

Original Papers

Polish Psychological Bulletin
 2012, vol. 43(3), 191-198
 DOI - 10.2478/v10059-012-0021-6

Katarzyna Szczucka*

Attraction at first fright? What Dutton & Aron really demonstrated almost 40 years ago

Almost four decades have passed since Dutton and Aron (1974) published their classic article in JPSP in which they present the results of three studies. According to interpretations of the results done by the authors, the sufficient condition of obtaining the effect of increased sexual attraction toward the object (an attractive woman) – which must be present shortly after or while waiting to become an aversive stimulus – is the induction in the subjects of a strong autonomic arousal. This can be done via crossing a high suspended bridge or anticipating the receipt of strong electric shocks. However, the results of reanalysis do not allow such a conclusion. In the article the author presents the results of secondary analysis and lists methodological, theoretical and interpretative incoherences.

Key words: *effect of arousal on attraction; misattribution effect; Dutton and Aron; misinterpretations; errors*

*For I am the first and the last.
 I am the honored one and the scorned one.
 I am the whore and the holy one.
 I am the wife and the virgin.
 (Nag Hammadi manuscript)*

Dutton and Aron's article published in 1974 undoubtedly belongs to the classics of social psychology papers. References to this paper appear in most textbooks (cf. Aronson, Wilson, & Akert, 2010; Hogg & Vaughan, 2011; Kenrick, Neuberg, & Cialdini, 2010; Rogers, 2003). Almost each (I hope) undergraduate psychology student from all over the world, if awoken at midnight and asked: "What did Dutton and Aron demonstrate in the bridge experiment?" would answer: "They showed that when male participants crossed the dangerous, high, unstable bridge to meet a young, attractive woman, they misinterpreted arousal as a attraction." What if we would, next, ask him or her: "How did you know that?", arguably he or she would answer: "Dutton and Aron demonstrated such effect in experiments they had conducted." Is that conclusion really correct? What in fact did the two researchers show?

Donald Dutton and Arthur Aron conducted three studies – two field studies and one laboratory experiment.

The field studies are most known – an attractive female or (it is not obvious, if equally attractive) male confederate approached lonely walking young male subjects in the two conditions: (1) as they crossed the experimental, five-foot-wide, 450-foot-long, 230-foot-high unstable bridge, under which a wild river flowed; (2) as they crossed the control – a stable, 10-foot-high bridge. When the subjects crossed one of these bridges, a confederate asked them to fill in a questionnaire consisting of, among others, TAT-picture questions. After filling in the questionnaire the interviewer asked the subjects, if they wanted to receive further information about the study. If the answer was affirmative, the confederate wrote down her or his name and the phone number. The authors generated and experimentally confirmed the following research hypothesis: men who crossed the experimental Capilano Canyon Suspension Bridge would misattribute aversive arousal induced by walking only through this bridge as a sexual attraction to the female confederate. The dependent variable measures were: (1) TAT responses as an indicator of sexual imagery; (2) behavioral data i.e. accepting the phone number and making a phone call.

In this article I would like to offer arguments for the above mentioned conclusion presented by Dutton and

* University of Social Sciences and Humanities, Wrocław Faculty

Aron. I would like to present many inaccuracies from the original studies carried out by the authors by implementing the following distinction: (1) theoretical inconsistencies; (2) methodological errors; (3) statistical errors; (4) interpretative inconherences.

I. Theoretical inconsistencies

The authors' explanation of arousal – attraction link bases on the two-factor theory of emotion (Schachter & Singer, 1962) is a part of the attributional tradition in psychology. According to the assumptions of the theory sine qua non conditions of experiencing an emotion (in this case – sexual attraction) are: ambiguous actual source of arousal and situational clue, which gives a name to the kind of emotion a person is experiencing at present. Another theory, i.e. the excitation transfer theory (Zillmann, Katcher, & Milavsky, 1972), which Dutton and Aron should have known, contains similar assumptions. Meanwhile, in both field studies the source of experienced arousal was unambiguous and more salient compared to the attractive confederate. In opposition to these theoretical assumptions the authors claim that: “a sexual attraction-strong emotion link may occur even when the emotions are unambiguous” (op. cit., p. 511).

II. Presentation of the results of the reanalyses

1. First field study

1.1. Methodological errors

The first question that should be asked is whether this study should be named “experiment” and whether the authors really had made “arousal manipulation”? In fact this research was for certain neither an “experiment” (or “field experiment”; op. cit., p. 513) nor there was a manipulation of arousal level done by the confederates (cf. “arousal manipulation”; op. cit., p. 512). Moreover, there were neither randomization, constitutional for an experiment, nor exactly experimental manipulation. Incidentally, the only manipulation which the authors had done was the confederates sex manipulation, therefore the study was an example of a quasi-experiment (cf. Shadish, Cook, & Campbell, 2002). The weak side of the design is limited internal validity – the fact that between presumed cause (a sort of a bridge and, as a consequence, presence or absence of arousal) and the result (presence or absence of the reinterpretation of arousal as sexual attraction) association is observed. Yet, there is no indication that there is a causal relation between them (ibid.). We could not state explicitly that the results of the study were only an implication which out of the two bridges the subjects crossed.

The research results came from a non-random assignment of the subjects, hence – obtained intergroup differences in the measurement of the dependent variable

indicators could come from individual pre-existing differences of the subjects (e.g. the motives of sensation seeking, subjective perception of eustress and/or distress or perception of optimal arousal level) i.e. selection bias (ibid.). Meanwhile, Dutton and Aron in a gratuitous manner assumed that for the subjects who had crossed the experimental bridge this experience was unequivocally aversive and was subjectively interpreted as a fear: “They experience a strong emotion (fear)” (op. cit., p. 511).

Another doubt refers to the abundance of the groups of the subjects and why they were not equinumerous.

The authors do not present how long it had been since the participants crossed the experimental bridge until they were inquired by the confederates: “As subjects crossed either the control or experimental bridge, they were approached by the interviewer” (op. cit., p. 511). From the later empirical results point of view (cf. Cantor, Zillmann, & Bryant, 1975) the fact whether 1, 5 or 10 minutes had passed, is a very significant parameter. Of course, Dutton and Aron could have not known the above mentioned results, nonetheless no information about indicators of this parameter should be handled as a methodological malpractice.

Another methodological error refers to the extent of generalization of the results, hence the external validity. The participants were only “males visiting either of two bridge sites who fit the following criteria: (a) between 18 and 35 years old and (b) unaccompanied by a female companion” (op. cit., p. 511). In the first field study there were no such conditions when either an attractive male confederate approached lonely walking females or an average/unattractive female confederate and an average/unattractive male confederate approached both male and female subjects.

1.2. Statistical errors and the results of the reanalyses

In the first field study Dutton and Aron (1974) present, let us recall, three types of dependent variable indicators: (1) TAT responses; (2) behavioral data: both DV 1 – accepting phone number and DV 2 – subsequent calling. As we remember there were four conditions: male vs. female confederate and two kinds of bridges: experimental and control.

Female confederate conditions

The female confederate accosted in total 66 subjects, out of whom 45 agreed to fill in the questionnaire. In the experimental conditions 23/33 males filled in the questionnaire whereas in the control conditions 22/33. There were 7 questionnaires which were unusable hence to the further empirical investigation remained 20 in experimental conditions and 18 in control conditions, respectively.

DV: compliance with a request

The authors did not present the results of the analysis for dependent variable which was compliance with a request to fill in a questionnaire. I presume that this indicator of

dependent variable is salient and consistent with theoretical assumptions and the compliance with a request in the experimental conditions should be greater compare to control conditions. If the arousal in the experimental conditions, according to the research hypothesis, was misattributed by the subjects as a sexual attraction to the female confederate, approached males should more often comply after crossing the experimental bridge. Meanwhile the results of the analysis I have conducted show that compliance indicators for both groups were similar: 69,7% vs. 66,7%, odds ratio (OR) = 1,15; 95% Wald confidence interval (CI) = 0,41-3,24; $\chi^2(1, N = 66) = .07, p > .7$.

DV 1: accepting phone number

In the experimental conditions 18/23 subjects accepted phone number of the female confederate, in the control conditions 16/22 did. Dutton and Aron do not present the results of the analysis for this indicator of the dependent variable, these are nonsignificant: (78.3% vs. 72.7%), odds ratio (OR) = 1.35; 95% Wald confidence interval (CI) = 0.345-5.284; $\chi^2(1, N = 45) = .19, p > .6$. If the subject in the experimental conditions misattribute their arousal (experienced *hic et nunc!*) they should more often (i.e. statistically significant, with the big enough effect size) accept phone number offered by an attractive, young woman compared to the control conditions. Meanwhile this relation did not occur – confidence interval contains “1” which means that the intergroup differences are not the result of experimental manipulation, in other words – the link between the variables is weak or nonexistent (cf. Agresti, 2007).

DV 2: latter calling

Out of the group of 18 subjects who crossed the experimental bridge 9 phoned, while among the control conditions – 2 out of 16 subjects called: (50% vs. 12.5%), odds ratio (OR) = 7; 95% Wald confidence interval (CI) = 1.22-40.125; $\chi^2(1, N = 34) = 5.44, p < .02$. The odds ratio is 7, which means that the chance of achieving success (i.e. phoning) is sevenfold more likely than encountering failure (ibid.) and that the association between variables is strong (cf. Haddock, Rindskopf, & Shadish, 1998). The above results, as we can see, are statistically significant and nontrivial but what in fact did they prove? A key role in the interpretation of the results plays the temporal parameter, i.e. how long has it been since the subjects had met the confederate and latter phoning – I will relate to it in section 1.3.

Male confederate conditions

The male confederate accosted in total 93 subjects, out of whom 45 agreed to fill in the questionnaire. There were 5 questionnaires which were unusable hence to the further empirical investigation remained 20 in experimental conditions and analogical number in control conditions.

DV: compliance with a request

In the experimental conditions 23/51 subjects complied, in the control conditions – 22/42: 45.1% vs. 52.4%, odds

ratio (OR) = .75; 95% Wald confidence interval (CI) = .33-1.69; $\chi^2(1, N = 93) = .49, p > .4$.

DV 1: accepting phone number

In the experimental group 7 out of 23 accepted phone number of the male confederate, in the control group – 6 out of 22, (30.4% vs. 27.3%), odds ratio (OR) = 1.16; 95% Wald confidence interval (CI) = .321-4.247; $\chi^2(1, N = 45) = .05, p > .8$.

DV 2: latter calling

2 out of 7 subjects who had crossed the experimental bridge called to the male confederate (28.6%), among control subjects – 1 out of 6 (17%), odds ratio (OR) = 2; 95% Wald confidence interval (CI) = .134-29.81; $\chi^2(1, N = 13) = .26, p > .6$.

1.3. Interpretative incoherences

Donald Dutton and Arthur Aron claim that they carry out “arousal manipulation” (op. cit., p. 512) among the group of other 30 men at the age 18-35 to investigate the effectiveness of the procedure of inducing fear: >>Fifteen males on the experimental bridge were asked, „How fearful do you think the average person would be when he crossed this bridge?” The mean rating was 79 on a 100-point scale where 100 was equal to extremely fearful. Fifteen males on the control bridge gave a mean rating of 18 on the same scale ($t = 9.7, df = 28, p < .001$, two-tailed). In response to the question „How fearful were you while crossing the bridge?” experimental-bridge males gave a rating of 65 and control-bridge males a rating of 3 ($t = 10.6, p < .001, df = 28$, twotailed). Hence, it can be concluded that most people are quite anxious on the experimental bridge but not on the control bridge<< (op. cit., p. 512). Needless to say, the above conclusion is invalid. First – there was no manipulation (cf. section 1.1.), second – the results came from the incidental sample which consists of 15 participants in each condition hence the generalization of these results to the all actual participants of the field (i.e. $N = 159$) study is unjustified. There is no information about who asked the questions – attractive (wo)man, average/unattractive (wo)man? Further, it is unclear why the scale had only 100 points instead of 101 points, with “0” point which would indicate the state of experiencing no fear. What is more – why the authors assume that the thirty males would experience only fear, but not, for instance, non-specific arousal or excitation or both? Explanation that: “To present suspicion, no checks on the arousal of experimental subjects could be made” (op. cit., p. 512) is unclear.

Another error pertains to presented numerical amount of the subjects: “A total of 85 subjects were contacted by either a male or a female interviewer” (op. cit., p. 511), in the meantime in the study took part 159 subjects. Dutton and Aron claim that a total of 12 questionnaires were unusable (5 from a male and 7 from a female confederate). The question I would ask is how many unusable questionnaires out of the 12 “were incomplete or written in a foreign

language” (op. cit., p. 512) and why these subjects were the participants classified to the next measurements (accepting phone number and phoning)?

The authors refer that after filling in the questionnaire in the both research conditions the interviewer “invited each subject to call, if he wanted to talk further” (op. cit., p. 512). Meantime, in another paper Artur Aron claims that the confederates informed the subjects that they could “phone her that evening” (Lewandowski & Aron, 2004, p. 362). From the original paper we could not know: (1) When the subjects of both sexes could/should phone – the same evening? Next day? Someday?; (2) If all of the participants phoned more or less in the same time? The authors de facto did not control this indicator of the dependent variable.

Another issue is that the authors interpret phoning after – we do not know how long, we could only presume – a period of time as an indicator of misattribution of arousal and, as a result, the sexual attraction that only those participants who had crossed the dangerous bridge felt. “On the assumption that curiosity about the experiment should be equal between control and experimental groups, it was felt that differential calling rates might reflect differential attraction to the interviewer” (op. cit., p. 512). Meanwhile, this ascertainment is irrelevant with the theoretical assumptions (Schachter & Singer, 1962; Zillmann et al., 1972) – surely the subjects were not aroused at that moment of calling, anyways not “because of the confederate”. Consequently – is that indicator really “evidence for heightened sexual attraction under conditions of high anxiety”?

2. Second field study

Donald Dutton and Arthur Aron claim in the discussion of the results of the first study that these effects may be the consequence of differences between both populations of subjects. To eliminate possible alternative explanations the authors in the next study created different research design, namely: (1) in both – experimental and control – conditions the subjects were approached only by a female confederate; (2) the participants came from “the same subject population” (op. cit., p. 513), i.e. in both groups solely males took part (again at the age 18 – 35) that crossed The Capilano Suspension Bridge.

2.1. Methodological errors

The participants were accosted either at least 10 minutes that elapsed after the moment of crossing the bridge (i.e. the control conditions, that is – according to the hypothesis – no arousal is experienced), or after unspecified by the authors period of time. This time the authors analogically to the description of the procedure of the first study, do not inform about the temporal parameter in the experimental conditions. Meanwhile that parameter is crucial (cf. Zillmann et al., 1972; Cantor, Zillmann, & Bryant, 1975).

Other methodological objections are replication of those listed in section 1.1.

2.2. Statistical errors and the results of the reanalyses

As with the first study the dependent variables were twofold: (1) TAT responses; (2) behavioral data (accepting a phone number and later calling).

DV: compliance with a request

The authors again did not analyze the data addressing to compliance with a request. The analysis that I have conducted shows meanwhile that the compliance proportions were similar in both conditions: 25 out of 34 complied in the experimental group, in the control – 25 out of 35, (73.53% vs. 71.43%), odds ratio (OR) = 1.11; 95% Wald confidence interval (CI) = .39-3.20; $\chi^2(1, N = 69) = .04, p > .8$.

DV 1: accepting a phone number

Dutton and Aron in respect to TAT responses claim: “In the experimental group, 25 of 34 males who were approached agreed to fill in the questionnaire. In the control group, 25 out of 35 agreed” (op. cit., p. 514). In the next paragraph, in which they present the results of the behavioral data, we could read that: “In the experimental group, 20 of the 25 subjects who agreed to the interview accepted the interviewer’s phone number. In the control group, 19 out of 23 accepted” (op. cit., p. 514). Meanwhile, according to the dissection of the paper in the control conditions a phone number accepted not 19 out of 23 but 19 out of 25 subjects – certainly this kind of proportion had the authors do analysis of the results of the first study (cf. section 1.2. or table 1; op. cit., p. 513). The authors again do not present the results of the analysis of this dependent variable, which are: in the experimental group phone number accepted 80% of the subjects, in the control group – 76%; odds ratio (OR) = 1.26; 95% Wald confidence interval (CI) = .33-4.84; $\chi^2(1, N = 50) = .12, p > .7$. Hence as we can see, the results from the first study in respect to this dependent variable were replicated. The results are nonsignificant, effect size is small and the confidence interval includes the null hypothesis. On the grounds of these, doing correct analyses we should explicitly say that the male subjects in the two field studies done by Dutton and Aron did not take the female confederate’s phone number more often when they crossed the suspension bridge.

DV 2: later calling

The implication of the error mentioned above is the necessity of conducting the secondary analysis of the data collected by Dutton and Aron based on correct proportions, i.e. the second behavioral data – phoning. As we can read: “In the experimental group, 13 out of 20 called, while in the control group, 7 out of 23 phoned ($\chi^2 = 5.89, p < .02$)” (op. cit., p. 514). Meanwhile, in the low arousal conditions (i.e. control conditions) 7 out of 19 phoned the confederate, not 7 out of 23, inasmuch “In the control group, 19 out of 23 accepted [the phone number]” (op. cit., p. 514). Hence:

this analysis conducted by Dutton and Aron was false, the correct analysis results are as follows: in the experimental conditions 13 out of 20 called, in the control conditions – 7 out of 19, (65% vs. 37%), odds ratio (OR) = 3.18; 95% Wald confidence interval (CI) = .86-11.78; $\chi^2(1, N = 39) = 3.09, p = .08$. As we can see, there is a trend, but there is no statistical significance. Further – the confidence interval is wide, as the results of the analysis indicate, the sample was nonrepresentative for the population. The confidence interval contains “1”, which means that the association between the variables is weak or nonexistent, accidental. It was not the result of the used manipulation (cf. Agresti, 2007; Haddock et al., 1998), which confirms the p value.

2.3. Interpretative incoherences

Bearing in mind both: the results of the secondary analysis and methodological inconsistencies presented earlier it is hard to agree with the ascertainties that: „the behavioral result of Experiment 1 was also replicated” (op. cit., p. 514) and “Experiment 2 enables the rejection of the notion of differential subject populations as an explanation for the control-experimental bridge differences for female interviewers in Experiment 1” (op. cit., p. 514). The results of the first study, indeed, were replicated, but only in the case of accepting the phone number – both results did not to come close the trend limit ($p > .6$ in the first study and $p > .7$ in the second), but in the case of the second dependent variable, i.e. later calling – the results were not replicated ($p < .02$ in the first study and $p = .08$ in the second).

According to present standards, replications are the basis of experimental psychology, psychology as a science (cf. APA Publication Manual, 2010; Cumming, 2008; Wojciszke, 2006). As Geoff Cumming claims (2008), the p value is not the valid baseline of any statistical inferences, and in fact this value which comes from only one study could not say much if in replication this value (the magic “ $p < .05$ ”) will be repeated. The measures on which an experimental psychologist should base are, independent from the sample size, confident intervals and effect size (cf. also APA Publication Manual, 2010).

The questions we should ask in this place are – what de facto did Dutton and Aron measure in the two field studies and what did they, conclusively, demonstrate? Well, they show doubtlessly that men at the age 18-35, who were aroused (in aversive or nonaversive way), display more contents of sexual connotation than nonaroused males at the same age (these assessments based on competitive judges procedure, scores ranged from 1 to 5; cf. op. cit., p. 511). In the female interviewer conditions (first study) males who crossed the experimental bridge got higher scores ($M = 2.47$) than men who crossed the control bridge ($M = 1.41$), $t = 3.19, p < .01, df = 36$, two-tailed (cf. op. cit., p. 512). In the same conditions, i.e. when the interviewer was a female confederate (second study) the results were similar: $M = 2.99$ (experimental bridge) and $M = 1.92$ –

control bridge ($t = 3.07, p < .01, df = 36$, two-tailed (cf. op. cit., p. 514). There were no significant differences in the conditions when the subjects were accosted by a male interviewer on both bridges ($t = 0.36, ns$; op. cit., p. 512). It is thought-provoking why the authors in both studies, in the TAT variable context, used the two-tailed tests, having formulated a directional hypothesis. Another question is if the researchers would have obtained the results if the interviewer was an average/unattractive woman. All other indicators of dependent variables – i.e. behavioral data – do not confirm the hypotheses Dutton and Aron formulated.

The pretext for conducting the real experiment (in the original paper – »Experiment 3«) were the problems with gestalt. As we can read: »The gestalt created by the experimental situation may have made the interviewer appear more helpless or frightened, virtually a „lady in distress.« Such would not be the case in the control situation” (op. cit., p. 514). These doubts, in my opinion, are irrelevant – the authors at one point wrote that the female confederate was in each case blind to the experimental hypothesis (cf. op. cit., p. 511 and p. 514), at the other that “more stable nonverbal forms of communication (such as eye contact) could not be controlled without cueing the female interviewer to the experimental hypothesis” (op. cit., p. 514). Why should (only) the female confederate in the conditions in which the subjects experienced autonomic arousal be perceived like a helpless or frightened person? What kind of theoretical justification could explain this artificial problem? In my opinion this attempt to find an alternative explanation of the obtained results and the attempt to show, via a laboratory experiment that supposed alternative explanation (the problem of gestalt) was for certain abolished is from theoretical and interpretative point of view irrelevant.

3. Laboratory experiment

The participants of the first experiment de facto were 80 students, all were volunteers. The female confederate (cosubject) knew that the study concerned sexual attraction, but did not know the research hypothesis. According to the deception, the aim of the experiment was to show the association between electric shocks and learning. There were three types of the dependent variables: DV 1: measurement of the experienced anxiety (on a 5-point scale); DV 2: twofold measurement of sexual attraction to the female confederate (this variable was not measured in the dyad male – male): wanting to date and wanting to kiss the female confederate (assessment per analogy on the 5-point scale, but the average score from two items was measured); DV 3: like in the two prior studies – TAT responses.

3.1. Statistical incoherences

DV 1: Measurement of experienced anxiety

According to the original paper: “In conditions where the subject anticipated receiving a strong shock, subjects reported significantly more anxiety than in conditions where the subject anticipated receiving a weak shock ($t = 4.03, p < .001, df = 39$, one-tailed). In conditions where the subject anticipated receiving a strong shock with the female cosubject present, subjects reported significantly less anxiety than in a control condition ($n = 20$), where two male subjects were run ($t = 2.17, p < .025, df = 19$, one-tailed). No significant differences in the subject’s anxiety occurred as a function of the confederate receiving a strong versus a weak shock” (op. cit., p. 515).

Table 1 Reported anxiety in experimental conditions

Subject expects:	Female confederate to get strong shock	No female get weak shock	No female confederate
strong shock	(1) 3.17	(2) 3.05	(5) 3.8
weak shock	(3) 2.42	(4) 2.28	(6) ?

In each cell $n = 20$ (On the basis of the data presented by Dutton and Aron, 1974, p. 515).

Let us start with the incoherence relating to the total number of the participants. Earlier, as we remember, the authors claimed, that a total number of subjects in the third experiment was 80 (cf. op. cit., p. 514), but in the Table 2 (op. cit., p. 515 – a copy of Table 1 in this paper) we could see the cell “no female confederate” and the information, that in each cell “ $n = 20$ ”, but... we have here five cells, ergo: the total number of participants was 100, and, as a consequence, we do not have here the design 2×2 , but the design 2×2 with the isolated control group.

Dutton and Aron make the two intergroup comparisons with the use of t-test as follows: (1) the participants expect to receive a strong shock (cells: 1 and 2) vs. a weak shock (cells: 3 and 4); (2) the subjects expect to receive a strong shock when the cosubject (experimenter female confederate) is present (cells: 1 and 2) vs. the control conditions, i.e. the dyad male (participant) – male (cosubject, an experimenter male confederate), cell 5. If so then why – accordingly – in the first comparison $df = 39$, instead of 78, and in the second $df = 19$, instead of 58? Further – why do the authors use in this case one-tailed comparisons, if were in the first and the second study there two-tailed comparisons used? Unfortunately, we could not run the secondary analysis basing on the data where standard deviations values are not known.

3.2. Methodological errors

The idea of the laboratory experiment, the main hypothesis, which appealed to the gestalt, as I mentioned above, is arguable. I do not see the logical continuity between the studies. The fact that there was only an attractive female

cosubject could be included among the unquestionable infringements

3.3. Interpretative incoherences

DV 2: Sexual attraction scores

The results of this dependent variable measurement were as follows: regardless of whether the female confederate would expect strong or weak shocks, the participants experienced greater sexual attraction to the female cosubject only in the conditions, in which they expected to receive a strong shock.

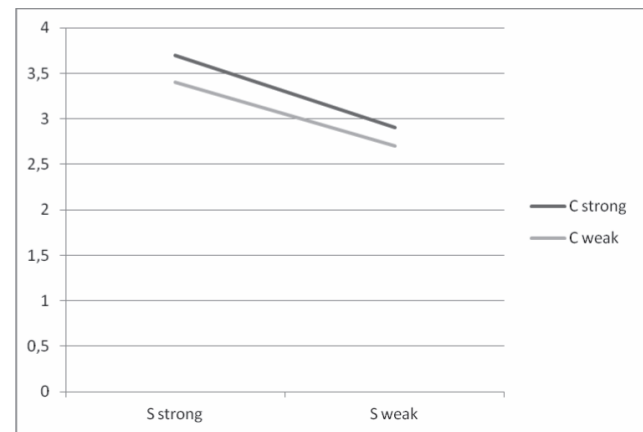


Figure 1
Attraction scores
The power of electric shock
C – confederate (cosubject)
S – subject

How do the authors interpret this, statistically significant (i.e. the main effect of the strength of electric shocks which only the participants expected) – $F = 22.8, p < .001$ (op.cit., p. 516)? “Hence, the lady-in-distress effect on attraction did not seem to appear in this study” (op. cit., p. 516). This interpretation appears to be invalid because of, what I have mentioned earlier, lack of a rational and theoretical justification of such a hypothesis (gestalt, “lady in distress”) put to the empirical investigation and because of the presence of so many methodological incorrectnesses. Further – the alternative theoretical explanations observed in the old studies and the laboratory experiment seem to be more valid (cf. e.g. later proposition by Kenrick & Cialdini, 1977).

DV 3: TAT responses

An analysis of indicators of this dependent variable shows that only the interaction was statistically significant ($F = 4.73, p < .05$; op. cit., p. 516), the main effect of the strength of electric shocks which the subject expected hits the trend ($F = 4.22, p = .07$; op. cit., p. 516).

There are lots of problems in the interpretation of obtained results in the theoretical context proposed by the authors, i.e. the attributional paradigm. What kind of association could there be between the results of the laboratory experiment (gestalt) and the results obtained from

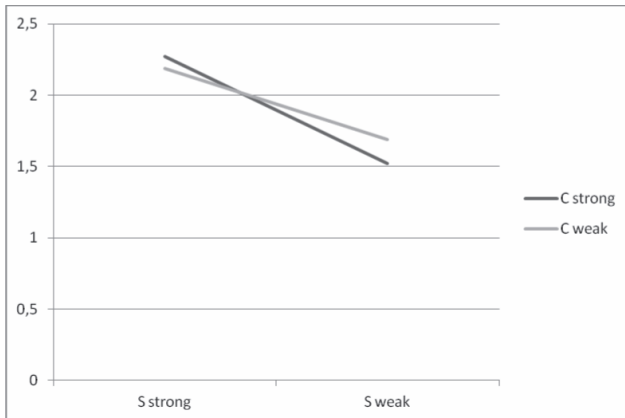


Figure 2
 Attraction scores
 The power of electric shock
 C – confederate (cosubject)
 S – subject

the measurement of the DV 2? There, as we remember, the authors showed the existence of the statistically significant main effect of the strength of electric shocks anticipated by the subjects, regardless of whether the female confederate expected strong or weak shocks. Here the main effect did not occur ($p = .07$) but we can observe the trend. Basing on the present methodological standards this result should rather “statistically upset”, than “statistically hush” (cf. Cumming, 2008).

General discussion

To sum up – what then did Dutton and Aron really demonstrate in 1974? What are the authors’ interpretations and what are the facts?

1. “The strong result of Experiment 3 supports the notion that strong emotion per se increases the subject’s sexual attraction to the female confederate” (op. cit., p. 516). This interpretation is strongly invalid, considering that the main effect of the DV 2 had been not replicated in regard to DV 3, on the other hand, the experimental design consists of many methodological errors (vide section 3.2.).

2. “The results of these studies would seem to provide a basis of support for an emotion – sexual attraction link” (op. cit., p. 516). In the light of the secondary analyses I had conducted the conclusion is invalid. The association exists, as the authors claim, even when the participants phone the female confederate after a dozen or so hours, when they are not affected of the past arousal state?

3. “The theoretical implications (...) provide additional support in favor of the theoretical positions from which the original hypothesis was derived: the Schachter and Singer (1962) tradition of cognitive labeling of emotions” (op. cit., p. 516) and later: “When subjects anticipated receiving a strong shock and the female confederate was present during the anxiety manipulation, subjects reported significantly

less fear than when no potential sexual object was present” (op. cit., p. 517). This is evident overinterpretation (section 3.2. and 3.3.) – the authors contort the assumptions of the Schachter and Singer theory (cf. section I.); why should the participants reinterpret experienced nonspecific arousal which was the function of the presence of such salient and unambiguous factors as: anticipated receiving a strong electric shock or crossing unstable, narrow and high bridge, as it arose because of seeing an attractive woman, and, as a consequence – a sexual attraction? Schachter and Singer (1962) claim explicitly that the misattribution effect occurs only if the real cause inducing arousal is ambiguous.

4. “However, regardless of the interpretation of the mechanics of this link, the present research presents the clearest demonstration to date of its existence” (op. cit., p. 517). Meanwhile the authors did not demonstrate the existence of this association, and the constation “regardless of the interpretation” is astonishing.

5. Dutton and Aron did not unequivocally show that the participants of the first and the second study experienced anxiety – the confederates leading the study did not ask the subjects how frightened they were, in order to “prevent suspicion” (op. cit., p. 512). As I mentioned earlier – according to this assumption in the laboratory experiment the measurement of the experienced anxiety before measuring the DV 2 and DV 3 “suspicion” should not occur. Interesting from this point of view is if the scores of experienced anxiety level would be the same if the measurement was made at the end, instead of at the beginning of the experiment.

6. The conditions in the field studies and the laboratory experiment were nonequivalent in the aspect of both: evaluation of the arousal and the anticipation or removal of the source of the arousal. First – the source of the arousal should be evaluating in a positive way (e.g. watching a comedy), a neutral (e.g. exercise) or unequivocally in a negative way (e.g. expecting to receive electric shock). Second – in the field studies the cause of the arousal had been removed (we could not ascertain explicitly as we do not have data concerning whether all the subjects experienced negative arousal). In the laboratory experiment – the cause would just occur (in the conditions of anticipating a strong shock – the cause was unequivocally aversive). The two research conditions are quite different also in terms of induction of affiliation motives.

7. Self-selection of the participants in the first and second studies rules out any generalization of these results.

8. Further for the important role of the replication in the experimental social psychology it should be mentioned that the effect Dutton and Aron obtained in the laboratory experiment (i.e. the male participants perceived as more attractive the female cosubject in the conditions of awaiting for a strong shock as compared with this participants who anticipated to receive a weak shock, which were “mere tingle, in fact some subjects describe it as enjoyable”

(op. cit., p. 515) was not replicated neither by Kenrick, Cialdini, & Linder (1979) nor by Riordan & Tedeschi (1983).

The heuristic function of the results obtained by Dutton and Aron is incontrovertible. The three studies which the authors conducted, apart from the correctness, interpretational and methodological validity – from the present point of view – were *the first* unusually influential (as we can read in the manuscript from Nag Hammadi in the motto) series of research exploring a fascinating phenomenon of the influence of arousal on attraction (cf. Foster et. al., 1998), which were, I hope, not *the last*.

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