

Is Empathic Emotion a Source of Altruistic Motivation?

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It has been suggested that empathy leads to altruistic rather than egoistic motivation to help. This hypothesis was tested by having subjects watch another female undergraduate receive electric shocks and then giving them a chance to help her by taking the remaining shocks themselves. In each of two experiments, subjects' level of empathic emotion (low versus high) and their ease of escape from continuing to watch the victim suffer if they did not help (easy versus difficult) were manipulated in a 2×2 design. We reasoned that if empathy led to altruistic motivation, subjects feeling a high degree of empathy for the victim should be as ready to help when escape without helping was easy as when it was difficult. But if empathy led to egoistic motivation, subjects feeling empathy should be more ready to help when escape was difficult than when it was easy. Results of each experiment followed the former pattern when empathy was high and the latter pattern when empathy was low, supporting the hypothesis that empathy leads to altruistic rather than egoistic motivation to help.

Evidence indicates that feeling empathy for the person in need is an important motivator of helping (cf. Aderman & Berkowitz, 1970; Aronfreed & Paskal, cited in Aronfreed, 1970; Coke, Batson, & McDavis, 1978; Harris & Huang, 1973; Krebs, 1975; Mehrabian & Epstein, 1972). In the past few years, a number of researchers (Aronfreed, 1970; Batson, Darley, & Coke, 1978; Hoffman, 1975; Krebs, 1975) have hypothesized that this motivation might be truly altruistic, that is, directed toward the end-state goal of reducing the other's distress. If the empathy-altruism hypothesis is correct, it would have broad theoretical implications, for few if any major theories of motivation allow for the possibility of truly altruistic motivation (cf. Bolles, 1975, for a review). Current theories tend to be egoistic; they are built on the assumption that everything we do is ultimately directed toward the end-state goal of benefiting ourselves.

The egoistic orientation of modern psychology should not be dismissed lightly; it has prevailed for decades, and it can easily account for what might appear to be altruistic motivation arising from empathic emotion. To illustrate: You may answer the question of why you helped someone in other-directed, altruistic terms—you felt sorry for that person and wished to reduce his or her distress. But this apparently altruistic concern to reduce another's distress may not have been the end-state goal of your action but rather an intermediate means to the ultimate end of reducing *your own* distress. Your own distress could have arisen not only from the unpleasant emotions you experienced as a result of knowing that the other person was suffering (shock, disgust, fear, or grief) but from the increase in unpleasant emotion you anticipated if you did not help (guilt or shame). Interpreted in this way, your helping was not altruistic. It was an instrumental egoistic response. You acted to reduce the other person's distress because that reduced your own distress.

If we allow that apparently altruistic helping may be no more than an instrumental egoistic response, and we believe that we must, then there is no clear empirical evidence that empathic emotion leads to altruistic motivation to help. The difficulty in

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providing evidence is, of course, that egoism and altruism are motivational concepts, and we cannot directly observe motivation, only behavior. If we are to provide empirical evidence that empathic emotion leads to altruistic motivation, we need to identify some point at which the egoistic and altruistic interpretations differ at a behavioral level. If no such point can be found, then we must conclude that the claim that empathy evokes altruistic motivation is of no real theoretical significance.

Conceptual Distinction Between Egoism and Altruism

In an attempt to find a point of behavioral difference, it is important, first, to be clear about the points of conceptual difference. Therefore, let us be explicit about what we mean by egoistic and altruistic motivation for helping. As we shall use the terms, a person's helping is egoistic to the degree that he or she helps from a desire for personal gain (e.g., material rewards, praise, or self-esteem) or a desire to avoid personal pain (e.g., punishment, social castigation, private guilt, or shame). That is, *egoistically motivated helping is directed toward the end-state goal of increasing the helper's own welfare*. In contrast, a person's helping is altruistic to the degree that he or she helps from a desire to reduce the distress or increase the benefit of the person in need. That is, *altruistically motivated helping is directed toward the end-state goal of increasing the other's welfare*.

This conceptual distinction between egoism and altruism leads to three observations: (a) Helping, as a behavior, can be either egoistically or altruistically motivated; it is the end-state goal, not the behavior, that distinguishes an act as altruistic. (b) Motivation for helping may be a mixture of altruism and egoism; it need not be solely or even primarily altruistic to have an altruistic component. (c) Increasing the other's welfare is both necessary and sufficient to attain an altruistic end-state goal. To the degree that helping is altruistically rather than egoistically motivated, increasing the other's welfare is not an intermediate, instrumental response directed toward increasing one's

own welfare; it is an end in itself. Although one's own welfare may be increased by altruistically motivated helping (for example, it may produce feelings of personal satisfaction or relief), personal gain must be an unintended by-product and not the goal of the behavior. This conception of altruism and of the distinction between it and egoism seem quite consistent not only with Auguste Comte's (1875) initial use of the term but also with modern dictionary definitions, for example, "unselfish concern for the welfare of others."

Empirical Distinction Between Egoism and Altruism

Equipped with this conceptual distinction, we may turn to the problem of making an empirical distinction between egoistic and altruistic motivation for helping. As we have said, all we can directly observe is the behavior, helping. The challenge is somehow to use the behavior as a basis for inferring whether the motivation underlying it is egoistic or altruistic.

Batson and Coke (in press) have recently proposed a technique for doing this. Building on the work of Piliavin and Piliavin (Note 1), they point out that the effect on helping of a cost variable—the cost of escaping from the need situation without helping—should be different, depending on whether the bystander's motivation is egoistic or altruistic. If the bystander's motivation is egoistic, his or her goal is to reduce personal distress caused by seeing the other suffer. This goal can be reached either by helping, and so removing the cause of one's distress, or by escaping (physically or psychologically) and so removing contact with the cause; either behavior can lead to the desired goal. The likelihood that the egoistically motivated bystander will choose to help should, therefore, be a direct function of the costs associated with choosing to escape. These costs include the physical effort involved in escaping from the need situation (often minimal) and, more importantly, the feelings of distress, guilt, and shame anticipated as a result of knowing that the person in need is continuing to suffer. Thus, if the bystander were egoistically motivated and all other

variables were held constant, increasing the cost of escaping by, for example, preventing the bystander from leaving the scene of the accident and so making it hard to avoid thinking about the continuing distress of the unhelped victim should increase the rate of helping. Conversely, reducing the costs of escaping by, for example, making it easy for the bystander to leave the scene of the accident and thus avoid thinking about the victim's continuing distress should decrease the rate of helping.

If the bystander's motivation is altruistic, his or her goal is to reduce the other's distress. This goal can be reached by helping, but not by escaping. Therefore, the likelihood that the altruistically motivated bystander will help should be independent of the cost of escaping because escaping is a goal-irrelevant behavior. Increasing or decreasing the cost of escaping should have no effect on the rate of helping; the rate should remain as high when escape is easy as when it is difficult.

These predictions suggest a way of determining whether the motivation for helping is egoistic or altruistic. The motivation cannot be inferred from any single behavioral response, but it can be inferred from the *pattern* of helping responses presented in Table 1. To the extent that the motivation for helping is egoistic, the helping rate should be affected by the difficulty of escaping. The easier it is to escape continued exposure to the need situation, the lower the cost of escaping and the less chance of a bystander's helping. But to the extent that the motivation for helping is altruistic, the helping rate should be unaffected by the difficulty of escaping; helping should be just as high when escape is easy as when it is difficult.¹

Application to the Problem of the Motivation Resulting From Empathic Emotion

Now let us apply this general technique for discriminating between egoistic and altruistic motivation to the specific question of whether empathic emotion leads to altruistic motivation to help. If the motivation

Table 1
Rate of Helping When Difficulty of Escape is Varied and Motivation is Egoistic or Altruistic

Difficulty of escape	Type of motivation (level of empathic emotion)	
	Egoistic (low empathy)	Altruistic (high empathy)
Easy	Low	High
Difficult	High	High

associated with feeling empathy for the person in need is altruistic (the empathy-altruism hypothesis), individuals induced to feel a high degree of empathy should help regardless of whether escape is easy or difficult (column 2 of Table 1); individuals feeling little empathy should help only when escape is difficult (column 1). Thus, if empathy leads to altruistic motivation to help, one can relabel the columns in Table 1, as has been done in parentheses. If, however, the motivation to help resulting from empathic emotion is egoistic, as seems to be implied by those who speak of "empathic pain," helping in the high-empathy condition should be affected by the ease of escape. Then we would expect to observe two main effects: As in previous research, high empathy should lead to more helping than low empathy, presumably as a result of an increase in feelings of personal distress or in anticipated guilt or shame. And in each empathy condition difficult escape should lead to more helping than easy escape.

Note that the entire one-versus-three interaction pattern depicted in Table 1 is important if one is to provide evidence for the empathy-altruism hypothesis. If, for example, one were to compare the easy and difficult escape cells only in the column marked altruistic motivation (high empathy), the

¹ It is worth noting that another cost variable, the cost of helping, is frequently thought to be the key to altruism. If helping occurs when the cost of helping is high (at the extreme, when the helper's life is in danger), this is thought to be evidence of altruistic motivation. A little reflection shows that such an inference is unfounded, for even highly costly helping could easily be an instrumental egoistic response, motivated by a desire to avoid guilt or to attain praise and honor either in this life or an anticipated life to come.

altruistic prediction is for no difference in the rate of helping. Such a result could easily occur simply because the escape manipulation was too weak or the behavioral measure was insensitive. If, however, an escape manipulation has a significant effect on helping when a bystander feels little empathy but does not when a bystander feels much empathy, the evidence that empathic emotion evokes altruistic motivation is much stronger. Then the evidence cannot be dismissed as being the result of a weak escape manipulation or an insensitive measure.

It is also clear that one must be on guard for a possible ceiling effect. A ceiling effect in the high-empathy column could obscure the two-main-effect pattern that would be expected if the motivation were egoistic, making it look like the one-versus-three interaction that would be expected if the motivation were altruistic.

Present Research

We conducted two experiments to test the hypothesis that empathic emotion leads to altruistic motivation to help. As suggested by the preceding analysis, a 2×2 design was used in each. Subjects observed a young woman named Elaine receiving electric shocks; they were given an unanticipated chance to help her by volunteering to take the remaining shocks in her stead. Cost of escaping without helping was manipulated by making escape either easy or difficult. Subjects believed that if they did not take Elaine's place, either they would continue to observe her take the shocks (difficult escape condition) or they would not (easy escape condition). Level of empathic emotion (low versus high) was manipulated differently in the two experiments. Following the classic studies of Stotland (1969) and Krebs (1975), in Experiment 1 we used similarity information to manipulate empathy. In Experiment 2 we sought to manipulate empathy more directly through the use of an emotion-specific misattribution to a placebo. In both experiments, the empathy-altruism hypothesis predicted that helping responses would conform to the one-versus-three pattern depicted in Table 1.

Experiment 1

There is evidence (e.g., Hornstein, 1976; Krebs, 1975; Stotland, 1969) that people are more likely to identify with a person they perceive to be similar to themselves and, as a result, to feel more empathy for a similar than for a dissimilar other. In the clearest demonstration of this relationship, Krebs (1975) manipulated male subjects' perceptions of their similarity to a young man (an experimental confederate) prior to having them watch him perform in a roulette game in which he received money if the ball landed on an even number and an electric shock if the ball landed on an odd number. Similarity was manipulated by telling subjects that their responses to a personality test completed several days earlier indicated that they and the performer were either similar or different. In addition, subjects received information suggesting that the performer's values and interests were either similar or different from their own. Compared with subjects in the dissimilar condition, subjects who perceived themselves to be similar to the performer showed greater physiological arousal in response to his pleasure and pain, reported identifying with him to a greater degree, and reported feeling worse while waiting for him to receive shock. These subjects also subsequently helped him more. But it was not clear whether the motivation to help was egoistic or altruistic. To clarify this issue, we used a procedure similar to Krebs's but varied perceived similarity and difficulty of escape in a 2×2 factorial design.

Method

Subjects

Subjects were 44 female introductory psychology students at the University of Kansas participating in partial fulfillment of a course requirement. They were randomly selected from those who had completed a personal value and interest questionnaire, which formed the basis for the similarity manipulation, at a screening session held a few weeks earlier. Subjects were assigned to the four conditions of the 2 (easy versus difficult escape) \times 2 (similar versus dissimilar victim) design through the use of a randomized block procedure, 11 subjects to each cell. Four additional participants, one from each cell, were excluded from the design because they suspected Elaine was not actually receiving shocks.

Procedure

All subjects were tested individually by a female experimenter. On arrival, subjects were told that they would have to wait a few minutes for the arrival of a second subject, Elaine (actually a confederate). They were given an introduction to read while waiting:

In this experiment we are studying task performance and impression projection under stressful conditions. We are investigating, as well, whether any inefficiency that might result from working under aversive conditions increases proportionately with the amount of time spent working under such conditions.

Since this study requires the assistance of two participants, there will be a drawing to determine which role will be yours. One participant will perform a task (consisting of up to, but not more than, ten trials) under aversive conditions; the aversive conditions will be created by the presentation of electric shock at random intervals during the work period. The other participant will observe the individual working under aversive conditions. This role involves the formation and report of general attitudes towards the "worker" so that we may better assess what effect, if any, working under aversive conditions has upon how that individual is perceived.

After reading the introduction and signing a consent form, subjects drew lots for their role. The drawing was rigged so that they always drew the observer role.

Subjects were then escorted to the observation room and given more detailed instructions. They learned that they would not actually meet the worker but would instead observe her over closed-circuit television as she performed up to 10 2-min. digit-recall trials. At random intervals during each trial, the worker would receive moderately uncomfortable electric shocks. The instructions went on to explain that equipment limitations made it impossible to capture visually all of the worker's reactions and that this was a problem, since prior research suggested that nonverbal cues were important in assessing another person's emotional state. To compensate for this lost information, the worker would be connected to a galvanic skin response (GSR) monitor, which would be visible in the lower right-hand corner of the television screen. The level of arousal indicated on the monitor would enable the subjects to assess more accurately the worker's emotional response, and help them form an impression.

Difficulty of escape manipulation. To manipulate difficulty of escape without helping, the last line of the detailed instructions varied the number of trials that subjects expected to observe. In the easy-escape condition, subjects read: "Although the worker will be completing between two and ten trials, it will be necessary for you to observe only the first two." In the difficult-escape condition they read: "The worker will be completing between two and ten trials, all of which you will observe." All subjects were later to learn that Elaine agreed to complete all 10 trials, and they were given the chance to help her by trading places after the second trial. Therefore, in the easy-escape condition, subjects who did not help would not have to watch Elaine take

any more shocks; in the difficult-escape condition they would.

Similarity manipulation. After the subject finished reading the detailed instructions, the experimenter handed her a copy of the personal values and interest questionnaire administered at the screening session, explaining that this copy had been filled out by Elaine and would provide information about her that might be of help in forming an impression. Elaine's questionnaire was prepared in advance so that it reflected values and interests that were either very similar or very dissimilar to those the subject had expressed on her questionnaire. In the similar-victim condition, Elaine's responses to six items that had only two possible answers (e.g., "If you had a choice, would you prefer living in a rural or an urban setting?") were identical to those the subject had given; her responses to the other eight items were similar but not identical (e.g., "What is your favorite magazine?"). Answers: *Cosmopolitan* for the subject, *Seventeen* for Elaine; *Time* for the subject, *Newsweek* for Elaine). In the dissimilar-victim condition, Elaine's responses to the six two-answer items were the opposite of those the subject had given, and her responses to the other eight were clearly different (e.g., *Cosmopolitan* for the subject, *Newsweek* for Elaine).

The experimenter was blind to subjects' escape condition and to whether Elaine's questionnaire was similar or dissimilar. She remained blind to the similarity manipulation until after all measures were recorded, but she made herself aware of the escape manipulation just prior to presenting the opportunity to help Elaine. This was to allow her to remind the subjects how many more trials they would be observing if they did not help. Since the empathy-altruism hypothesis predicted that the two independent variables would interact, remaining blind to one independent variable was sufficient to rule out an experimenter-bias explanation (Rosenthal, 1966) for the predicted pattern of helping.

While the subject looked over Elaine's questionnaire, the experimenter left to see if Elaine had arrived. She returned to say that she had and that the subject could now begin observing her over the closed-circuit television. So saying, the experimenter turned on a video monitor, allowing the subject to see Elaine. Unknown to the subject, what she saw was actually a videotape.

Need situation. On the videotape, subjects first saw Elaine, a moderately attractive young woman, tell the research assistant (female) that she would complete all 10 of the digit-recall trials. As the assistant was going over the procedure, Elaine interrupted to ask about the nature of the electric shocks that were to be used. The assistant answered that the shocks would be of constant intensity and, although uncomfortable, would cause "no permanent damage." "You know if you scuff your feet walking across a carpet and touch something metal? Well, they'll be about two to three times more uncomfortable than that."

After GSR electrodes were attached to the first and third fingers on Elaine's nondominant hand and a shock electrode was attached to her other arm, the digit-recall trials began. The experimenter left subjects alone at this point. As the first trial progressed, Elaine's facial expressions, body movement, and the GSR monitor all indicated that she was finding the shocks extremely un-

pleasant. By midway through the second trial, her reactions were so strong that the assistant interrupted the procedure to ask if Elaine were all right. Elaine answered that she was but would appreciate having a glass of water. The assistant readily agreed to this request and went to get the water.

Manipulation check. During this 90-sec break, the experimenter reentered the observation room and gave subjects a brief questionnaire, ostensibly assessing their impression of Elaine thus far. The questionnaire included six 7-point trait rating scales (attractive, intelligent, competent, friendly, mature, cooperative). Subjects were also asked how likable Elaine was and how enjoyable they thought it would be to work with her. To check on their perceptions of her distress, subjects were asked, "In your opinion, how uncomfortable were the aversive conditions (random shocks) for the person in the working conditions experiment?" Finally, to check on the effectiveness of the similarity manipulation, they were asked, "How similar to you is the person in the working conditions experiment?" Responses to each of these four questions were on 7-point scales (1 = not at all; 7 = extremely). When subjects finished the questionnaire, the experimenter collected it and left.

Returning with the glass of water, the assistant asked Elaine if she had ever had trouble with shocks before. Elaine confessed that she had—as a child she had been thrown from a horse onto an electric fence. The doctor had said at the time that she suffered a bad trauma and in the future might react strongly to even mild shocks. (This information was provided to ensure that subjects would view Elaine's extreme reaction to the shocks as atypical and would not expect to find the shocks as unpleasant if they chose to take her place.) Hearing this, the assistant said that she did not think Elaine should continue with the trials. Elaine replied that even though she found the shocks very unpleasant, she wanted to go on: "I started; I want to finish. I'll go on . . . I know your experiment is important, and I want to do it." At this point, the assistant hit upon an idea: Since the observer was also an introductory psychology student, maybe she would be willing to help Elaine out by trading places. Elaine readily consented to the assistant checking about this possibility. The assistant said that she would shut off the equipment and go talk with the experimenter about it. Shortly thereafter, the video screen went blank.

Dependent measure: Helping Elaine. About 30 sec later, the experimenter entered the observation room and said:

First of all, let me say that you're under no obligation to trade places. I mean, if you would like to continue in your role as observer that's fine; you did happen to draw the observer role. If you decide to continue as the observer, ([easy-escape condition] you've finished observing the two trials, so all you need to do is answer a few questions about your impression of Elaine and you'll be free to go) ([difficult-escape condition] I need you to observe Elaine's remaining trials. After you've done that and answered a few questions about your impression of Elaine, you'll be free to go.). If you decide to change places with Elaine, what will happen is that she'll come in here and observe you,

and you'll do the aversive conditioning trials with the shocks. And then you'll be free to go.

What would you like to do? [Experimenter gets response from subject.] OK, that's fine. [If subject says she wants to trade places with Elaine, the experimenter continues.] How many trials would you like to do? Elaine will go ahead and do any of the eight remaining trials that you don't want to do. [Experimenter gets response.] Fine.

The experimenter then left, ostensibly to go tell the assistant what had been decided. In fact, she recorded whether the subject wanted to trade places and, if so, how many of the eight remaining trials she would do. This information provided the dependent measure of helping. Then the experimenter made herself aware of the subject's similarity condition.

Debriefing. The experimenter returned promptly and fully debriefed the subject. Subjects seemed readily to understand the necessity for the deception involved in the experiment, and none seemed upset by it. After debriefing, subjects were thanked for their participation and excused.

Results and Discussion

Effectiveness of the Similarity Manipulation

To check the effectiveness of the similarity manipulation, subjects were asked how similar the worker (Elaine) was to them. On the 7-point response scale, subjects in the similar-victim condition perceived Elaine to be more similar to themselves ($M = 5.09$) than subjects in the dissimilar-victim condition ($M = 2.69$), $F(1, 40) = 39.56$, $p < .001$. No other effects approached significance ($F_s < 1.20$). Similar but weaker patterns were found for two related items: ratings of Elaine's attractiveness and likability. Subjects in the similar-victim condition perceived Elaine to be more attractive ($M_s = 5.86$ versus 5.14), $F(1, 40) = 4.38$, $p < .05$, and more likable ($M = 5.14$ versus 4.23), $F(1, 40) = 5.06$, $p < .03$. For each of these items, no other effects approached significance ($F_s < 1.30$). These results suggested that the similarity manipulation was successful, although as might be expected, manipulating similarity did not just affect perceived similarity; it had some effect on perceived attractiveness and liking as well.²

² There were no reliable differences across conditions in ratings of how enjoyable it would be to work with Elaine (overall $M = 4.57$) or in ratings of her intelligence

Table 2
Proportion of Subjects Agreeing to Trade Places With Elaine in Each Condition of Experiment 1

Difficulty of escape condition	Similarity condition			
	Dissimilar victim		Similar victim	
	Proportion	M no. ^a	Proportion	M no. ^a
Easy	.18	1.09	.91	7.09
Difficult	.64	4.00	.82	5.00

Note. *n* = 11 in each condition.
^a Mean number of shock trials (from 0 to 8) that subjects agreed to take for Elaine (*MS*_e = 9.70, *df* = 40).

A formal check on the escape manipulation seemed impractical. It also seemed unnecessary, since subjects received the manipulation twice—once in their written instructions and again orally just prior to indicating whether they wished to help. Examination of debriefing notes indicated that, as expected, subjects were aware of their escape condition and its implications.

Perception of Elaine's Distress

As intended, subjects in all conditions perceived Elaine to be suffering. When asked on a 1–7 scale to indicate how uncomfortable the shocks were for her, subjects' modal response in each condition was 7 (extremely uncomfortable); the overall mean was 6.25. There were no reliable differences across conditions.

Relieving Elaine's Distress by Helping

The proportion of subjects in each experimental condition who offered to help Elaine by trading places is presented in Table 2.

(overall *M* = 4.23), friendliness (overall *M* = 5.18), maturity (overall 4.77), or cooperativeness (overall *M* = 5.45). On ratings of her competence, there was an unexpected, significant (*p* < .03) interaction; Elaine was perceived to be more competent in the easy-escape-similar-victim and the difficult-escape-dissimilar-victim conditions than in the other two conditions. Since there was no ready explanation for this interaction, it seemed best attributed to chance.

Following the procedure recommended by Langer and Abelson (1972) and Winer (1971, pp. 399–400), these dichotomous data were analyzed through analysis of variance by employing a normal approximation based on an arc sine transformation. The 2 × 2 analysis revealed a highly significant main effect for similarity, $\chi^2(1) = 11.69, p < .001$, qualified by a significant Escape × Similarity interaction, $\chi^2(1) = 4.19, p < .04$. The main effect for difficulty of escape did not approach significance, $\chi^2(1) = 1.34, p > .20$.

Inspection of the proportion of helping in each condition revealed that the interaction was of the form predicted by the empathy-altruism hypothesis; the proportion in the easy-escape-dissimilar-victim condition was much lower than in the other three conditions. To test the statistical significance of this predicted one-versus-three pattern, the rate of helping in this condition was contrasted with the rate in the other three conditions. This planned comparison revealed a highly significant difference, $\chi^2(1) = 14.62, p < .001$. Residual variance across the other three conditions did not approach significance, $\chi^2(2) = 2.60, p > .25$. Individual cell comparisons revealed that, as predicted, the proportion of helping in the easy-escape-dissimilar-victim condition was significantly lower than the proportion in each of the other three conditions (*z*s ranging from 2.27 to 3.87, all *ps* < .015, one-tailed). Comparisons among the other three conditions revealed no reliable differences (all *z*s < 1.60).

With one exception, an identical pattern of significant effects emerged from analysis of variance and planned comparisons on the number of shock trials subjects in each condition volunteered to take for Elaine. The one exception was that the number of trials was significantly lower in the two difficult-escape conditions (pooled) than in the easy-escape-similar-victim condition, $t(40) = 2.25, p < .03$, two-tailed.

These results were quite consistent with the empathy-altruism hypothesis; they were not consistent with the view that empathy simply increases egoistic motivation to help. In the dissimilar-victim condition, where empathic emotional response to Elaine's distress was expected to be relatively low and,

according to the empathy-altruism hypothesis, the motivation to help was expected to be primarily egoistic, the difficulty of escape manipulation had a dramatic effect on helping. When escape was easy, subjects were not likely to help, presumably because a less costly way to reduce any personal distress caused by watching Elaine receive shock was to answer the experimenter's final questions and leave. When escape was difficult, subjects were likely to help, presumably because taking the remaining shocks themselves was less costly than sitting and watching Elaine take more.

In the similar-victim conditions, however, where empathic emotional response to Elaine's distress was expected to be relatively high and, according to the empathy-altruism hypothesis, the motivation to help should be at least in part altruistic, difficulty of escape had no effect on subjects' readiness to help. Presumably, because their concern was to reduce Elaine's distress and not just their own, they were very likely to help, even when escape was easy.

Nor could this pattern of results be dismissed as an artifact of a ceiling effect in the difficult-escape-similar-victim condition. Although the proportion of helping in both similar-victim conditions was high, there was a nonsignificant trend for the proportion to be higher under easy than under difficult escape ($z = -.63$). This was not what would be expected if a ceiling effect were operating. Moreover, a ceiling-effect explanation was even less plausible for the number of shock trials subjects volunteered to take, since the mean response on this measure in the difficult-escape-similar-victim condition was far from the upper endpoint of the scale. And on this measure too there was a nonsignificant trend for the number of trials to be larger under easy than under difficult escape, $t(44) = -1.58$.

Finally, internal analyses provided an opportunity to check on a possible alternative explanation for the low level of helping in the easy-escape-dissimilar-victim condition: derogation of Elaine. If derogation were inhibiting helping in this condition, we would expect positive correlations between the helping measures and the ratings of Elaine's

attractiveness and likability. But these correlations appeared to be, if anything, negative ($r_s = -.08$ to $-.31$). There was, then, no evidence that derogation was inhibiting helping in this condition. And covariance analyses indicated that derogation could not account for the pattern of helping across experimental conditions. Removing the effects of perceived attractiveness or of likability on either likelihood or amount of helping, the predicted one-versus-three pattern of helping responses remained highly significant (all $F_s \geq 13.63$, all $p_s < .001$).

Overall, the results of Experiment 1 seemed to conform closely to the one-versus-three pattern that, according to Table 1, would be expected if increased empathic emotion led to altruistic motivation; they did not conform to the two-main-effect pattern that would be expected if increased empathy led to egoistic motivation. Still, although Stotland (1969) and Krebs (1975) had provided rather strong evidence that a similarity manipulation like the one used in Experiment 1 manipulated empathic emotion, the manipulation was indirect. Therefore, a second experiment was conducted in which we sought to test the empathy-altruism hypothesis by manipulating empathic emotion more directly.

Experiment 2

Based on the results of four different studies, Batson and Coke (in press) have suggested that two qualitatively distinct emotional states are elicited by witnessing another person in distress: *empathic concern*, made up of emotions such as compassion, concern, warmth, and softheartedness, and *personal distress*, made up of emotions such as shock, alarm, disgust, shame, and fear. It seemed to us that in the absence of a similarity manipulation, watching Elaine take shocks should elicit a reasonably high degree of both of these emotional states. And, generalizing from the work on the misattribution of dissonance arousal (Zanna & Cooper, 1974; Zanna, Higgins, & Taves, 1976), we thought that if subjects could be induced to misattribute one of these emotions to some other source, such as a placebo, they would

perceive their response to Elaine's distress to be predominated by the other. That is, if they attributed their feelings of empathic concern to the placebo, they should perceive their responses to Elaine to be predominantly personal distress. If they attributed their feelings of personal distress to the placebo, they should perceive their response to Elaine to be predominantly empathic concern. So if empathic emotion leads to altruistic motivation to help, crossing such a misattribution manipulation with a difficulty-of-escape manipulation, like the one used in Experiment 1, should again produce the one-versus-three pattern of helping responses depicted in Table 1. Subjects induced to attribute their empathic concern to the placebo should attribute relatively little empathic concern to watching Elaine suffer, and as a result, their motivation to help should be predominantly egoistic. This egoistic motivation should be reflected in less helping under easy than difficult escape. In contrast, subjects induced to attribute their personal distress to the placebo should attribute a relatively large amount of empathic concern to watching Elaine, and as a result, their motivation to help should be predominantly altruistic. This altruistic motivation should be reflected in a lack of effect for the escape manipulation; helping should be relatively high under both easy and difficult escape.

Method

Subjects

Subjects were 48 female introductory psychology students at the University of Kansas participating in partial fulfillment of a course requirement. They were assigned to the four conditions of the 2 (easy vs. difficult escape) \times 2 (personal distress vs. empathic concern as response to watching Elaine) design through the use of a randomized block procedure. Twelve subjects were assigned to each cell. Five additional participants were excluded from the design because they did not believe that the placebo capsule contained a drug, and six more were excluded because they suspected Elaine was not actually receiving shocks. Although this relatively high suspicion rate (19%) was regrettable, it was not unexpected in an experiment using a placebo manipulation. Fortunately, there was no evidence of reliable differences across conditions in the number of participants excluded for suspicion, and data analyses, with all suspicious participants included, revealed the same, although somewhat weaker, pattern of significant effects

reported below. Therefore, the relatively high suspicion rate did not appear to provide an alternative explanation for the results.

Procedure

The procedure was the same as in Experiment 1, except for three changes. First, instead of using a similarity manipulation, level of empathic response to Elaine's distress was manipulated by having subjects misattribute either empathic concern or personal distress to a placebo administered in the context of a separate study. Second, time constraints arising from employing two studies restricted the number of shock trials subjects watched and were given a chance to take for Elaine. This restriction led to a minor wording change in the escape manipulation and the use of only a dichotomous (yes-no) measure of helping. Third, since the change in the number of trials necessitated creation of a new videotape, two new actresses played the parts of Elaine and the research assistant. Except for minor changes required by the procedural differences, the script for the videotape was the same as in Experiment 1.

Introduction. The introduction subjects read on arrival informed them that we were running two studies concurrently because one involved a time delay and the other required the assistance of an observer. Through a drawing, subjects were assigned to the former study—the effect of Millentana on short-term memory—and Elaine was assigned to the second study—task performance under aversive conditions.

As a rationale for the first study, subjects read, "One of the enzymes in the drug Millentana is believed to increase the level of serotonin in the brain. This modification . . . results in greater ability for short-term memory recall." To test the possible effect of Millentana on short-term memory, subjects were to complete two brief memory tasks, one before and one after taking a capsule containing Millentana. Since it would take approximately 25 min. for the Millentana to be completely absorbed into their system, and absorption was necessary before the second memory task could be administered, subjects were to serve as the observer for the aversive conditions study in the interim.

Emotional response manipulation. After completing the first memory task, subjects were given a capsule containing Millentana (actually a corn starch placebo). Before taking the capsule, all subjects were informed on a typed statement that in addition to its brief effect on short-term memory, the oral form of Millentana we were using had a side effect. Subjects in the personal-distress condition read:

Prior to total absorption, Millentana produces a clear feeling of warmth and sensitivity, a feeling similar to that you might experience while reading a particularly touching novel. You should begin to notice this side effect sometime within the first five minutes after ingestion. The side effect will disappear within twenty-five minutes, when the drug is totally absorbed.

Subjects in the empathic-concern condition read the same statement, except that the side effect of Millentana was described as "a clear feeling of uneasiness and discomfort, a feeling similar to that you might experience

while reading a particularly distressing novel." These manipulations were based on the assumption that subjects who were led to misattribute feelings of empathic concern to Millentana would perceive their emotional response to watching Elaine to be primarily personal distress, whereas those led to misattribute feelings of personal distress to Millentana would perceive their emotional response to Elaine to be primarily empathic concern. All subjects signed the statement to indicate that they had read and understood the information about the side effect of Millentana. The experimenter remained blind to the emotional response manipulation until debriefing.³

Escape manipulation. After ingesting the Millentana capsule, subjects were given instructions for their role as observer in the aversive conditions study. As in Experiment 1, the last sentence of these instructions contained the escape manipulation. In the easy-escape condition subjects read: "Although the worker will be completing two trials, you will be observing only the first." In the difficult-escape condition they read: "The worker will be completing two trials, both of which you will observe."

Need situation. As in Experiment 1, subjects watched over closed-circuit television as Elaine reacted very strongly to the moderately uncomfortable shocks. At the end of the first trial, the assistant interrupted the procedure and, at Elaine's request, went to get her a glass of water.

Manipulation check. During this break, subjects were given a list of 28 emotion adjectives and asked to circle any that they were experiencing as a result of taking the Millentana capsule. The list contained 10 adjectives that in previous research (cf. Batson & Coke, in press) had tended to load together on an empathic concern factor (sympathetic, kind, compassionate, warm, softhearted, tender, empathic, concerned, moved, and touched) and 10 that had tended to load together on an orthogonal, personal distress factor (alarmed, bothered, disturbed, upset, troubled, worried, anxious, uneasy, grieved, and distressed). Not only did completion of this form provide a partial check on the effectiveness of the emotional response manipulation, it also served to remind subjects of the possibility that any emotion they were experiencing could be due, in part, to the Millentana capsule.

Dependent measure: Helping Elaine. When the assistant returned, the conversation began about Elaine's reaction to the shocks. As in Experiment 1, it led up to the idea that the subject might be willing to help Elaine by trading places. Shortly thereafter, the experimenter entered the observation room and presented the subject with the opportunity to help. Paralleling the procedure in Experiment 1, in the easy-escape condition subjects were reminded that if they did not help they would not have to watch Elaine's second trial; in the difficult-escape condition subjects were reminded that they would. The dependent variable was whether or not subjects volunteered to trade places with Elaine for the second trial.

Response to Elaine and her need. After subjects indicated whether they wished to help, they were given a four-item questionnaire assessing their reactions to observing Elaine. The first two questions asked how much "uneasiness" and "warmth and sensitivity" ob-

serving the task performance study caused them to experience (1 = none; 9 = a great deal). The last two questions asked how likable the worker was and how uncomfortable the aversive conditions (random shocks) were for her (for both questions, 1 = not at all; 9 = extremely).

Debriefing. On completion of this questionnaire, subjects were fully debriefed. As with Experiment 1, they seemed readily to understand the necessity for the deception involved, and none seemed upset by it. After debriefing, subjects were thanked for their participation and excused.

Results and Discussion

Perception of Elaine's Distress

Ratings of how uncomfortable the shocks were for Elaine suggested that subjects in all conditions perceived her to be in considerable distress. On the 9-point response scale, the modal response in the difficult-escape-personal-distress condition was 8; in each of the other three conditions, it was 9. The overall mean was 8.07, with no reliable differences across conditions.

Effectiveness of the Emotional Response Manipulation

Perceived emotional response to Millentana. To check the effectiveness of the emotional response manipulation, subjects were first asked to circle adjectives describing the emotions that they were experiencing as side effects of Millentana. Because there were large individual differences in the number of adjectives circled, the most appropriate index of the type of emotion experienced seemed to be a simple classification: If a subject circled more empathic concern than personal distress adjectives, she received a score of 1; if she circled an equal number, she received a score of 0; and if she circled fewer,

³ Unlike the typical placebo-misattribution manipulation, in which some people are told that the placebo will arouse them and some are told that it will not or some are led to expect side effects relevant to the arousal they are experiencing and others to expect irrelevant side effects, all subjects in Experiment 2 were told that the placebo would produce relevant arousal. What was manipulated was the *nature* of the arousal the placebo would produce—empathy or distress. Because the nature rather than the amount of arousal was being manipulated, a no-side-effect condition of the sort employed as a control when amount of arousal is manipulated was not appropriate for our design.

she received a score of -1 . A 2×2 analysis of variance on this measure revealed only one reliable effect, a main effect for the emotional response manipulation, $F(1, 44) = 14.82, p < .001$. As intended, subjects in the personal-distress condition reported experiencing a relative predominance of empathic concern emotions as a result of taking the Millentana capsule ($M = .21$), whereas subjects in the empathic-concern condition reported experiencing a relative predominance of personal distress emotions ($M = -.46$). Thus, the emotional response manipulation appeared to produce the intended perceptions of side effects. But did it produce reciprocal perceptions of emotional response to Elaine's distress?

Perceived emotional response to Elaine's distress. Subjects' ratings of the amount of uneasiness and of warmth and sensitivity caused by observing the aversive conditions experiment provided indices of their emotional response to Elaine's distress. It was expected that subjects in the two emotional response conditions would not differ in the average amount of emotion attributed to watching Elaine, but they would differ in the nature of the emotion. To provide an index of the overall amount of emotion experienced, ratings of uneasiness and of warmth and sensitivity were averaged. (Across the entire design, these ratings were positively correlated: $r[46] = .45, p < .01$, presumably reflecting individual differences in emotionality or in response set.) A 2×2 analysis of variance revealed no reliable differences on this index (overall $M = 4.59$).

To provide an index of the nature of the emotion experienced, a difference measure was created by subtracting the rating of uneasiness from the rating of warmth and sensitivity. Analysis of this index revealed only one reliable difference, a main effect for the emotional response manipulation, $F(1, 44) = 5.92, p < .02$. As intended, this main effect was a mirror image of the main effect on emotion experienced as a side effect of the placebo. Subjects in the distress condition reported a predominance of uneasiness in their response to observing Elaine ($M = -1.50$); subjects in the empathy condition reported more warmth and sensitivity ($M = .21$). Moreover, within-cell correla-

tions between this index and the index of type of emotion experienced as a side effect of Millentana provided no evidence for differences independent of the experimental manipulations; none of the within-cell correlations differed reliably from zero. Looking separately at the ratings of uneasiness and of warmth and sensitivity, the main effect on the index of nature of emotional response was found to be primarily a result of a difference in reported warmth and sensitivity ($M = 3.46$ and 5.08 for the distress and empathy conditions, respectively), $F(1, 44) = 5.41, p < .03$; the difference in reported uneasiness was not reliable ($M_s = 4.96$ and 4.88 , respectively). There were no other reliable differences on either emotional response item.

It appeared, then, that the emotional response manipulation was effective. Although there was no difference across conditions in the total amount of emotion reported as a result of observing Elaine, there was a difference in the relative amount of empathic emotion reported. Significantly more empathy was reported in the empathic-concern than in the personal-distress condition. Moreover, unlike the similarity manipulation used in Experiment 1, the emotional response manipulation produced no reliable differences across conditions in how likable Elaine was perceived to be; she was seen as moderately likable in all conditions (overall $M = 6.04$ on the 9-point response scale).

As in Experiment 1, it was not considered practical or necessary to have a formal check on the escape manipulation. Debriefing notes again indicated that subjects were aware of their escape condition and its implications.

Relieving Elaine's Distress by Helping

Since the subjects reported less empathy as a result of witnessing Elaine's distress in the distress condition than in the empathy condition, it was possible to test the empathy-altruism hypothesis once again. The proportion of subjects offering to help Elaine in each experimental condition of Experiment 2 is presented in Table 3. As in Experiment 1, these dichotomous data were analyzed through analysis of variance and planned comparisons by employing a normal

approximation based on an arc sine transformation. A 2×2 analysis revealed only one significant effect, an Escape \times Emotional Response interaction, $\chi^2(1) = 6.10, p < .02$. As predicted by the empathy-altruism hypothesis, this effect was due to the proportion of helping being lower in the easy-escape-distress condition than in the other three conditions. A planned comparison revealed that this predicted one-versus-three pattern was highly significant, $\chi^2(1) = 5.96, p < .02$; residual variance across the other three conditions did not approach significance, $\chi^2(2) = 1.94, p > .40$. Individual cell comparisons revealed that the proportion helping in the easy-escape-distress condition differed significantly from the proportion in the easy-escape-empathy condition ($z = 2.62, p < .01$, one-tailed), and the difficult-escape-distress condition ($z = 2.12, p < .02$, one-tailed), but not from the difficult-escape-empathy condition ($z = 1.24$). Comparisons among the other three conditions revealed no reliable differences (all z s ≤ 1.38).

These results were again quite consistent with the empathy-altruism hypothesis. In the distress conditions, where motivation was assumed to be egoistic, the rate of helping was significantly lower under easy than under difficult escape. In the empathy conditions, where motivation was assumed to be at least in part altruistic, the rate of helping remained high, even when escape was easy. In addition, the correlation between helping and the index of nature of emotional response was significantly more positive in the easy-escape conditions, $r_{pb}(24) = .27$, than in the difficult-escape conditions, $r_{pb}(24) =$

$-.32, z = 1.97, p < .05$, two-tailed. This indicated a more positive association between relative empathy and helping in the easy- than in the difficult-escape conditions, as would be predicted by the empathy-altruism hypothesis.

And again there was no evidence of a ceiling effect in the difficult-escape-empathy condition. Instead, in the empathy conditions there was again a nonsignificant trend for the rate of helping to be higher under easy than under difficult escape ($z = -1.38$). Moreover, the rate of helping in the difficult-escape-empathy condition was near the midpoint of the response scale. Nor was there any evidence that derogation could account for the pattern of results. Paralleling results of Experiment 1, within-cell correlation and covariance analyses revealed no evidence of derogation in the easy-escape-distress condition.

General Discussion

As we noted at the outset, the hypothesis that empathic emotion produces truly altruistic motivation contradicts the egoistic assumption of most, if not all, current theories of motivation. Because egoism is a widely held and basic assumption, it is only prudent to require that the evidence supporting altruism be strong before this hypothesis is accepted.

To the degree that the conceptual analysis and resulting predictions presented in Table 1 provide an adequate framework for an empirical test of truly altruistic motivation, the two experiments reported here seem to make an initial step toward providing such evidence. The results of the two experiments were highly consistent; in each, conditions assumed to produce relatively high empathic response to a person in distress led to helping regardless of whether escape without helping was easy or difficult. In contrast, conditions assumed to produce relatively low empathic response led to helping only when it was difficult to escape without helping. This was precisely the pattern of results predicted by the hypothesis that empathic emotion evokes altruistic motivation to see another's need reduced.

Still, two experiments are not many on

Table 3
Proportion of Subjects Agreeing to Trade Places With Elaine in Each Condition of Experiment 2

Difficulty of escape condition	Subject's dominant emotional response to Elaine's distress	
	Personal distress	Empathic concern
Easy	.33	.83
Difficult	.75	.58

Note. $n = 12$ in each condition.

which to base so radical a change in our view of human motivation, especially when they have at least two limitations. First, in each experiment the person in need was female, and because it seemed likely that subjects would be more likely to empathize with a same-sex individual, only female subjects were used. Although there is evidence that females report experiencing quantitatively more empathy than males (Hoffman, 1977), we know of no evidence nor any a priori reason why empathy, when experienced, would elicit qualitatively different kinds of motivation in males than in females. But future research should look more closely at the motivational consequences of empathy for males. Second, both experiments came out of the same laboratory—ours. Confidence in the hypothesis that empathic emotion elicits truly altruistic motivation would certainly be strengthened by converging evidence from other laboratories, especially ones with perspectives different from our own.

It may be, then, too early to conclude that empathic emotion can lead to altruistic motivation to help. But if future research produces the same pattern of results found in the experiments reported here, this conclusion, with all its theoretical and practical implications, would seem not only possible but necessary. For now, the research to date convinces us of the legitimacy of *suggesting* that empathic motivation for helping may be truly altruistic. In doing so, we are left far less confident than we were of reinterpretations of apparently altruistically motivated helping in terms of instrumental egoism.

Reference Note

1. Piliavin, J. A., & Piliavin, I. M. *The Good Samaritan: Why does he help?* Unpublished manuscript, University of Wisconsin, 1973.

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