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The Bystander-Effect: A Meta-Analytic Review on Bystander Intervention in Dangerous and Non-Dangerous Emergencies

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Research on bystander intervention has produced a great number of studies showing that the presence of other people in a critical situation reduces the likelihood that an individual will help. As the last systematic review of bystander research was published in 1981 and was not a quantitative meta-analysis in the modern sense, the present meta-analysis updates the knowledge about the bystander effect and its potential moderators. The present work (a) integrates the bystander literature from the 1960s to 2010, (b) provides statistical tests of potential moderators, and (c) presents new theoretical and empirical perspectives on the novel finding of non-negative bystander effects in certain dangerous emergencies as well as situations where bystanders are a source of physical support for the potentially intervening individual. In a fixed effects model, data from over 7,700 participants and 105 independent effect sizes revealed an overall effect size of $g = -0.35$. The bystander effect was attenuated when situations were perceived as dangerous (compared with non-dangerous), perpetrators were present (compared with non-present), and the costs of intervention were physical (compared with non-physical). This pattern of findings is consistent with the arousal-cost-reward model, which proposes that dangerous emergencies are recognized faster and more clearly as real emergencies, thereby inducing higher levels of arousal and hence more helping. We also identified situations where bystanders provide welcome physical support for the potentially intervening individual and thus reduce the bystander effect, such as when the bystanders were exclusively male, when they were naive rather than passive confederates or only virtually present persons, and when the bystanders were not strangers.

Keywords: bystander effect, bystander intervention, dangerous emergencies, helping, meta-analysis

> On the 12th September, 2009, Dominik Brunner was murdered at a German train station after he helped little children against two perpetrators. He has not chosen to look the other way, but sacrificed himself when others were in need.
>
> —Dominik Brunner Foundation

> It is a denial of justice not to stretch out a helping hand to the fallen; that is the common right of humanity.
>
> —Seneca (5 BC–65 AD)

The bystander effect refers to the phenomenon that an individual's likelihood of helping decreases when passive bystanders are present in a critical situation (Darley & Latané, 1968; Latané & Darley, 1968, 1970; Latané & Nida, 1981). Many sad real-life examples illustrate this effect: In 1964, Kitty Genovese was raped and murdered in Queens, New York, while several of her neighbors looked on. No one intervened until it was too late.[1] More recently, in 2009, Dominik Brunner was murdered at a German train station by two 18-year-olds after he tried to help children who were attacked by these young criminals. Several passersby witnessed the murder, but nobody physically intervened. In support of this anecdotal evidence, an influential research program conducted

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[1] The precise number of bystanders, what they saw, and how they interpreted the situation are still under dispute (Manning, Levine, & Collins, 2007).

by Bibb Latané and John Darley provided strong empirical evidence for the existence of the bystander effect in a variety of experimental settings (see Latané & Nida, 1981, for a review). In both a theoretical and a practical sense, the bystander effect has played an increasingly important role in our understanding of helping behavior. References to the effect can be found in nearly every introductory (social) psychology textbook. Various television shows continuously report and try to replicate the effect, and knowledge of the effect is now firmly anchored in public awareness.

Although the evidence for the inhibitory bystander effect is striking, there are also counter-examples. Sometimes, the presence of bystanders can facilitate acts of moral courage. In Munich in 2001, for example, a young man from Turkey helped a young Greek who was chased and beaten by a group of skinheads. The young Turk risked his life while many other bystanders were watching. Similar results were found in laboratory experiments, where the bystander effect vanished when the emergency was a particularly dangerous one (e.g., Fischer, Greitemeyer, Pollozek, & Frey, 2006). Given the variation in the size and the direction of the bystander effect, we asked on a theoretical level and an empirical level whether there are specific situations that might reduce or even revert the traditional inhibitory effect of bystanders on helping. To answer this question, we conducted a meta-analysis across both the classic and recent studies.

There are three pertinent reasons why research on the bystander effect should be submitted to a meta-analytic integration. First, the last systematic review (Latané & Nida, 1981) is now dated, and it did not meet current standards for meta-analysis. Second, potential moderator variables have not been considered beyond qualitative description, and the role of some of these moderators (e.g., the ambiguity of the emergency) is unsettled. Third, and perhaps most importantly, reversals of the traditional, inhibitory bystander effect have recently been reported, especially for dangerous emergencies.

For these reasons, we conducted a full-scale meta-analysis. Doing so, we aimed to address the following theoretically motivated questions: (a) Is the bystander effect reduced or reversed in situations of dangerous emergencies? (b) Are there specific situations where bystanders can increase helping because they are seen as welcome physical support in dangerous emergencies? (c) Are there other theoretically and practically important moderators of the bystander effect? (d) Does the recent wave of bystander studies (i.e., post-1981) offer new insights into the magnitude of the effect (e.g., has the effect increased or declined over time)?

Review of Bystander Research

Definitions and Psychological Accounts

Early research consistently showed that the presence of passive bystanders reduces the likelihood that individuals will intervene and help a victim in a critical situation (Darley & Latané, 1968; Latané & Darley, 1968, 1970; Latané & Nida, 1981). To account for the effect, Latané and Darley (1970) proposed a five-step psychological process model. They postulated that for intervention to occur, the bystander needs to (1) notice a critical situation, (2) construe the situation as an emergency, (3) develop a feeling of personal responsibility, (4) believe that he or she has the skills necessary to succeed, and (5) reach a conscious decision to help.

Latané and Darley (1970) identified three different psychological processes that might interfere with the completion of this sequence. The first process is *diffusion of responsibility*, which refers to the tendency to subjectively divide the personal responsibility to help by the number (*N*) of bystanders. The more bystanders there are, the less personal responsibility any individual bystander will feel. Likewise, the individual bystander will only feel responsible for a fraction of the cost to the victim associated with non-intervention. The second process is *evaluation apprehension*, which refers to the fear of being judged by others when acting publicly. In other words, individuals fear to make mistakes or act inadequately when they feel observed, which makes them more reluctant to intervene in critical situations. The third process is *pluralistic ignorance*, which results from the tendency to rely on the overt reactions of others when defining an ambiguous situation. A maximum bystander effect occurs when no one intervenes because everyone believes that no one else perceives an emergency (cf. Latané & Nida, 1981).

The bystander literature has remained somewhat ambiguous with regard to the relevant psychological processes. Latané and Nida (1981), for example, distinguished the processes of diffusion of responsibility, social influence, and audience inhibition, which are rather close but not fully identical to the processes assumed by Latané and Darley (1970). This discrepancy might be one reason for some of the process ambiguity in bystander research. Finally, it should be noted that there are also explanations of the bystander effect that are derived from evolutionary psychology or game theory. Among these are reciprocal altruism (Axelrod & Hamilton, 1981; Trivers, 1971), competitive altruism (Hardy & Van Vugt, 2006), inclusive fitness (Hamilton, 1964a, 1964b), and the volunteer's dilemma (Krueger & Massey, 2009).

The Classic Bystander Research Paradigm

A typical bystander study proceeds as follows: Participants work either alone or in the presence of one or more other participant(s) (passive bystanders) on an allegedly important task (e.g., filling out questionnaires, waiting for the experimenter). They suddenly witness a staged emergency (e.g., the experimenter becomes injured, a perpetrator offends someone, a thief steals something). Their responses to these emergencies are recorded, typically in terms of their probability of intervening and the time it takes them to do so. Results in the multiple-bystander condition are then compared with results in the single-bystander condition. By applying this classic paradigm, bystander effects have been found in many domains. For example, bystanders decrease helping in serious emergencies, such as an injury (Latané & Darley, 1968), an asthma attack (Harris & Robinson, 1973), or physical illness (Darley & Latané, 1968). However, the bystander effect also occurs in less critical situations, such as a stranded motorist (Hurley & Allen, 1974) or other technical problems (Misavage & Richardson, 1974). The effect occurs even in cases of mundane mishaps, as when pencils spill to the ground (Latané & Dabbs, 1975) or when a door needs to be answered (Levy et al., 1972).

The Bystander Literature Before 1981

After the tragic death of Kitty Genovese, Latané and Darley began to investigate the social psychological conditions that keep

people from helping. Their influential research program yielded a variety of important empirical and theoretical insights. Above all, their work showed that the bystander effect is a robust phenomenon that occurs in many experimental and field situations.

Summarizing the existing body of evidence at the time, Latané and Nida (1981) concluded that Darley and Latané's original conjectures and findings had been corroborated. After summarizing Latané and Nida's main findings, we turn to studies conducted after 1981, quantitatively address possible moderating effects, and consider bystander effects in dangerous emergencies. Besides focusing on the role of the number of bystanders, Latané and Nida looked at seven different characteristics of bystander/emergency situations: attributes of the incident (e.g., incident occurred in rural vs. urban areas), whether the study was conducted in the laboratory or in the field, to what extent the incident was ambiguous, bystander/participant attributes (e.g., bystander competency, sex of participant), victim attributes (e.g., sex of victim), attributes of other bystanders (e.g., friends vs. strangers), and to what extent bystanders could communicate with each other. Overall, the authors identified four different contexts of the bystander effect: (1) all bystanders are in danger (e.g., a room becomes suddenly filled with smoke; Latané & Darley, 1968; or a fire bell started to ring; Ross & Braband, 1973), (2) a victim is in danger (e.g., a person has an asthma attack; Harris & Robinson, 1973; a person simulates a seizure; Darley & Latané, 1968; or a person falls from a bookshelf; Latané & Rodin, 1969), (3) villain acts (e.g., a perpetrator steels money; Latané & Elman, 1970; a case of beer is stolen; Latané & Darley, 1970; books are stolen; Howard & Crano, 1974), and (4) non-emergency incidents (e.g., answering the door; Freeman, 1974; help with a broken car tire; Hurley & Allen, 1974; or leaving a tip; Freeman, Walker, Bordon, & Latané, 1975). Latané and Nida's distinction between emergency/villain acts (with implied high danger in case of intervention) and non-emergency situations (with implied low danger in case of intervention) is of special interest for the present meta-analysis, because we postulate on the basis of more recent findings (e.g., Fischer et al., 2006) that the strength of the bystander effect systematically varies with the bystander's expected danger when deciding whether to help. However, no firm answer can yet be given to this question because Latané and Nida did not statistically test whether there are differences in effect sizes between emergency (high-danger) versus non-emergency (low-danger) situations. In addition, we test whether the expected effect of emergency danger on the bystander effect depends on various moderators investigated by Latané and Nida.

Most importantly, Latané and Nida (1981) concluded that helping is reduced when the number of bystanders increases or when the situation is ambiguous (e.g., Clark & Word, 1974; Solomon, Solomon, & Stone, 1978). The effect occurs both in the laboratory and in the field (e.g., Shaffer, Rogel, & Hendrick, 1975). Latané and Nida found that the bystander effect occurs for both sexes of participant and victim (e.g., Latané & Dabbs, 1975) as well as across nearly all age groups (except very young children; Staub, 1970). Moreover, Latané and Nida found that the bystander effect tends to be stronger in cities than in rural areas (e.g., Merrens, 1973). The competence of the bystanders yielded mixed results. Sometimes highly competent bystanders reduced the bystander effect (e.g., Horowitz, 1971), and sometimes they increased it (e.g., Darley & Latané, 1968). Mixed conclusions were also drawn for age of bystanders (Ross, 1971) and similarity between them (Smith, Smythe, & Lien, 1972). In contrast, more helping was found for bystanders who were friends instead of strangers (Latané & Rodin, 1969). Finally, Latané and Nida stressed the importance of communication possibilities among bystanders in predicting the bystander effect; rather counter-intuitively, they found that increased communication possibilities increased the bystander effect.

In sum, from the perspective of the focal individual who is supposed to help, Latané and Nida (1981) found a substantial bystander effect in groups. Whereas 75% of participants helped when they faced a critical incident alone, only 53% did so when other bystanders were present. Also, from the victim's perspective, the likelihood of receiving help was lower when his or her need was witnessed by a group (70% helping) versus by a single person (82% helping); however, this difference was attenuated to nonsignificance when bystanders could not communicate with one another (for a critical discussion, see Krueger & Massey, 2009). The authors concluded that the effect is robust and that there are few limiting boundary conditions, such as very young bystander age (Staub, 1970), low situational ambiguity (Clark & Word, 1972), low competence to intervene (Bickman, 1971), or reduced communication among bystanders (Latané & Darley, 1976).

Although these conclusions were justified in light of the empirical evidence available at the time, Latané and Nida (1981) did not systematically employ quantitative meta-analytical methods to statistically test potential moderators. The present meta-analysis provides quantitative moderation analyses, integrates new studies on the bystander effect, and provides a new theoretical framework for the bystander effect in dangerous emergencies. Latané and Nida's theoretical distinction between emergency and non-emergency situations is of special interest in this meta-analysis, as we expect the bystander effect to be stronger in the latter than the former.

The Bystander Literature After 1981

We found 15 new articles on the bystander effect with over 40 effect sizes. The present meta-analysis adds these effect sizes to the effect sizes of the classic research and tests for theoretically important moderation effects. The new studies address a variety of critical issues, such as bystander intervention in dangerous emergencies (Fischer et al., 2006; Harari, Harari, & White, 1985), high versus low ambiguous emergencies (Kalafat, Elias, & Gara, 1993), or bystander intervention for a Black victim versus a White victim (Gaertner, Dovidio, & Johnson, 1982). Some recent studies investigated the bystander effect in new media contexts (e.g., online response via e-mail, sharing of virtual knowledge; Barron & Yechiam, 2002; Blair, Thompson, & Wuensch, 2005; Lewis, Thompson, Wuensch, Grossnickle, & Cope, 2004; Markey, 2000; Voelpel, Eckhoff, & Forster, 2008), social control behavior (Chekroun & Brauer, 2002), donation behavior (Wiesenthal, Austrom, & Silverman, 1983), or effects of group cohesiveness on bystander intervention (Rutkowski, Gruder, & Romer, 1983). Finally, with a focus on potential interventions against the bystander effect, research after 1981 has investigated the effects of trained versus untrained bystander groups (Shotland & Heinold, 1985), different levels of bystander competencies (Cramer, McMaster, Bartell, & Dragna, 1988; Pantin & Carver, 1982), and the effects of remind-

ers of disinhibition on bystander intervention (van den Bos, Müller, & van Bussel, 2009).

In sum, typical findings of more recent research were that the bystander effect (a) also occurs in the virtual world of the Internet (e-mail paradigms: Barron & Yechiam, 2002; Blair et al., 2005; for conflicting results, see also Lewis et al., 2004; help request in online communities: Voelpel et al., 2008) and (b) is stronger for lowly than for highly competent bystanders (Cramer et al., 1988; Pantin & Carver, 1982; for an implicit form of competency induction, see also van den Bos et al., 2009; for a conflicting finding, see Shotland & Heinhold, 1985). Finally, which is most important for the theoretical rationale of the present meta-analysis, some recent studies suggest that bystander inhibition is less pronounced in high-danger situations. For example, in the context of a simulated rape in a parking lot, Harari et al. (1985) found higher helping rates in the bystander conditions than in the alone condition. Similarly, Schwartz and Gottlieb (1976) found no bystander effect in a dangerous emergency (i.e., theft with a perpetrator present) when evaluation apprehension was possible. Finally, Fischer et al. (2006) directly manipulated whether the situation was highly dangerous. Again, there was a significant bystander effect for a non-dangerous emergency, but none for a dangerous emergency, where the focal participants could expect increased physical and/or social costs in case of intervention against a perpetrator.

In conclusion, the most noteworthy tendency in recent research is that the bystander effect often does not occur when the emergency is a dangerous one or when the bystanders are highly competent. The classic bystander research regarded present bystanders as "something negative," which reduces the probability of prosocial intervention. On the basis of the review of the more recent literature, we suspect that this perspective is not always correct. Bystanders can act as a positive source of physical support in case a focal person is in the process of deciding whether to intervene in a critical situation (especially when they are perceived as competent bystanders). The present meta-analysis allows us to systematically examine the possibility of a non-inhibiting (non-negative) bystander effect. In the following, we further explore this theoretical idea. We explain why we expect that dangerous emergencies are associated with a reduced magnitude of the bystander effect (see also Greitemeyer, Fischer, Kastenmüller, & Frey, 2006; Greitemeyer, Osswald, Fischer, & Frey, 2007).

Bystanders in Dangerous Emergencies

Empirical Evidence for a Reduced Bystander Effect in Dangerous Emergencies

Schwartz and Gottlieb (1976) simulated a violent theft in a campus building. A group of participants discussed a sexually embarrassing topic via intercom when they suddenly heard an intruder starting a fight with another participant. The bystanders heard a loud quarrel between the perpetrator and the victim as well as signs of a physical fight (i.e., the victim fell down and cried out loud). Participants heard this scene either alone or in the company of the other participants. Interestingly, when evaluation apprehension was possible participants in the bystander condition were more likely to help than participants in the non-bystander condition. Likewise, Clark and Word (1974) found no bystander effect when the emergency situation was unambiguous and serious. Solomon et al. (1978) found only a small bystander effect when participants could clearly recognize that an emergency had happened. Harari et al. (1985) found a similar result in a field experiment. While men were walking either alone (non-bystander condition) or together with another man (bystander condition) to their car in a parking lot, a man was attempting to rape a woman. A male and female actor staged this dangerous emergency situation; the man put his hand around the woman's waist, and the woman screamed out for help loudly. As in Schwartz and Gottlieb's study, participants in the bystander condition (85%) were more likely to intervene than those in the non-bystander condition (65%).

Fischer et al. (2006) also found a non-inhibiting (non-negative) effect of bystanders in a dangerous emergency situation. These authors used an experimental setting, in which participants either alone or in the company of a passive bystander observed an allegedly live broadcast of a cross-gender communication (a videotaped situation staged by two professional actors) in which the man sexually harassed the woman. The cost of intervention was manipulated by varying the physical stature of the perpetrator. In the low-danger condition, the perpetrator was a rather small man with a slight build. In the high-danger condition, the perpetrator was tall and fierce looking. Fischer et al. (2006) found a strong bystander effect in the low-danger condition (50% of participants helped in the no-bystander condition, whereas only 5.9% of participants helped in the bystander condition), but no effect in the high-danger condition (44% and 40% helping, respectively, in the no-bystander and the bystander condition).

Finally, research on social control yielded similar positive bystander effects. Chekroun and Brauer (2002) exposed participants to vignettes of antisocial behavior by a focal person (e.g., littering a park with plastic bottles). Interestingly, participants' reported intervention rates (social control) against a violator of social norms increased with the number of additional bystanders.

In sum, several recent studies revealed that additional bystanders can have non-inhibiting (non-negative) effects when focal participants are confronted with dangerous emergencies in which they would expect increased physical or social costs in case of intervention. We now consider two potential processes that might explain this positive bystander effect.

Potential Explanations for Reduced Bystander Inhibition in Dangerous Emergencies

1. Arousal and costs of non-intervention. Why do dangerous emergencies attenuate or reverse the bystander effect? Fischer et al. (2006) argued that dangerous emergencies are recognized faster and less ambiguously, which increases the cost of not helping the victim. Consequently, the bystander's experienced arousal should be increased, which might lead to increased helping independently of whether additional bystanders are present. This line of argument is consistent with predictions of the *arousal: cost–reward model* (see Dovidio, Piliavin, Gaertner, Schroeder, & Clark, 1991; Dovidio, Piliavin, Schroeder, & Penner, 2006; Piliavin, Dovidio, Gaertner, & Clark, 1981; Schroeder, Penner, Dovidio, & Piliavin, 1995), which assumes that unambiguous and severe emergencies increase experienced arousal (as a function of the victim's distress), which can then be reduced by helping the victim. This model would explain why the bystander effect is smaller in dangerous than in non-dangerous emergencies. We also

argue that experienced arousal should be strongest when a focal bystander perceives both high levels of danger to the victim as well as high danger to the self in case of intervention. The experience of danger to the self is a direct and immediate source of physical arousal, which fosters the identification and attribution of a real emergency where one should help. Thus, situations where bystanders experience increased danger to themselves should reduce the bystander effect. The present meta-analysis directly tests this hypothesis.

2. Bystanders as a source of physical support in the face of fear. If one is to intervene in a dangerous emergency, one may have to fear negative physical consequences. A perpetrator may not only attack the victim but also the intervener. In that case, additional bystanders may provide support in defeating a potential perpetrator. This line of reasoning is consistent with results reported by Horowitz (1971), who found that the bystander effect was reduced when the focal person knew that the other three bystanders were a competent service group instead of an unspecified social group. Participants in the service group condition were more likely to help (65%) than participants in the alone condition (55%). In contrast, the social group condition yielded the traditional negative bystander effect (20%). Recent studies showing that high bystander competency reduces the bystander effect also support this line of reasoning (e.g., Cramer et al., 1988; Pantin & Carver, 1982; van den Bos et al., 2009). These results provide evidence for the idea that bystanders can serve as a source of physical support in helping and thus can facilitate the individual's decision to help in dangerous emergencies.

To conclude, we expect that high-danger emergencies increase the focal bystander's fear that he or she will be attacked or injured in case of intervention. However, if other bystanders are present, they are recognized as a source of physical support, which mitigates against the traditional bystander effect. Note that we do not expect a complete disappearance of the bystander effect in dangerous emergencies. Instead, we expect that it substantially declines because of the fact that many dangerous emergency situations can only be resolved by a group. For example, additional bystanders could help to overpower a fierce perpetrator. We also do not expect the same psychological processes to be engaged when a bystander recognizes danger only to the victim but not the self. If, for example, a person who cannot swim falls into a lake, a single helper is sufficient—a circumstance that may trigger a diffusion of responsibility if there are more than one. Thus, we expect that it is mainly increased danger to the focal bystander that reduces the bystander effect, but not increased danger to the victim (which can be completely distinct from danger of intervention). In other words, if bystanders experience increased danger to themselves in case of intervention, they look for other bystanders to help them to intervene (e.g., to overpower a fierce perpetrator), which should finally reduce diffusion of responsibility and thus also attenuate the magnitude of the bystander effect (because bystanders acknowledge that they can only resolve the dangerous situation by cooperation in a group). In contrast, if increased danger is only attributed to the victim but not the focal bystander (e.g., someone fell into a river), other bystanders are less required to assist the focal bystander and thus processes of diffusion of responsibility should be more likely to occur, which finally increases the magnitude of the bystander effect.

3. Rational choice and the informational approach. As an alternative to the arousal hypothesis, a rational-choice hypothesis may also explain why especially dangerous emergencies reduce the bystander effect. From this perspective, a bystander's decision to help depends on the perceived cost of helping, the benefit of helping to the victim, and the perceived likelihood that other bystanders will help (Franzen, 1999; Krueger & Massey, 2009; Penner, Dovidio, Piliavin, & Schroeder, 2005). In general, a high cost of helping should reduce the individual's willingness to act. However, some emergencies might be so dangerous that effective help cannot be given by one individual; in that case, only several cooperating bystanders might provide effective and safe help (e.g., if a cruel perpetrator is present, many bystanders are more effective to resolve the situation without severe injuries than just one bystander alone). In other words, dangerous emergencies might be effectively resolved only if several bystander coordinate their help. In addition, dangerous emergencies might simply lead to the expectation that other bystanders will help as well (because the situation is so dangerous), which would additionally increase individual helping. Finally, from an informational perspective, additional bystanders might provide a clearer definition of the situation as a potential emergency, which at the end would also lead to a reduced bystander effect in dangerous incidents.

In sum, recent studies suggest that highly dangerous emergencies attenuate the size of the bystander effect. Against the background of the traditional literature, this finding may be surprising, but it can be explained by (a) increased levels of arousal that is experienced especially in high-danger situations, (b) reduced fear based on the expectation that additional bystanders can provide physical support in dangerous emergencies, and (c) the rational expectation that some emergencies can only be resolved by cooperation and coordination between several bystanders.

The Present Meta-Analysis: Rationale and Hypotheses

Rationale

The present meta-analysis aimed to (a) evaluate the updated research base, (b) provide a quantitative and theory-grounded exploration of relevant moderator variables, and (c) gain insight into contexts and conditions that produce reductions or reversals of the traditional bystander effect. The latter goal was specifically motivated by recent studies involving particularly dangerous emergencies.

First, we tested the classic moderator variables considered by Latané and Nida (1981), including various study attributes (e.g., study design, randomization, study location), participant attributes (e.g., sex, age, education), bystander attributes (e.g., sex, relation between bystanders, physical presence, number of present bystanders), and victim attributes (e.g., sex). Furthermore, we coded emergency attributes that are theoretically related to the idea that dangerous emergencies lead to a less pronounced bystander effect than non-dangerous emergencies, including emergency danger, bystander competencies, costs of intervention, costs of non-intervention, and perpetrator presence.

Hypotheses

Our main theoretical question was whether dangerous emergencies are associated with a smaller bystander effect than are non-

dangerous emergencies. We expected that dangerous emergencies would be associated with increased levels of emergency awareness (triggered by increased perceived costs of intervention), increased perceived costs of non-intervention, as well as increased expected physical support by other bystanders, which should altogether then reduce the bystander effect. This general hypothesis entails a series of specific predictions: The bystander effect will be reduced (1) in dangerous emergencies (i.e., dangerous emergencies increase the perceived costs to the bystander and victim and thus experienced arousal), (2) when the intervening person expects increased physical costs (i.e., increased physical costs lead to the attribution that a real emergency is at stake), (3) in realistic field settings compared with artificial laboratory settings (i.e., field settings normally contain more real, non-confederate bystanders, which should generally provide a clearer definition of a situation as a potential emergency), (4) in situations with male rather than female bystanders (i.e., because of their greater physical strength, male bystanders should be able to provide more physical support in case of intervention than female bystanders), and (5) when a perpetrator is present versus not present (i.e., a present perpetrator increases the attribution of danger and emergency and thus experienced arousal). Our theoretical outlook suggests that these five moderators cohere in that they all capture facets of the general perception of "costs of intervention." If these costs are perceived to be high (i.e., high perceived danger), focal individuals experience increased arousal and attribute a real emergency, which finally leads to increased helping responses.

Method

Search of Studies

We used the following methods to identify relevant research reports. First, literature searches were conducted in the following databases: PsycINFO, Educational Resources Information Center (ERIC), PubMed, PsychSpider/ZPID (a database for German language publications), PSYNDEX, Dissertation Abstracts International, and Google Scholar. The following keywords were entered as part of the search: "bystander," "prosocial*," "help*," "diffusion of responsibility," "evaluation apprehension," "pluralistic ignorance," "social influence," "social inhibition," "group size & helping," "bystander intervention," "unresponsive bystander," as well as all combinations of these search words. Secondly, we contacted researchers in the field via various social psychology mailing lists to locate unpublished studies. Finally, the reference lists of book chapters and review articles on the bystander effect and prosocial behavior in general were also investigated and used as links to additional research.

Selection Criteria

Two of the authors (Claudia Vogrincic and Magdalena Wicher) conducted the literature search. Abstracts were inspected, and reports were excluded on the basis of the following criteria: (a) not written in English or German, (b) containing no empirical study or not reporting original data (e.g., Funder & Ozer, 1983; House & Wolf, 1978), (c) no relevant measure of the bystander effect (e.g., Cacioppo, Petty, & Losch, 1986; Fritzsche, Finkelstein, & Penner, 2000), (d) not providing sufficient statistical data to compute an effect size (and where it was not possible to contact the authors), (e) studies that did not vary group size in a helping context (e.g., Levine, Cassidy, Brazier, & Reicher, 2002; Tice & Baumeister, 1985) or did not report separate *N*s for different group sizes (e.g., Markey, 2000), and (f) studies that varied group size only by comparing helping in urban versus rural areas (without a concrete indicator of group size difference). We included the data of studies that systematically varied number of bystanders independent of whether they were experiments or quasi-experiments. There were no cultural restrictions for study inclusion.

Measurement of Bystander Response

The bystander research mainly employed two types of data: (a) actual helping response[2] and (b) response latencies, with many studies measuring both types of reaction. In the present meta-analysis, for effect size calculation, we used a combination of both response types (i.e., mean weighted effect size) unless only one type of response was reported.

Coding and Analysis of Potential Moderators

Meta-analysis is a powerful tool to identify potential moderators, which are defined as variables that systematically affect the magnitude of the mean effect size (e.g., specific characteristics of the study, experimental situations, sub-populations of the participant sample or specific psychological processes). We first tested whether the distribution of effect sizes was significantly more heterogeneous than expected from sampling error alone. If it was, we performed further statistical analyses to partition the variance among the observed effect sizes (for a similar procedure, see Grabe, Ward, & Shibley-Hyde, 2008). Fifteen different categorical variables were coded and then tested to see whether they moderated the magnitude of the association between presence of additional bystanders (yes vs. no) and helping a person in need.

Study attributes. (a) Study design (experimental vs. quasi-experimental), (b) random assignment to experimental condition (yes vs. no), (c) laboratory versus field studies, and (d) year of publication.

Participant attributes. (a) Sex of participants.

Bystander attributes. (a) Number of present bystanders (1, 2, 3, 4, 5, and more), (b) relation between bystanders (familiar vs. stranger), (c) sex of bystanders (female vs. male vs. mixed vs. unknown), and (d) real (i.e., actually present naive other participant) versus confederate versus implied bystanders (i.e., bystanders who are not actually present in the critical situation but implied, e.g., to be next door or available via intercom).

Victim attributes. (a) Sex of victim and (b) proximity between focal bystander and victim (1 = *low*, 2 = *moderate*, 3 = *high*); for example, low values reflect situations where the victim was in another room and thus not visible; high values reflect situations where the victim was in the same room and thus visible to the focal bystander.

Danger and costs of intervention. (a) Type of critical situation (emergency vs. non-emergency vs. villain act vs. bystanders

[2] For consistency reasons we only used actual helping rates, which consistently reflects the victim's perspective of receiving help.

in danger situations); the emergency variable captured situations where a victim faced immediate danger, such as the victim injured him- or herself, or was threatened by a perpetrator; non-emergency situations reflected non-dangerous mishaps, such as spilt pens or a flat tire; villain acts reflected situations where a perpetrator committed some form of crime; bystanders in danger situations reflected incidents where no other victim except the bystanders themselves were present;[3] (b) perpetrator present versus non-present; (c) expected physical danger for participant (focal bystander) in case of intervention (high vs. low); for example, high physical danger reflected situations where the focal bystander could expect to be physically assaulted by a perpetrator; low physical danger reflected situations where the focal bystander could only expect financial or opportunity costs in case of intervention; and (d) potential danger to the victim (i.e., costs of non-intervention; high vs. low); for example, high danger (high cost of non-intervention) reflected situations where the victim would suffer from physical injury in case of non-intervention; low danger (low cost of non-intervention) reflected situations where the victim only had to expect financial or opportunity costs in case of non-intervention. Our understanding and coding of what is a dangerous emergency is similar to that of Shotland and Huston (1979), who found that emergencies are characterized by the following factors: accident situations, high agreement among bystanders that there is a problem, and potential threat and harm to the victim. Magdalena Wicher and Martina Kainbacher independently coded all moderators. Their agreement was greater than 95%, and the remaining inconsistencies were resolved by discussion. Finally, all codings were controlled for quality by Claudia Vogrincic.

Final Sample of Studies and Calculation of Effect Sizes

We obtained a total of 153 effect size estimates with a final sample of $N = 105$ fully independent effect size estimates from $N = 53$ articles on the basis of more than 7,700 participants (for an overview of included studies and independent subgroups, see Table 1). The effect size g recommended by Hedges and Becker (1986) was used in the statistical analysis. All effect sizes were computed via the statistics program Comprehensive Meta-Analysis, Version 2.2.048 (Borenstein, Hedges, Higgins, & Rothstein, 2005). We used a fixed effect model to assess the heterogeneity in different subsets of studies. Fixed effect models are sensitive to the number of participants within each study, so that studies with low sample size but extreme effects have less impact on the results of the meta-analysis. This was important to us to ensure that the test of our hypothesis that dangerous emergencies reduce the bystander effect is not biased by single studies with extreme effect sizes. Because fixed effect models assume a common true effect that underlies all studies, we also performed random-effects analyses to examine the critical variable of level of danger (high vs. low). Overall, it turned out that use of fixed or random effects models did not qualify the main findings.

If means and standard deviations were reported in the research studies, we computed the index g by subtracting the mean for the control group (no bystander present) from the mean for the experimental group (bystander present group) and divided the difference by the pooled within-group standard deviation. Hence, a negative sign indicated an inhibitory effect of bystanders on helping behavior. When no means and standard deviations were reported, we estimated g from t, F, or p values following the procedures recommended by Hedges and Becker (1986). If there was more than one measure of helping, we computed means as a composite measure. We report g along with its 95% confidence interval (CI), the number of samples (k) on which it is based, and Q_T as the homogeneity statistic. We performed moderator analyses if there was significant effect size heterogeneity (Q).

Results

Mean Effect Sizes

For an overview of effect size parameters, see Table 2. The weighted mean effect size (point estimate) for the help-reducing bystander effect over 105 independent samples and more than 7,700 participants was −0.35 for the fixed effects model (95% CI [−0.40, −0.29], $SE = 0.03$, $Z = -11.66$, $p < .001$) and −0.33 for the random effects model (95% CI [−0.45, −0.22], $SE = 0.056$, $Z = -5.96$, $p < .001$), which amounts to a small to moderate effect (Cohen, 1988). Further analyses within the fixed effects model revealed that the sample of effect sizes was heterogeneous, $Q_T(104) = 266.29$, $p < .001$. Therefore, further moderator analyses were required.

Moderator Analyses on Variables Related to Emergency Danger

In the present meta-analysis, perceived danger is reflected by the following coded variables: (a) emergency danger (high vs. low), (b) perpetrator present versus absent, (c) and physical versus non-physical cost of intervention. In our opinion, these three interconnected variables reflect whether a critical situation is dangerous for the potentially intervening person. All moderator effects were tested in a fixed-effects model. For an overview, see Table 3.

Emergency versus non-emergency situations. As expected, non-emergencies ($g = -0.47$, $SE = 0.041$, $Z = -11.42$, $p < .001$, $N = 22$) yielded a larger bystander effect than dangerous emergencies ($g = -0.30$, $SE = 0.048$, $Z = -6.34$, $p < .001$, $N = 65$) and potentially dangerous villain acts ($g = 0.29$, $SE = 0.101$, $Z = 2.82$, $p = .005$, $N = 14$), $Q_B(1) = 48.42$, $p < .001$.[4,5] Separate 1-*df* comparisons revealed that non-emergencies led to a larger bystander effect than dangerous emergencies, $Q_B(1) = 6.77$, $p = .009$, as well as potentially dangerous villain acts, $Q_B(1) = 47.51$, $p < .001$.

[3] Please note that because of conceptual similarity, dangerous emergencies and villain acts were collapsed in some of the later moderator analyses into the concept of "high-danger situations."

[4] The classic category "bystanders in danger" (see Latané & Nida, 1981) was excluded from these and all following analyses because of the low number of independent effect sizes ($N = 4$, $g = -0.90$).

[5] This crucial effect was also significant within the assumptions of a random effects model, $Q(2) = 17.20$, $p < .001$.

Table 1
Effect Sizes Estimates (Point Estimate Hedges' g) of Helping Behavior (Actual Intervention and Response Latencies Combined)

Study	Sample	*g*	*N*			Study type	Study design	Setting	Perpetrator present	No. of bystanders	Type of costs	Dependent variable
			E	C	S							
Barron & Yechiam (2002)	quintuple institutional	−0.176	74	35		field	exp	non emerg	no	4	time	helping rate
	quintuple generic	−0.264	81	35		field	exp	non emerg	no	4	time	helping rate
Becker-Haven & Lindskold (1978)	deindividuated	0.298	30	30		lab	exp	emerg	no	1	time	combined
	individuated	−1.198	30	30		lab	exp	emerg	no	1	time	combined
Bickman (1971)	able	−0.945	15	7		lab	exp	emerg	no	1	time	helping rate
	not able	−0.311	15	8		lab	exp	emerg	no	1	time	helping rate
Blair et al. (2005)	full sample	−0.601	200	200		field	exp	non emerg	no	>5	time	combined
Borges & Penta (1977)	full sample	0.309	36	36		field	q.exp	emerg	no	2	physical	helping rate
Campbell (1974)	high need, long hair	−1.798	12	12		field	q.exp	non emerg	no	n.s.	time	interv. score
	high need, short hair	−0.673	12	12		field	q.exp	non emerg	no	n.s.	time	interv. score
	low need, long hair	−0.908	12	12		field	q.exp	non emerg	no	n.s.	time	interv. score
	low need, short hair	−0.671	12	12		field	q.exp	non emerg	no	n.s.	time	interv. score
Chekroun & Brauer (2002) (Study 2)	groups of 2, elevator	0.217	44	33		field	q.exp	villain act	yes	1	n.s.	interv. rate
	groups of 3, elevator	0.746	34	33		field	q.exp	villain act	yes	2	n.s.	interv. rate
	groups of 2, park	0.361	42	18		field	q.exp	villain act	yes	1	n.s.	interv. rate
	groups of 3, park	0.842	22	18		field	q.exp	villain act	yes	2	n.s.	interv. rate
Chekroun & Brauer (2002) (Study 3)	groups of 2	0.764	46	19		field	q.exp	villain act	yes	1	n.s.	interv. rate
	groups of 3	1.036	21	20		field	q.exp	villain act	yes	2	n.s.	interv. rate
Clark & Word (1972) (Study 1)	friends	−0.246	20	5		lab	q.exp	emerg	no	1	time	latency
	strangers	0.621	20	5		lab	q.exp	emerg	no	1	time	latency
Clark & Word (1972) (Study 2)	groups of 2	−0.148	20	10		lab	exp	emerg	no	1	time	helping rate
	groups of 5	0.111	20	10		lab	exp	emerg	no	4	time	helping rate
Clark & Word (1974) (Study 1)	high ambiguity	0.799	8	11		lab	exp	emerg	no	1	physical	helping rate
	moderate ambiguity	0.303	8	11		lab	exp	emerg	no	1	physical	helping rate
	no ambiguity/danger	0.466	8	11		lab	exp	emerg	no	1	physical	helping rate
	no ambiguity/danger	−0.169	8	11		lab	exp	emerg	no	1	physical	helping rate
Clark & Word (1974) (Study 2)	high ambiguity	0.736	8	8		lab	exp	emerg	no	1	physical	helping rate
	moderate ambiguity	0.000	8	8		lab	exp	emerg	no	1	physical	helping rate
	no ambiguity	0.626	8	8		lab	exp	emerg	no	1	physical	helping rate
Cramer et al. (1988)	high competence	−0.207	14	14		lab	exp	emerg	no	1	time	helping rate
	low competence	−0.805	14	14		lab	exp	emerg	no	1	time	helping rate
Darley & Latané (1968)	groups of 5	−1.165	26	7		lab	exp	emerg	no	4	time	combined
	groups of 2	−0.578	13	6		lab	exp	emerg	no	1	time	combined

Table 1 (*continued*)

Study	Sample	*g*	*N* E	*N* C	*N* S	Study type	Study design	Setting	Perpetrator present	No. of bystanders	Type of costs	Dependent variable
Darley et al. (1973)	facing	−0.421	10	5		lab	exp	emerg	no	1	time	helping rate
	non-facing	−1.859	10	5		lab	exp	emerg	no	1	time	helping rate
Fischer et al. (2006)	high danger	0.339	25	18		lab	exp	emerg	yes	1	physical	combined
	low danger	−1.497	17	24		lab	exp	emerg	yes	1	time	helping rate
Gaertner (1975)	Black victim	−2.019	10	10		lab	exp	emerg	no	3	time	helping rate
	White victim	−0.636	10	10		lab	exp	emerg	no	3	time	helping rate
Gaertner & Dovidio (1977)	Black victim	−1.735	16	16		lab	exp	emerg	no	2	time	helping rate
	White victim	−0.199	16	16		lab	exp	emerg	no	2	time	helping rate
Gaertner et al. (1982)	full sample	−0.809	21	22		lab	exp	emerg	no	3	time	latency
A. Gottlieb & Schwartz (1976)	full sample	−0.377	60	60		lab	exp	emerg	no	1	physical	latency rank
J. Gottlieb & Carver (1980)	full sample	−0.375	65	33		lab	exp	emerg	no	4	time	latency
Harari et al. (1985)	full sample	0.609	40	40		field	q.exp	emerg	yes	2	physical	helping rate
Harris & Robinson (1973)	full sample	−0.584	23	23		lab	exp	emerg	no	2	time	latency
Horowitz (1971)	g_1, norm nonsalient	−0.229	10	10		lab	exp	emerg	no	3	time	interv. score
	g_2, norm nonsalient	−0.435	10	10		lab	exp	emerg	no	3	time	interv. score
	g_1, norm salient	−0.595	10	10		lab	exp	emerg	no	3	time	interv. score
	g_2, norm salient	−1.007	10	10		lab	exp	emerg	no	3	time	interv. score
Howard & Crano (1974)	female conversation	0.244	18	18		field	q.exp	villain act	yes	2	time	helping rate
	female no conversation	−0.120	18	18		field	q.exp	villain act	yes	2	time	helping rate
	male conversation	−0.395	18	18		field	q.exp	villain act	yes	2	time	helping rate
	male no conversation	0.000	18	18		field	q.exp	villain act	yes	2	time	helping rate
Hurley & Allen (1974)	full sample	−0.442	2000	500		field	q.exp	non emerg	no	n.s.	time	helping rate
Kalafat et al. (1993)	high ambiguity	−0.677	75	79		lab	exp	non emerg	no	>5	time	helping rate
	low ambiguity	−1.111	79	81		lab	exp	non emerg	no	2	time	helping rate
Karakashian et al. (2006)	full sample	−0.775			83	lab	exp	non emerg	no	2	time	helping rate
Konecni & Ebbesen (1975)	no presence of child	0.094	29	14		field	q.exp	emerg	no	1	physical	combined
	presence of child	−0.428	29	19		field	q.exp	emerg	no	1	physical	combined
Latané & Darley (1968)	confederates	−1.748	10	12		lab	exp	byst. danger	no	2	time	helping rate
	naive bystanders	−0.839	8	12		lab	exp	byst. danger	no	2	time	helping rate
Latané & Rodin (1969)	confederates	−1.825	14	9		lab	exp	emerg	no	1	physical	helping rate
	friends	0.000	20	9		lab	exp	emerg	no	1	physical	helping rate
	strangers	−0.671	20	8		lab	exp	emerg	no	1	physical	helping rate

(*table continues*)

Table 1 (*continued*)

Study	Sample	*g*	*N*			Study type	Study design	Setting	Perpetrator present	No. of bystanders	Type of costs	Dependent variable
			E	C	S							
Lewis et al. (2004)	plus 1	−0.007	158	48		field	exp	non emerg	no	1	time	combined
	plus 14	0.136	153	48		field	exp	non emerg	no	>5	time	combined
	plus 49	0.890	134	49		field	exp	non emerg	no	>5	time	combined
Pantin & Carver (1982)	high competence	0.445	24	30		lab	exp	emerg	no	4	time	latency
	low competence	−0.861	24	20		lab	exp	emerg	no	4	time	latency
Peterson (1983)	full sample	−0.670	51	50		lab	exp	emerg	no	1	time	helping rate
Ross (1971)	scream	−1.229	12	6		lab	exp	emerg	no	2	time	helping rate
	smoke	−1.668	12	6		lab	exp	byst. danger	no	2	time	helping rate
Ross & Braband (1973)	scream	−0.726	28	14		lab	exp	emerg	no	1	time	helping rate
	smoke	−0.332	28	14		lab	exp	byst. danger	no	1	time	helping rate
Rutkowski et al. (1983)	high cohesiveness	0.467	29	30		lab	exp	emerg	no	3	time	combined
	low cohesiveness	−0.595	31	29		lab	exp	emerg	no	3	time	combined
Schwartz & Clausen (1970)	high responsibility	−0.296	60	30		lab	q.exp	emerg	no	4	time	speed score
	low responsibility	−0.685	60	29		lab	q.exp	emerg	no	4	time	speed score
Schwartz & Gottlieb (1976)	full sample	−1.407	13	11		lab	exp	emerg	yes	4	time	helping rate
Senneker & Hendrick (1983)	mixed sex	−0.471	54	26		lab	exp	emerg	no	4	time	latency
	same sex	−0.471	54	26		lab	exp	emerg	no	4	time	latency
Shaffer et al. (1975)	no request female	−0.986	8	8		field	exp	villain act	yes	1	physical	helping rate
	no request male	−1.260	8	8		field	exp	villain act	yes	1	physical	helping rate
	request female	−1.039	8	8		field	exp	villain act	yes	1	physical	helping rate
	request male	−0.277	8	8		field	exp	villain act	yes	1	physical	helping rate
Shotland & Heinold (1985)	trained group	−0.422	40	40		field	exp	emerg	no	2	time	helping rate
	untrained group	−0.358	40	43		field	exp	emerg	no	2	time	helping rate
Smith et al. (1972)	dissimilar	−0.664	20	10		lab	exp	emerg	no	1	time	helping rate
	similar	−1.912	20	10		lab	exp	emerg	no	1	time	helping rate
Solomon et al. (1978)	full sample	−0.275	16	16		field	q.exp	emerg	no	n.s.	time	helping rate
Staub (1970)	full sample	0.671	73	83		lab	q.exp	emerg	no	1	time	helping rate
Valentine (1980)	gaze	0.671	28	28		field	exp	non emerg	no	1	time	helping rate
	no gaze	−0.751	27	27		field	exp	non emerg	no	1	time	helping rate
van den Bos et al. (2009)	disinhibiton	−0.814	13	13		lab	exp	emerg	no	2	time	combined
	no disinhibition	−1.511	13	13		lab	exp	emerg	no	2	time	combined
Voelpel et al. (2008)	large group	−0.268	68	28		field	exp	non emerg	no	>5	time	helping rate
	medium group	−0.781	77	28		field	exp	non emerg	no	>5	time	helping rate
	very large group	−0.154	101	28		field	exp	non emerg	no	>5	time	helping rate
Wegner & Schaefer (1978)	full sample	−0.572	24	24		lab	exp	non emerg	no	3	time	other

Table 1 (*continued*)

Study	Sample	g	N E	N C	N S	Study type	Study design	Setting	Perpetrator present	No. of bystanders	Type of costs	Dependent variable
Wiesenthal et al. (1983) (Study 1)	full sample	−0.493		212		field	q.exp	non emerg	no	>5	time	other
Wiesenthal et al. (1983) (Study 2)	full sample	−0.885	13	7		field	q.exp	non emerg	no	>5	time	other
Wilson (1976)	esteem oriented	−0.987	43	24		lab	exp	emerg	no	2	time	helping rate
	middle	0.134	42	23		lab	exp	emerg	no	2	time	helping rate
	safety oriented	−0.294	42	22		lab	exp	emerg	no	2	time	helping rate

Note. A negative g indicates a negative relationship between the number of bystanders and helping behavior. N = sample size; E = experimental condition; C = control condition; S = survey study; exp = experimental design; q.exp. = quasi-experimental design; emerg = emergency setting; non emerg = non-emergency setting; byst. danger = bystander in danger; n.s. = variable not specified; interv. score = intervention score; interv. rate = intervention rate.

Perpetrator present versus absent. Incidents with no perpetrator present (low danger; $g = -0.41$, $SE = 0.031$, $Z = -13.24$, $p < .001$, $N = 87$) resulted in a larger bystander effect than critical situations where a perpetrator was present (high danger; $g = 0.24$, $SE = 0.091$, $Z = 2.67$, $p = .008$, $N = 18$), $Q_B(1) = 46.30$, $p < .001$.[6] Consistent with our theoretical argument on perceived emergency danger, additional bystanders even lead to increased helping if a perpetrator is present. We interpret this finding in a way that bystanders are perceived as welcome physical support in high-danger situations, which can even increase bystander intervention.

Physical versus non-physical costs of intervention and non-intervention. Situations in which participants had to expect a greater physical cost if they intervened (high danger; $g = -0.06$, $SE = 0.097$, $Z = -0.64$, $p = .53$, $N = 20$) yielded smaller effect sizes than situations in which participants only had to expect financial or opportunity costs (pooled) (low danger; $g = -0.44$, $SE = 0.032$, $Z = -13.58$, $p < .001$, $N = 79$), $Q_B(1) = 13.38$, $p < .001$.[7] In contrast, but as predicted by our theoretical account of perceived emergency danger, the costs of non-intervention from the perspective of the person in need (victim) had no statistical impact on the bystander effect (physical cost to victim in case of non-intervention: $g = -0.34$, $SE = 0.046$, $Z = -7.46$, $p < .001$, $N = 71$; non-physical cost to the victim in case of non-intervention: $g = -0.43$, $SE = 0.042$, $Z = -10.19$, $p < .001$, $N = 26$), $Q_B(1) = 1.97$, $p = .16$. Note that the cost of intervention and the cost of non-intervention affected the magnitude of the bystander effect in the same direction, which is plausible, as both variables should have common variance.

In sum, the bystander effect is reduced when the situation is perceived as dangerous, when a perpetrator is present, or when the focal bystander faces a physical cost of intervention. These meta-analytic findings support recent results reported by Fischer et al. (2006) and others (e.g., Harari et al., 1985). Fischer et al. argued that dangerous emergencies are most likely to induce arousal in the bystander. This argument, which is derived from the arousal: cost–reward model (Dovidio et al., 1991, 2006; Piliavin et al., 1981; Schroeder et al., 1995), suggests that bystanders intervene in part because they seek to reduce their own arousal. The present results suggest that arousal sources related to the perceived danger to oneself in case of intervention seem to explain more variance of the bystander effect than the perceived danger to the victim. Yet, the present findings are only partially consistent with the traditional empathy hypothesis (e.g., Batson, 1998), which assumes that empathy and empathy-induced helping increase mostly with the level of perceived danger to the victim. Finally, the finding that dangerous emergencies lead to increased helping may also be viewed from a social-cognitive perspective. That is, danger might first raise the expectation that others will help, which in turn facilitates one's own decision to help.

[6] This effect was also significant in a random effects model, $Q(1) = 17.43$, $p < .001$.

[7] This effect was also significant in a random effects model, $Q(1) = 8.04$, $p = .005$.

Table 2
Summary of Mean Effect Sizes for Fixed and Random Effects Analyses

Model	*k*	Point estimate *g*	95% CI *LL*	95% CI *UL*	Q_T
Fixed-effects model	105	−0.35***	−0.40	−0.29	266.29***
Random-effects model	105	−0.33***	−0.45	−0.22	
Trim and Fill estimate fixed-effects model (no studies imputed)	105	−0.35***	−0.40	−0.29	
Trim and Fill estimate random-effects model (no studies imputed)	105	−0.33***	−0.45	−0.22	

Note. *k* = number of independent effect sizes; *g* = Hedges' *g* effect size; *LL* = lower limit; *UL* = upper limit; *Q* = heterogeneity parameter.
*** $p \leq .001$.

Moderator Analyses on Variables Related to "Bystanders as Potential Physical Support"

For an overview of the findings presented below, see Table 4. The idea evaluated in this section is that in dangerous situations bystanders can be perceived as a welcome source of physical support, and thus increase the probability that individuals help.

Type of bystander: Real versus confederate versus implied. As expected, the bystander effect was smaller for real (naive) bystanders ($g = -0.23$, $SE = 0.04$, $Z = -5.69$, $p < .001$, $N = 46$) than for instructed confederates ($g = -0.57$, $SE = 0.09$, $Z = -6.30$, $p < .001$, $N = 27$) or bystanders whose presence was only implied ($g = -0.47$, $SE = 0.051$, $Z = -9.20$, $p < .001$, $N = 32$), $Q_B(2) = 20.73$, $p < .001$. Follow-up 1-*df* comparisons revealed that real bystanders led to a smaller bystander effect than did confederates, $Q_B(1) = 11.93$, $p = .001$, or implied bystanders, $Q_B(1) = 13.97$, $p < .001$.

Separate analyses for dangerous (victim in danger and villain acts combined) versus non-dangerous incidents (non-emergency situations) revealed that in contexts of dangerous emergencies real (naive), bystanders even increased helping ($g = 0.21$, $SE = 0.064$, $Z = 3.30$, $p = .001$, $N = 36$) compared with contexts involving confederates ($g = -0.63$, $SE = 0.108$, $Z = -5.84$, $p < .001$, $N = 22$) or contexts involving implied bystanders ($g = -0.50$, $SE = 0.07$, $Z = -7.10$, $p < .001$, $N = 21$), $Q_B(2) = 74.84$, $p < .001$. Real bystanders even increased helping compared with a non-bystander control group. In contrast, no such pattern was found for low-danger, non-emergency settings (real bystanders: $g = -0.49$; confederates: $g = -0.38$; implied bystanders: $g = -0.44$), $Q_B(2) = 0.58$, $p = .75$. Furthermore, real bystanders ($g = 0.41$, $SE = 0.10$, $Z = 4.09$, $p < .001$, $N = 11$) increased helping when a perpetrator was present compared with confederate bystanders ($g = -0.49$, $SE = 0.24$, $Z = -2.05$, $p = .04$, $N = 6$), $Q_B(1) = 11.99$, $p = .001$.[8] In contrast, increased helping of real bystanders was less pronounced for incidents where no perpetrator was present (real bystanders: $g = -0.35$; confederates: $g = -0.58$; indirect bystanders: $g = -0.46$), $Q_B(2) = 6.18$, $p = .045$. This differential pattern was the same for physical versus non-physical costs; however, no separate analyses were computed for this variable because some of the relevant samples of studies were too small (e.g., $N = 1$). Overall, the evidence supports the view that actually present real (non-confederate) bystanders seem to provide physical support, especially in dangerous situations, and thus reduce the bystander effect. This finding is also in line with the more informational perspective that real bystanders might provide a clearer definition of a situation as a real dangerous emergency than confederate bystanders, which overall reduces the bystander effect.

Bystander instruction: Active versus passive versus non-instructed. Bystanders who were instructed to be passive ($g = -0.53$, $SE = 0.052$, $Z = -10.06$, $p < .001$, $N = 51$) produced a larger bystander effect than non-instructed bystanders ($g = -0.37$, $SE = 0.045$, $Z = -8.32$, $p < .001$, $N = 26$) or confederate bystanders who were told to be active ($g = 0.53$, $SE = 0.107$, $Z = 4.91$, $p < .001$, $N = 9$), $Q_B(2) = 78.55$, $p < .001$. Follow-up 1-*df* comparisons revealed that real, non-instructed bystanders had a smaller effect than passive confederates, $Q_B(1) = 5.04$, $p = .025$, but had a larger effect than active confederate bystanders, $Q_B(1) = 59.86$, $p < .001$. Finally, active confederate bystanders also had a smaller inhibitory effect than did passive confederates, $Q_B(1) = 77.98$, $p < .001$.[9]

Consistent with our theoretical model, this moderator effect was qualified by emergency danger. In contexts of dangerous incidents (dangerous emergencies and villain acts combined), active confederates ($g = 0.51$, $SE = 0.113$, $Z = 4.51$, $p < .001$, $N = 8$) and non-instructed bystanders ($g = 0.16$, $SE = 0.098$, $Z = 1.67$, $p = .096$, $N = 16$) produced less bystander inhibition than passive confederates ($g = -0.50$, $SE = 0.056$, $Z = -8.96$, $p < .001$, $N = 45$), $Q_B(2) = 82.29$, $p < .001$. Note that both conditions even increased helping compared with a non-bystander control group. In contrast, both naive ($g = -0.51$) and passive ($g = -0.70$) bystanders had a substantial inhibitory effect in non-emergency situations.[10,11] This pattern of results was similar for different types of cost (physical vs. non-

[8] The indirect bystander group was not analyzed because of $N = 1$ ($g = -1.41$).

[9] We excluded six effect sizes from analyses, which were not clearly attributable into one of these three categories; bystander samples who communicated only via e-mail ($N = 9$) were also excluded from these analyses.

[10] Because of the low sample size of $N = 1$, we did not include active bystanders in this analysis ($g = 0.67$).

[11] Because of low sample sizes in the condition "perpetrator present" (all Ns < 7), we did not perform these combined moderator analyses for the variable "perpetrator present versus absent."

Table 3
Moderators of the Bystander Effect With Regard to Emergency Danger

Variable	Between Q	k samples	Point estimate Hedges' g	Within Q
Emergency danger	48.42***			
Non-emergency (low danger)		22	−0.47***	43.34**
Victim in danger (high danger)		65	−0.30***	141.08***
Villain acts (high danger)		14	0.29**	25.72*
Perpetrator presence	46.30***			
Absent		87	−0.41***	177.89***
Present		18	0.24**	42.09**
Costs of intervention for bystander	13.38***			
Non-physical		79	−0.44***	160.78***
Physical		20	−0.06*ns*	30.87*
Costs of non-intervention for victim	1.97*ns*			
Non-physical		26	−0.43***	45.50**
Physical		71	−0.34***	156.00***

Note. More negative values of g indicate a stronger bystander effect.
* $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$.

physical) as well as for perpetrator present versus non-present. However, because of small sample sizes in some cells, no analyses were performed. In sum, especially in dangerous situations, non-instructed and instructed active bystanders reduce the bystander effect compared with instructed passive bystanders. In other words, bystanders are likely to be perceived as potential physical support when the act and communicate naturally (real bystanders) or when they show active helping behavior anyway.

Implied physical strength of bystanders. We approximated the variable "physical strength" by comparing studies with different proportions of male bystanders, because men normally have more physical strength than women. In line with our expectation that additional bystanders can be regarded as potential physical support and thus reduce the bystander effect, we found lowest effect sizes for studies with all male bystanders ($g = -0.21$, $SE = 0.092$, $Z = -2.23$, $p = .026$, $N = 29$), followed by the mixed sex category ($g = -0.34$, $SE = 0.033$, $Z = -10.41$, $p < .001$, $N = 53$), studies with all female bystanders ($g = -0.44$, $SE = 0.103$, $Z = -4.27$, $p < .001$, $N = 18$), and the unknown sex category ($g = -1.30$, $SE = 0.32$, $Z = -4.09$, $p = .001$, $N = 5$), $Q_B(3) = 12.17$, $p = .007$. Separate follow-up analyses showed that the bystander effect was marginally less pronounced in the all male condition rather than the all female condition, $Q_B(1) = 2.87$, $p = .09$.

Additional combined moderator analyses for high-danger situations (emergencies and villain acts combined) versus low-danger situations (non-emergency situations) revealed that the bystander effect is reduced in higher danger situations as long as male bystanders (with implied high physical strength) are present and available (all male: $g = -0.20$, $SE = 0.094$, $Z = -2.13$, $p = .033$, $N = 28$; mixed sex: $g = -0.09$, $SE = 0.055$, $Z = -1.56$, $p = .12$, $N = 31$; all female: $g = -0.54$, $SE = 0.112$, $Z = -4.78$, $p < .001$, $N = 16$), $Q_B(2) = 13.06$, $p = .001$. The all male versus all female bystander conditions also differed significantly in a 1-*df* follow-up test, $Q_B(1) = 5.29$, $p = .021$. In contrast, and as expected, the presence of male bystanders did not substantially reduce the bystander effect in low-danger, non-emergency situations ($g = -0.48$, $SE = 0.041$, $Z = -11.61$, $p < .001$, $N = 20$).[12]

Familiarity among bystanders. Overall, situations where bystanders knew one another (friends and acquaintances combined) yielded a smaller effect ($g = -0.24$, $SE = 0.041$, $Z = -5.72$, $p < .001$, $N = 30$) than situations where bystanders were complete strangers ($g = -0.46$, $SE = 0.042$, $Z = -10.86$, $p < .001$, $N = 75$), $Q_B(1) = 14.65$, $p < .001$.

In line with our assumption that bystanders can be seen as potential physical support in dangerous emergencies, bystanders who knew one another ($g = 0.30$, $SE = 0.075$, $Z = 3.99$, $p < .001$, $N = 22$) yielded a smaller effect than complete strangers ($g = -0.44$, $SE = 0.053$, $Z = -8.38$, $p < .001$, $N = 57$), $Q_B(1) = 65.77$, $p < .001$. Indeed, familiar bystanders even increased helping compared with a non-bystander control group. In contrast, familiar bystanders ($g = -0.47$, $SE = 0.049$, $Z = -9.49$, $p < .001$, $N = 8$) and strangers ($g = -0.47$, $SE = 0.073$, $Z = -6.35$, $p < .001$, $N = 14$) yielded a similarly strong effect in low-danger, non-emergency settings, $Q_B(1) = 0.001$, $p = .99$. This pattern of results was similar (but not significant) for perpetrator present versus non-present as well as for physical versus non-physical costs of intervention.

Number of bystanders. Overall, situations with one additional bystander ($g = -0.12$, $SE = 0.062$, $Z = -1.87$, $p = .062$, $N = 43$) yielded a smaller effect than situations with two ($g = -0.21$, $SE = 0.078$, $Z = -2.65$, $p = .008$, $N = 25$), three ($g = -0.45$, $SE = 0.119$, $Z = -3.76$, $p < .001$, $N = 10$), four ($g = -0.38$, $SE = 0.083$, $Z = -4.62$, $p < .001$, $N = 12$), or more than five present bystanders ($g = -0.49$, $SE = 0.074$, $Z = -6.62$, $p < .001$, $N = 9$), $Q_B(4) = 19.18$, $p = .001$. This tendency was similar for high- versus low-danger situations. Additional 1-*df* follow-up tests revealed that significant differences only occurred between one versus three bystanders, $Q_B(1) = 6.06$, $p = .014$; one versus four bystanders, $Q_B(1) = 6.66$, $p = .01$; and one versus five and

[12] Because of the low sample size of $N = 2$, no tests for only female bystanders could be performed ($g = 0.08$). Because of the same problem, no analyses were performed for the variables "perpetrator present versus absent" as well as "physical versus non-physical costs of intervention."

Table 4
Moderators of the Bystander Effect With Regard to "Bystanders as Physical Support"

Variable	Between Q	k samples	Point estimate Hedges' g	Within Q
Type of bystander	20.73^{***}			
Real		46	-0.23^{***}	166.55^{***}
Passive confederate		27	-0.57^{***}	40.38^{*}
Implied bystander		32	-0.47^{***}	38.63^{ns}
Bystander instruction	78.55^{***}			
Active		9	0.53^{***}	7.36^{ns}
Passive		51	-0.53^{***}	73.95^{*}
Non-instructed		26	-0.37^{***}	77.75^{***}
Implied physical strength of bystanders	12.17^{**}			
High (all men)		29	-0.21^{*}	160.78^{***}
Less high (all women)		18	-0.44^{***}	30.87^{*}
Mixed sex		53	-0.34^{***}	162.23^{***}
Sex unknown		5	-1.30^{***}	0.85^{ns}
Familiarity among bystanders	14.65^{***}			
Familiar		30	-0.24^{***}	118.52^{***}
Non-familiar		75	-0.46^{***}	133.12^{***}
No. of bystanders	19.18^{**}			
1		43	$-0.12^{\dagger}$	110.83^{***}
2		25	-0.21^{**}	74.59^{***}
3		10	-0.45^{***}	18.01^{*}
4		12	-0.38^{***}	13.15^{ns}
5 and more		9	-0.49^{***}	12.87^{ns}

Note. More negative values of g indicate a stronger bystander effect.
$^{\dagger}p \leq .01$. $^{*}p \leq .05$. $^{**}p \leq .01$. $^{***}p \leq .001$.

more bystanders, $Q_B(1) = 14.86$, $p < .001$. There was no significant difference between one versus two bystanders, $Q_B < 1$. Furthermore, we found significant differences between two versus five and more bystanders, $Q_B(1) = 6.92$, $p = .009$, but not between two versus three and two versus four bystanders, all $Q_B < 2.86$, all ps $> .08$. No significant differences were observed for any of the remaining comparisons, all $Q_B < 1$.[13] To conclude, the finding that the bystander effect increases as the group of bystanders becomes larger is consistent with the predictions of social impact theory (Latané, 1981) and game theory (i.e., the volunteer's dilemma; Krueger & Massey, 2009).

In sum, this set of analyses shows that additional bystanders can provide social, physical, and psychological support when focal individuals have to decide whether to intervene, and this is especially true for dangerous emergencies. For example, the effect is attenuated in dangerous situations when bystanders know one another, when they are real instead of passive confederates, when they can provide more physical support (i.e., when they are male instead of female), and when their presence is physically real instead of implied. Additional combined moderator analyses showed that these effects are stronger for dangerous (emergency) rather than non-dangerous situations. The latter point in particular supports our assumptions about bystanders as potential physical support; when others are actually there, and not just implied (e.g., via intercom systems), the focal helper can expect assistance from other bystanders in dangerous emergencies. The present meta-analysis as well as studies by Horowitz (1971), Fischer et al. (2006), and van den Bos et al. (2009) provide direct support for this reasoning.

The effect of bystanders is not always as negative as traditional research suggests. Additional combined moderator analyses across different numbers of bystanders and different levels of emergency danger revealed support for Latané's (1981) social impact theory, which proposes that the bystander effect increases with the number of bystanders. This was found for both dangerous and non-dangerous situations. At first blush, this finding seems inconsistent with our assumption that additional bystanders serve as physical support in dangerous emergencies. However, from the differential effects for dangerous versus non-dangerous situations, we can conclude that the bystander effect is substantially reduced on a dichotomous qualitative level (i.e., bystander present vs. non-present). As the number of bystander increases on a quantitative level (i.e., one vs. two vs. three bystanders) the magnitude of the bystander effect increases again, because the classic bystander processes, such as diffusion of responsibility or evaluation apprehension, have a broader basis to work.

General Study Attributes

Study design, location, and randomization. For an overview of the results of these moderator analyses, see Table 5. Experimental studies ($g = -0.46$, $SE = 0.042$, $Z = -10.94$, $p < .001$, $N = 78$) produced larger effects than did quasi-experimental studies ($g = -0.23$, $SE = 0.042$, $Z = -5.54$, $p < .001$, $N = 27$), $Q_B(1) = 14.31$, $p < .001$. Laboratory studies ($g = -0.42$, $SE = 0.047$, $Z = -8.74$, $p < .001$, $N = 66$) yielded marginally larger

[13] We also tested with SPSS (weighted curve fitting) whether the effect for number of bystanders is more linear or more quadratic. The results are significant for both linear curve fitting, $b = -.02$, $F(1, 5780) = 24.50$, $p < .001$, as well as for quadratic curve fitting, $b_1 = -.41$, $b_2 = .06$, $F(2, 5779) = 92.20$, $p < .001$.

Table 5
Moderators of the Bystander Effect With Regard to General Study, Participant, and Bystander Attributes

Variable	Between Q	k samples	Point estimate Hedges' g	Within Q
Study design	14.31^{***}			
Experimental		78	-0.46^{***}	127.11^{***}
Quasi-experimental		27	-0.23^{***}	124.86^{***}
Study location	$3.57^{\dagger}$			
Laboratory		66	-0.42^{***}	140.47^{***}
Field		39	-0.30^{***}	122.25^{***}
Randomization	0.09^{ns}			
Yes		79	-0.35^{***}	186.32^{***}
No		21	-0.33^{***}	77.16^{***}
Year of publication	$2.87^{\dagger}$			
≤1975		53	-0.40^{***}	102.42^{***}
>1975		52	-0.30^{***}	161.00^{***}
Sex of victim	0.001^{ns}			
Female		32	-0.34^{***}	106.08^{***}
Male		38	-0.34^{***}	58.07^{*}
Familiarity between bystander and victim	1.01^{ns}			
Familiar		50	-0.40^{***}	81.86^{**}
Non-familiar		51	-0.32^{***}	175.69^{***}
Spatial closeness	1.76^{ns}			
Low		11	-0.42^{***}	15.30^{ns}
Moderate		66	-0.34^{***}	180.13^{***}
High		28	-0.29^{***}	69.09^{***}

Note. More negative values of g indicate a stronger bystander effect.
† $p \leq .01$. * $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$.

bystander effects than field studies ($g = -0.30$, $SE = 0.038$, $Z = -7.94$, $p < .001$, $N = 39$), $Q_B(1) = 3.57$, $p = .059$. Moreover, randomized studies ($g = -0.35$, $SE = 0.04$, $Z = -8.63$, $p < .001$, $N = 79$) did not yield larger effect sizes than non-randomized studies ($g = -0.33$, $SE = 0.045$, $Z = -7.24$, $p < .001$, $N = 21$), $Q_B(1) = 0.09$, $p = .76$.[14]

Finally, a meta-regression analysis revealed a trend suggesting that the bystander effect has diminished over the years ($\beta = .005$, $SE = .002$, $Z = 1.94$, $p = .052$; also see Figure 1). In a separate analysis, we checked whether year of publication was confounded with our main assumption that dangerous incidents reduce the bystander effect. The median year of publication being 1975, we tested whether dangerous emergencies yielded smaller effects both for older (<1975) and more recent studies (>1975). Both for older studies (non-emergencies: $g = -0.48$, $N = 5$; dangerous emergencies: $g = -0.25$, $N = 36$; villain acts: $g = -0.23$, $N = 8$), $Q_B(2) = 6.91$, $p = .032$, and for more recent studies (non-emergencies: $g = -0.45$, $N = 17$; dangerous emergencies: $g = -0.34$, $N = 29$; villain acts: $g = 0.58$, $N = 6$), $Q_B(2) = 54.97$, $p < .001$, the bystander effect was smaller for dangerous compared with non-dangerous incidents. Similar patterns emerged for perpetrator present versus absent as well as physical versus non-physical cost. It seems thus unlikely that the differential impact of high versus low emergency danger on the bystander effect is confounded by year of publication.

Participant and victim attributes. No significant differences were found between female ($g = -0.43$, $N = 19$), male ($g = -0.30$, $N = 34$), and mixed sex ($g = -0.35$, $N = 48$) participant groups, $Q_B < 1.02$. Furthermore, no significant difference was found between situations with a male victim ($g = -0.34$, $N = 38$) versus a female victim ($g = -0.34$, $N = 32$), $Q_B < 1$. Moreover, the magnitude of the bystander effect was not differentially affected by whether the bystander knew the victim (e.g., met the victim before in the experimental situation; $g = -0.40$, $N = 50$) or whether the bystander and victim were strangers ($g = -0.32$, $N = 51$), $Q_B < 1.01$. Finally, spatial closeness between bystander and victim (e.g., whether they saw each other or whether they were in the same room) did not qualify the effect (low closeness: $g = -0.42$, $N = 11$; moderate closeness: $g = -0.34$, $N = 66$; high closeness: $g = -0.29$, $N = 28$), $Q < 1.77$, $p > .41$, although there was a non-significant trend suggesting that closeness might reduce the bystander effect. In addition, combined moderator analyses revealed that this null effect of spatial closeness occurred for both dangerous and non-dangerous situations, all Qs < 1.86, all ps $> .17$.

In sum, moderator analyses revealed that experimental groups yielded larger effect sizes than quasi-experimental (correlational) studies. This effect is also reflected by the finding that laboratory studies (which are more likely to be true experiments than field studies) revealed marginally larger effect sizes than field studies. These findings reflect the fact that experiments and laboratory studies more effectively control confounding variables and other forms of "noise." Another interesting finding was that the bystander effect has declined over the years. Perhaps this trend reflects a positive societal impact of social psychological knowledge. Alternatively, however, this decline may be, at least in part,

[14] Five effect sizes were excluded because no information about randomization was reported.

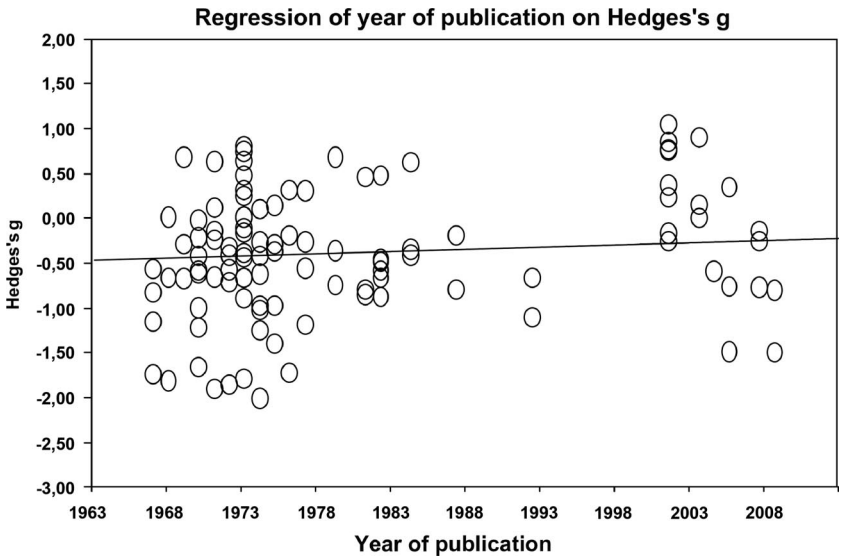

Figure 1. Regression of bystander effect sizes on year of publication.

a more general regression effect, where strong effects attenuate over time (Fiedler & Krueger, in press).

Controlling Publication Bias

We sought to limit errors based on publication bias by incorporating published and unpublished studies, but we received only few replies to our e-mail request for unpublished data. The main reason for this lack of current work might be that the bystander literature is rather mature and settled, and thus researchers did not conduct many new studies. Therefore, we also tried to control for publication bias statistically. First, the classic, though controversial fail-safe *N* analysis revealed that it would take 2,240 studies with null-effects to dilute the reported meta-analytical effect to overall non-significance ($Z = -9.87$ for observed studies, $p < .001$). Second, inspection of the funnel plot yielded no hints of asymmetries that might change the conclusions drawn from the present meta-analysis (see Figure 2). Finally, we used a Trim and Fill analysis to reduce the potential effect of publication bias (Duval, 2005; Duval & Tweedie, 2000). With this additional control, the estimated effect size for the bystander effect did not change ($g = -0.35$, 95% CI [−0.41, −0.29]). The funnel plot was relatively symmetrical and thus no studies had to be imputed by the Trim and Fill analysis. Thus, publication bias is unlikely to have distorted the reported findings even though the number of unpublished effect sizes is rather low.

Controlling Possible Confounds

We inspected inter-correlations among all dichotomous and metric moderator variables (see Table 6). There was a significant correlation between cost of bystander (physical vs. non-physical) and presence of perpetrator, which was expected, however, because both variables reflect the potential danger of the situation from the perspective of the focal bystander. We also found a significant correlation between cost and number of bystanders, which simply means that the cost of intervention declines with the number of bystanders, which was expected as well. In addition, many studies with a large number of bystanders are e-mail or online paradigms, which by their very nature remove the physical dangers that are characteristic of many face-to-face situations. In contrast, we found no significant inter-correlations between cost of intervention to the bystander and publication year, study *N*, type of study (field vs. laboratory), and study design. Nevertheless, to further control for possible confounds between moderators, we compared the moderator effect of dangerous versus non-dangerous emergencies for all levels of the other moderating variables (e.g., field vs. laboratory studies). In total, there were 10 comparisons. All comparisons revealed that high-danger incidents yielded smaller bystander effects than did low-danger incidents. Thus, we are confident that our main findings on cost of intervention are not confounded by other variables.

Finally, we checked whether raw versus log-transformed latencies made a difference on the effect size of the bystander effect. This could be the case because raw values for latencies may be skewed. However, although raw latencies ($g = -0.32$, $N = 22$) yielded a somewhat smaller overall effect size than log-transformed latencies ($g = -0.45$, $N = 4$), this difference was not significant, $Q_B < 1$, $p > .33$. Thus, confounds due to different transformations of latencies are unlikely.

General Discussion

The present meta-analysis provided clear support for the assumption that passive bystanders in critical situations reduce helping responses ($g = -0.35$). Consistent with our theoretical approach, however, bystander inhibition is less pronounced in dangerous than in non-dangerous situations. We argued that in dangerous emergencies, the focal individual may perceive additional bystanders as positive resources for helping. Three psychological processes may contribute to this perception. First, dangerous situations are more likely to be construed as clear-cut emergencies where someone needs help, which increases experienced arousal and thus helping responses. Second, bystanders can

Figure 2. Funnel plot for inspection of potential asymmetries and publication bias.

be seen as providers of physical support and thus reduce fear of intervention. Third, some dangerous emergencies can only be effectively resolved by cooperation and coordination among several bystanders. Rather counter-intuitively, but consistent with this line of argument, the bystander effect was attenuated when situational attributes referred to increased danger, that is, (a) when focal individuals expected increased physical costs for helping (instead of time or financial costs), (b) when a perpetrator was present (vs. absent), and (c) when the critical situation was recognized as a clear-cut emergency (compared with a non-emergency). Less counter-intuitively but consistent with the hypothesis, (d) the effect was marginally reduced when no male bystanders were present to provide physical support.

In sum, the meta-analytic results support recent bystander research, which found no bystander effect in dangerous emergencies (e.g., Fischer et al., 2006; Harari et al., 1985). Moreover, the present meta-analysis yielded additional findings related to study, participant, and bystander attributes. That is, the bystander effect was stronger in experimental than in quasi-experimental studies, in female than in male participants, and in strangers than in friends; the bystander effect was also marginally stronger in laboratory than in field experiments. We also found significant evidence for the idea that the bystander effect becomes stronger with an increasing number of bystanders.

Theoretical Implications

The present meta-analysis identified an important boundary condition for the bystander effect: the perceived danger of an emergency. Dangerous emergencies produced smaller bystander effects than did non-dangerous emergencies. Three processes may account for this finding: (a) Dangerous emergencies are more clearly perceived as actual emergencies; (b) additional bystanders reduce fear because they signal the possibility of social, physical, or psychological support when the focal individual contemplates intervention; and (c) dangerous emergencies are most effectively resolved by coordination and cooperation among a greater number of bystanders. With regard to point (a), dangerous emergencies are more likely to increase a focal individual's arousal and the perceived costs of non-intervention (i.e., increased perceived responsibility for potential negative outcomes for the victim if nobody intervenes). This argument is in line with predictions of the arousal: cost–reward model (Dovidio et al., 1991, 2006; Piliavin et al., 1981; Schroeder et al., 1995), which postulates that severe (and

Table 6
Inter-Correlations of Dichotomous/Rank Ordered (Spearman) and Metric Moderators (Pearson)

Variable	Study *N*	Type of study	Design	Perpetrator	No. of bystanders	Bystander costs
Study *N*	—					
Type of study	−.19	—				
Design	−.16	.54**	—			
Perpetrator	−.07	−.44**	−.37**	—		
No. of bystanders	.47**	−.13	.01	−.16	—	
Bystander costs	−.10	−.07	.02	.28**	−.48**	—
Publication year	.06	−.38**	.01	.24*	.32**	−.18

* $p \leq .05$. ** $p \leq .01$.

thus less ambiguous) emergencies increase arousal (as a function of the victim's distress), which can then be reduced by helping the victim. Fischer et al. (2006) found that reduced bystander inhibition in dangerous emergencies was mediated by increased levels of experienced arousal. Because the empirical evidence for this underlying process is still limited, research is needed to further test this psychological explanation.

With regard to point (b), additional bystanders can provide physical support especially in dangerous emergencies where focal individuals have to fear negative social and physical consequences in case of intervention (e.g., being offended by a perpetrator). Studies by Horowitz (1971), Fischer et al. (2006), and van den Bos et al. (2009) support this idea. Also, Latané and Nida (1981) found small (non-significant) increases of at least one person helping (from the victim's perspective) when bystanders were able to communicate (for a critical discussion, see Krueger & Massey, 2009). Thus, bystander effects are not always as negative as they are typically portrayed. It will be a fruitful endeavor for future research to explore other potential non-inhibiting or even positive effects of bystanders under specific conditions.

Concerning point (c), a reduced bystander effect in dangerous emergencies might by the result of a rational inference process. Some dangerous emergencies might only be resolved if several bystanders cooperate and coordinate their helping response. Some perpetrators might only be defeated if several bystanders act jointly.

The present meta-analysis suggests a more nuanced understanding of the bystander phenomenon by revealing the role of several critical moderator variables. Answers are now available to some of the inconsistencies identified by Latané and Nida (1981). For example, with regard to attributes of the emergency situation, we found that experimental studies yielded larger effects than quasi-experimental studies, which is probably due to the fact that experiments are most powerful in controlling sources of error variance. We also found that laboratory studies revealed marginally larger effects than field studies, which also might be due to better control of potential confounders in the laboratory than in the field. Interesting findings were also obtained with regard to participant attributes. Studies with female participants revealed in tendency a larger bystander effect than studies with male participants. This finding might be due to the typically less physical strength of women compared with men, which is a welcome resource especially when dealing with potential perpetrators in emergencies.

Consistent with the classic notion of the bystander being sensitive to the social context, we found that increasing the number of bystanders also increases the inhibitory effect (Latané & Nida, 1981). Furthermore, the effect was largest when the bystanders were strangers to one another. Finally, situations with an all-male group of bystanders resulted in a smaller effect than situations with female bystanders. This finding is important from the perspective that bystanders in dangerous emergencies reduce the typical inhibitory effect. Especially in dangerous emergencies with increased physical costs of intervention, male bystanders might be perceived as being more helpful than female bystanders in confronting a perpetrator. Future research should test this idea directly and involve mediational analyses to clarify the underlying psychological processes.

Another important theoretical point relates to the result that the perceived cost of intervention moderated the magnitude of the bystander effect, whereas the perceived cost of non-intervention to the victim did not. Participants decided to intervene in emergencies when they perceived their own cost as high (i.e., physical instead of financial or opportunity costs). In contrast, increased physical cost to the victim in case of non-intervention had no differential effect on the effect size magnitude. This finding may seem counterintuitive, but upon closer inspection, it becomes clear how people decide to intervene in critical situations when bystanders are present. If participants recognize that they might be injured in case of intervention, they realize that they are indeed confronted with a dangerous situation (both for themselves as well as the victim). This is a more direct experience than assessing the potential cost to the victim from a third-person perspective. In other words, experienced arousal seems to have its origin in the assessment of own risks and not in the assessment of risks for a potential victim. If individuals perceive their own risk to be high, they may also projectively infer that the costs to the victim are high, and thus intervene (Krueger, 2000).

Moreover, an important finding of the present meta-analysis is that the bystander effect has abated in size over time. This finding could have several reasons. For example, authors of recent studies used different (e.g., less realistic) paradigms than authors of older studies. Another potential explanation for this moderator effect is that knowledge of the bystander effect (i.e., the classic research by Darley & Latané, 1968, and/or the many tragic real life examples, such as Kitty Genovese in the United States or Dominik Brunner in Europe) might have worked against so-called bystander apathy. In fact, Beaman, Barnes, Klentz, and McQuirk (1978) found that students who had learned about the bystander effect in a lecture were more likely to intervene in a bystander emergency at a later date than were students who were not informed. This would be "good news" to the public, showing that social psychological research indeed helps to make the world a bit better.

Finally, it is important to discuss the findings of the present meta-analysis in the light of Manning, Levine, and Collins's (2007) article, which questioned the existence of 38 unresponsive eye witnesses in the Kitty Genovese murder case. The authors worry that research on bystander intervention focuses too much on the negative aspects of groups for helping, thereby neglecting the positive aspects. The findings of the present meta-analysis partially support Manning et al.'s arguments; however, there is also a difference. We found a substantial bystander effect across a broad variety of studies; thus, in contrast to Manning et al., we conclude that there is a negative effect of bystanders on the rate of helping interventions, even if there were no 38 witnesses in the Kitty Genovese murder. However, we also found that this effect is attenuated when bystanders are faced with real emergencies. Under specific conditions (i.e., when the communication among bystanders is real and not manipulated by the experimenter, and when the bystanders are naive individuals instead of instructed confederates), additional bystanders even lead to more, rather than less, helping. In sum, the findings of the present meta-analysis are in line with Manning et al. for the positivity of the human condition with the message, "Yes, if there is a real person in real need under real conditions, I/we will help her."

Methodological Considerations and Limitations

A fail-safe *N* analysis as well as inspection of the funnel plot gave no hint of publication bias. Yet, we were not able to retrieve a large number of unpublished studies, which would have been the best way to control for the "waste-basket effect." The lack of unpublished studies might stem from the fact that bystander studies are difficult to conduct, and thus, not many unpublished studies are available. Moreover, because of the high investment necessary for this sort of study, they are perhaps published rather rapidly.

As expected, some of the moderator variables were intercorrelated, which presents a difficulty that conventional meta-analytic tools cannot correct for. In the present case, however, we do not see a significant threat to the validity of our main argument that dangerous emergencies are associated with a lower bystander effect than non-dangerous ones. That is, we discussed all moderators that refer to emergency danger in a combined fashion. In other words, we regard the variables whether a perpetrator is present, whether the situation is likely to be perceived as a dangerous one, and whether it is an emergency or non-emergency situation as facets of the construct "perceived emergency danger." The moderator results of all four variables point into the same direction, that is, a lower bystander effect in dangerous than in non-dangerous emergencies.

Finally, the present meta-analysis increased confidence in the validity of some processes suggested to fuel the bystander effect. We suggested three different processes to account for the recent findings that dangerous emergencies are associated with a less pronounced bystander effect than non-dangerous incidents—that is, increased arousal due to perceived danger, bystanders as additional physical support in dangerous emergencies, and a more informational approach based on adequate recognizing of a potential dangerous situation. We have most direct support for a combination of the arousal and informational accounts. Findings—that when compared with passive confederates, real bystanders lessen the inhibitory effect on helping, and do so especially in dangerous emergencies—clearly support the assumption that potentially dangerous situations increase the bystanders' arousal and thus foster the correct identification of a dangerous situation. Therefore, additional naive bystanders should further enhance this process in dangerous emergencies. Furthermore, the present meta-analysis also partially supports the "bystander as additional physical support" hypothesis, because we found that, especially in dangerous emergencies, all male bystander conditions marginally reduce the bystander effect compared with all women conditions. Also, non-stranger bystanders produced a less pronounced bystander effect than complete strangers as bystanders. However, as it is the nature of meta-analyses, psychological processes cannot be directly tested by mediation analysis as it is routinely applied to primary data (Baron & Kenny, 1986). Therefore, it is important that these findings can only provide indirect evidence for specific psychological accounts on the bystander effect in dangerous versus non-dangerous emergencies.

Conclusion

Although the present meta-analysis shows that the presence of bystanders reduces helping responses, the picture is not as bleak as conventionally assumed. The finding that bystander inhibition is less pronounced especially in dangerous emergencies gives hope that we will receive help when help is really needed even if there is more than one witness of our plight.

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