclusion	.20	.11

Note. N = 50 per cell, except for "Specific-general with joint lead-in", N = 56. Correlations marked by an asterisk differ from chance, $p < .05$. Adapted from Schwarz, N., Strack, F., & Mai, H.P. (1991). Assimilation and contrast effects in part-whole question sequences: A conversational logic analysis. Public Opinion Quarterly, 55, 3-23. Reprinted by permission.

In a subsequent study, Schwarz and Hippler (unpublished data) observed that the conversational norm of non-redundancy may not only be evoked by a joint lead-in, but also by the graphic lay-out of a questionnaire. Specifically, the marital satisfaction question and the general question were either presented in separate boxes, with a black frame drawn around each question, or in a joint box, with one frame drawn around both questions. As in the above data, increasing the conversational relatedness of both questions by presenting them in one box significantly reduced the otherwise obtained correlation, again illustrating the conversational relevance of apparently formal features of questionnaires.

Note, however, that the applicability of the norm of non-redundancy may vary as a function of the number of specific questions that precede the more general one. If only one specific question precedes the general one, the repeated use of the information on which the answer to the specific question was based results in redundancy in the response to the general question. Hence, this repeated use of the same information is avoided if both questions are assigned to the same conversational context, as the above data demonstrated. Suppose, however, that several specific questions precede

the general one. For example, respondents may be asked to report on their marital satisfaction, their job satisfaction, and their leisure time satisfaction before a general life-satisfaction question is presented. In that case, they may interpret the general question in two different ways. On the one hand, they may assume that it is a request to consider still other aspects of their life, much as if it were worded, "Aside of what you already told us,..." On the other hand, they may interpret the general question as a request to integrate the previously reported aspects into an overall judgment, much as if it were worded, "Taking these aspects together, how satisfied are you with your life-as-a-whole?". Note that this interpretational ambiguity of the general question does not arise if only one specific question was asked. In that case, an interpretation of the general question in the sense of "taking all aspects together" would make little sense because only one aspect was addressed, thus rendering this interpretation of the general question completely redundant with the specific one. If several specific questions were asked, however, both interpretations of the general question are viable. In this case, the interpretation of the general question as a request for a final integrative summary judgment is legitimate from a conversational point of view. If several specific questions have been asked, an integrative judgment is informative because it does provide "new" information about the relative importance of the respective domains, which are in the focus of the conversation. Moreover, "summing up" at the end of a series of related thoughts is acceptable conversational practice – whereas there is little to sum up if only one thought was offered. Accordingly, respondents may interpret a general question as a request for a summary judgment if it is preceded by several specific ones, even if all questions are explicitly placed into the same conversational context.

To test this theoretical analysis, other respondents of the Schwarz et al. (1991) study were asked three specific questions, pertaining to their leisure time satisfaction, their job satisfaction, and, finally, their marital satisfaction. As shown in the second column of Table 4, the correlation between marital satisfaction and life-satisfaction increased from $\underline{r} = .32$ to $\underline{r} = .46$ when answering the specific questions first brought information about one's marriage to mind. However, this increase was less pronounced than when the marital satisfaction question was the only specific question that preceded the general one ($\underline{r} = .67$), reflecting that the three specific question brought a more varied set of information to mind. More importantly, introducing the three specific and the general question by a joint lead-in, thus assigning them explicitly to the same conversational context, did <u>not</u> reduce the emerging correlation, $\underline{r} = .48$. This indicates that respondents adopted a "Taking-all-aspects-together" interpretation of the general question if it was preceded by three, rather than one, specific questions. This interpretation is further supported by a highly similar correlation of $\underline{r} = .53$ when the general question was reworded to request an integrative judgment, and a highly dissimilar correlation of $\underline{r} = .11$ when the reworded question required the consideration of other aspects of one's life.

In combination, these findings further emphasize that the interpretation of an identically worded question may change as a function of conversational variables, resulting in markedly different responses. Moreover, the emerging differences are not restricted to the means or margins of the response distribution, as social scientists have frequently hoped. Rather, context variables may result in different correlational patterns, thus violating the assumption that context effects would be restricted to differences in the means, whereas the relationship between variables would be "form resistant" (Schuman & Duncan, 1974; Stouffer & DeVinney, 1949).

2. Behavioral Reports and Evaluative Judgments

This conclusion is further supported by a study reported by Strack, Martin, and Schwarz (1988) that indicates that the above effects are not restricted to order effects between similar evaluative judgments. Rather, the same variables may also affect the observed relationship between behavioral reports and subsequent judgments. Specifically, Strack et al. (1988, Experiment 2) asked American college students to report their general life-satisfaction as well as their dating frequency. When the life-satisfaction question preceded the dating frequency question, the correlation was weak, $\underline{r} = -.12$, and not significant, suggesting that dating frequency may contribute little to students' overall well-being. Reversing the question order, however, increased the correlation dramatically to $\underline{r} = .66$. This presumably reflects that the dating frequency question increased the cognitive accessibility of dating related information, which was then used in evaluating one's life as a whole. On the substantive side, this correlation would suggest that dating frequency is a major contributor to life-satisfaction for college students. However, placing both questions in the same conversational context by a joint lead-in again reduced the obtained correlation to non-significance, $\underline{r} = .15$, reflecting that respondents ignored the information they had already provided when the conversational context elicited the norm of nonredundancy.

3. Conclusions

The findings of the Schwarz et al. (1991) and Strack et al. (1988) studies have methodological as well as theoretical implications. On the methodological side, they illustrate that a researcher may draw very different substantive conclusions about the contribution of marital happiness or dating frequency to individuals' overall well-being, depending on the order in which the questions are asked. To account for the impact of question order, however, it is not sufficient to consider purely cognitive variables in isolation. Whereas preceding questions increase the cognitive accessibility of information used to answer them, this increase does not necessarily result in an increased use of the primed information in making subsequent judgments, in contrast to what current theorizing in social cognition

would suggest.

According to current models of information accessibility and use (see Bodenhausen & Wyer, 1987; Higgins & Bargh, 1987; Wyer & Srull, 1986), the use of information is determined by its accessibility in memory and its applicability to the task at hand. In all of this research, applicability has been assumed to be solely determined by the nature of the stimulus materials. As the present studies indicate, however, the conversational context may change the perceived nature of the judgmental task and may lead subjects to deliberately ignore information that is easily accessible and potentially relevant to the judgment at hand. Hence, the emergence of priming effects is not only determined by the nature of the stimulus materials or the literal question asked, but also by the conversational context in which subjects are asked to make their judgment. As Strack et al. (1988) emphasized, a full understanding of the use of highly accessible information therefore requires not only a consideration of its applicability to the task at hand, but also of its appropriateness in the conversational context (see Martin & Achee, 1992; Schwarz & Bless, 1992; Strack, 1992a,b, for more general discussions of information use).

V. Conclusions

Social cognition research has typically ignored that assessing a judgment in a research situation is part of an ongoing conversation between the subject and the researcher. In essence, social cognition researchers have conceptualized the judge as doing his or her cognitive work in social isolation. In many instances, the obtained findings provided a less than flattering portrait of our respondents. Apparently, people are happy to offer meaningless opinions on non-existent issues and are biased by irrelevant material such as the numeric values of a rating scale or the response alternatives of a frequency question. Moreover, they are apparently more than willing to use worthless personality information and to ignore more meaningful base-rates, or to draw strong dispositional inferences despite obvious situational pressures, and so on. As soon as we conceptualize the assessed judgments as part of an ongoing conversation, however, the often dramatic findings seem less surprising. Rather, research participants seem to do what they would rightly be expected to do in any other conversation: They assume that our utterances as researchers are meaningful, that we do not ask questions about things that don't exist, that we do construct meaningful rather than arbitrary scales, and so on. And they try to make sense of our utterances and of our research instruments on the basis of these assumptions.

In many cases, people do not lack the ability to make adequate judgments. Rather, what they lack is the insight that we as researchers do not live up to the standards that we would typically observe in any other conversation that we conduct. They simply give us more credit than we deserve

by assuming that the information we provide is relevant to the task at hand, truthful, informative and clear. Unless we learn to observe these standards in our conduct of research, and in our interpretation of results, we may run the risk of painting a rather inadequate picture of human judgment by severely overestimating the size and pervasiveness of judgmental biases and shortcomings. Certainly, violations of conversational norms are not the only source of judgmental biases. Like most robust phenomena, these biases are likely to be overdetermined, as Ross and Nisbett (1992) emphasized. To fully understand their operation in natural contexts, however, we have to ensure that their operation in our laboratories does not reflect determinants that are unlikely to be similarly powerful outside of our labs.

Obviously, this concern with possible "side-effects" of research procedures is not new. Rather, it has been at the heart of a research tradition concerned with the social psychology of the psychological experiment (see Kruglanski, 1975, for an overview). However, following Orne's (1962, 1969) seminal discussion of demand characteristics, this research has been guided by the assumption that subjects are motivated to look for cues in the experimental situation that provide them with the experimenter's hypothesis. Depending on their motivation to play the role of a "good subject", they may then react in line with the suspected hypothesis. Accordingly, most of that early research focused on subjects' motivation to detect and to act according to the experimenter's hypothesis, rather than on the process by which subjects extract information from the research procedures used. In contrast, the present analysis suggests that we do not need to make special assumptions about motivations that may be germane to the participation in an experiment to account for the reviewed findings. Rather, the present analysis indicates that subjects' behavior in an experiment or research interview is guided by the same assumptions and motivations that govern the conduct of conversation in any other setting (see Bless, Strack, & Schwarz, in press, for a detailed comparison of Orne's analysis and a conversational perspective). From a conversational point of view, the key difference between experiments and conversations in natural settings is only that the experimenter is less likely to comply to conversational rules in conducting an experiment than in conducting any other conversation, while subjects have no reason to suspect that the experimenter is not a cooperative communicator. As a result, they apply the tacit assumptions that usually govern the conduct of conversation to the research setting and go beyond the literal information provided to them by drawing inferences on the basis of the conversational context.

The apparent biases and errors that subjects commit by relying on conversational maxims are less likely to result in mistakes in everyday contexts where communicators try conform to conversational norms, provide information that is relevant to the judgment at hand, and make the task one that is clear rather than ambiguous -- and where recipients are indeed expected to use contextual

cues to disambiguate the communication, should the communicator not live up to the ideal. Thus, the behavior that may lead to errors in the experimental context may be adaptive in everyday settings. As Funder (1987, p. 82) noted in a related context, "it seems ironic that going beyond the information given in this way is so often interpreted by social psychologists as symptomatic of flawed judgment. Current thinking in the field of artificial intelligence is that this propensity is exactly what makes people smarter than computers". To acknowledge this special potential of human information processors, social cognition research will need to pay closer attention to the social context in which much of our cognitive work is conducted. To do so, social cognition research will eventually need to extend the "flowchart model of information processing that presents us only with a unilateral input/output paradigm that stops short of reciprocity" (Markus & Zajonc, 1985, p. 212). Taking the tacit assumptions that govern the conduct of conversation into account is likely to provide a good starting point for this endeavor.

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