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It really comes as a surprise to read a discussion about the group polarization effect, a field that goes back so many years (Rodrigo & Ato, 2002, 2002a; Kiers, 2002). Usually the publication rate follows an epidemic spread curve (Nowakowska, 1973), which means we should by now be immune from this bacillus called the “group polarization effect”. The recent discussion, however, is a good example of the tendencies in our world of research: After more than 40 years of research in this area, two young colleagues are able to correctly point out the “lack of fit between the definition and the statistical analysis of the group polarization phenomenon” (2002, p. 5). As someone who started his own research in this area more than 30 years ago (Witte, 1971, 1971a) I feel obliged to join the discussion from a more fundamental and historical angle.

Traditionally our approach is to detect effects and then list them in our textbooks, e.g., the Ringelmann-effect, the autokinetic effect, the Asch conformity effect, Milgram’s obedience effect, Latane´s bystander effect, shared view effect, Koehler effect, blocking effect, group polarization effect, and many more. We are now in a situation where one of these effects is being questioned due to a fundamental error in the evaluation methods that were employed. Perhaps this deficit is much more general than we assume, and may apply to many more effects we are familiar with.

What defines the level of measurement?

If we accept that it is the theory and the hypothesis derived from the theory that define the level of measurement then it is easy to reach a consensus in many areas: generally, there are no quantitatively specified hypotheses to be tested and nominal data are sufficient to test these specific hypotheses using the appropriate statistical tests. Using interval data to test a qualitative hypothesis could lead to errors. On the other hand, if we are able to use quantitative data we can improve the accuracy of our hypotheses on account of the greater precision that interval or ordinal data provide. The gap between the quantitative empirical data level and the qualitative theoretical level, combined with statistical tests based on interval data (t-test, analysis of variance, Pearson-product-moment correlation, etc.), can be seen as the reason behind many empirical misunderstandings, when attempting to verify qualitative statistical hypotheses (different from zero or greater than zero are the statistical alternative hypotheses mostly used to corroborate our theories). Why do we use measurement procedures on an interval level when testing qualitative hypotheses? The answer is twofold: 1. We are much more familiar with these methods. 2. The observation units, i.e., the sample size, can be much smaller if we use these procedures instead of chi-squared statistics or other methods of discrete multivariate analysis to test a hypothesis, because of the different power of the statistical tests.

Why do we ignore the quantitative information obtained by measurements on interval scales to construct quantitative hypotheses? In general, the hypotheses based on a simple qualitative effect, which should be tested against a random result, are less precise than an ordinal hypothesis with the prediction of a rank order, or a quantitative hypothesis with the prediction of an amount. An increase in precision depends, of course, on research done in the past, which enables the scientific community to make more accurate predictions by comparing and combining the results of other studies. On the one hand, we accept the fact that we can measure social phenomena quantitatively; on the other hand, we ignore this information for the sake of theory construction. And at the end of a long path of research into

the field of group polarization we are confronted with the statement: “Polarization is first and foremost a categorical phenomenon” (Rodrigo & Ato, 2002a). So much effort resulting in such a simple effect which even becomes isolated from all the others?

A few remarks on some inconsistencies in the commentary of Rodrigo and Ato

As I am forced to make this article as brief as possible I am not able to touch on all aspects, only those I consider to be severe inconsistencies. If we concentrate on figure 1. (p. 6), then especially in the case C) the authors implicitly assume a scale with a natural origin, an absolute scale. The value zero on the Likert-scale is not a theoretically relevant position to categorize groups into different qualities. There is only an interval scale and the theoretically relevant origin can be adapted to the data. (On the original probability scale used in the early risky-shift research it was not the 50%:50% scale value that was used to differentiate between the risky-effect and the cautious-effect, it was the 60%:40% point on a scale from 0%:100% to 100%:0%).

The second point of these illustrated examples in figure 1 (p. 6) is the influence that regression effects have on the mean if the group means before discussion are so extreme that there is no chance of becoming more extreme on the given scale.

The most important point, however, is the mixture of quantitative evaluation, used to get a description of the single groups by using the mean of individual reactions in the small group, and the qualitative interpretation of the polarization effect. All the problems given in figure 1 are comparable to the lower level, if the reaction represented by the figure refers to a single individual changing his or her mind after group-discussion. The arithmetic mean is only meaningful if we accept a quantitative measurement of individual reactions. This then poses the question why we are able to measure individual reaction on an interval scale but have to stop measuring reactions quantitatively if we combine groups. The only consequence is to prove a qualitative hypothesis by using nominal data, as the research process does not allow

for greater accuracy of the hypothesis through the formulation of a quantitative hypothesis as to the amount of change. A consistent method of proving the qualitative hypothesis is the use of nominal data and the model of social decision schemes (Davis, 1973). Using this approach Laughlin and Earley (1982) managed to prove the polarization effect. They dichotomized the reaction scale into a risky and cautious reaction and examined the reaction of each single group after discussion. Using nominal data there is no mean but only a distribution of reactions within the group. These different distributions of individual reactions in groups with a specific number of members, say 5, (5:0, 4:1, 3:2, 2:3, 1:4, 0:5) provide the baseline to test the qualitative polarization hypothesis. The main result was: risk (caution) supported wins. Thus, a minority of two is able to sway a majority of three in the predicted direction. This result, however, depends on the content of the items and is only valid for items that allow for a clear, pronounced shift in opinion. This is a consistent test of the qualitative hypothesis, with nominal data, that groups can change their mind in a direction which was not supported by the majority.

The justification of a consistent quantitative approach

At the very beginning of my research in this area I hoped to be able to predict group polarization quantitatively. Initially, of course, I had to find out whether something like a choice-shift effect existed at all. I therefore constructed new items because the original questionnaire was not uni-dimensional and the means of the items before discussion varied from more risky to more cautious reactions. I discovered that it was possible to find a significant cautious and risky-shift effect and that I was able to produce this effect by controlled arguments (Witte, 1971, 1971a). In this study (based on my diploma thesis) I was also able to predict the amount of change after group discussion by the mean of the individual reactions before discussion. The next question was why the subjects chose different mean reactions depending on the content of the items. Could I predict quantitatively the mean reactions by the central values attributed to the choice-dilemma problems? Amazingly, this too was possible (Witte & Arez, 1974). These results were replicated with a

different sample (Witte & Lutz, 1982). The next question was whether the arguments in the uncontrolled discussion could predict the quantitative amount of change (Witte & Lutz, 1982). The result was that the arguments measured quantitatively on the risk-scale were less extreme than the mean risk levels of the groups. Thus, a quantitative prediction of the change resulting from the **"riskiness"** of arguments exchanged during the discussion was not possible. For this reason a qualitative process was employed. There is a well-known relationship between confidence and extremity. If confidence increases by the exchange of arguments which are socially shared by the group members, then a more pronounced internal reaction can be justified. Furthermore, the content of the discussion is totally determined by socially shared values and by the individual reactions of group members before the discussion. There is no new information which could evoke a more extreme reaction, only the social support of the individual position by the other group members. This is an assumption which contradicts the persuasive arguments hypothesis, and has been corroborated in other experiments. The next point was the attempt to predict quantitatively the shift-effect using the mean of the individual reactions. I collected data from many different studies using the risk-scale and determined the values of a regression line between means before and after discussion. With the help of this regression line I was able to fix the socially shared value of new items and new samples. I could predict quantitatively the average position of any item's social value on the risk-scale by knowing the mean of the individual reaction of a control group, without having to take into account the discussion of the choice-dilemma item. I was then able to formulate a quantitative theory to predict the amount of change in group discussion, trying to integrate the isolated effects into a theory with a quantitative prediction (Witte, 1990, 1996).

The general assumption was that the social value of a choice dilemma item rested on the general orientation of each group member before took place discussion. During the discussion they find a compromise between the specific reactions in the small group and the individual reactions before discussion. The arguments exchanged have the sole function of corroborating this compromise. They have no influence on the shift of opinion but strengthen

the certainty of each group member that his or her opinion is correct and this increase of the individual certainty was followed by an extremization of the reactions on the risk-scale.

I believe that group polarization is more than just a categorical phenomenon. Research has developed in such a way that we have access to findings which could be used for a more precise formulation of results in a strictly quantitative way based on interval data. It is only in this sense that I disagree with the main position of Rodrigo and Ato (2002). Their critical comments are much more general than they expected themselves, mainly as a result of the inconsistencies between the level of measurement and the theoretical level which is to be tested.

References

Davis, J.H. (1973). Group decision and social interaction: A theory of social decision schemes. *Psychological Review, 80*, 97-125.

Kiers, H.A.L. (2002) Categorization is not the only way: a comment on Rodrigo and Ato. *European Journal of Social Psychology, 32*, 19-26.

Laughlin, P.R. & Earley, P.C. (1982). Social combination models, persuasive arguments theory, social comparison theory, and choice shift. *Journal of Personality and Social Psychology, 42*, 273-280.

Nowakowska, M. (1973). Epidemical spread of scientific objects: At attempt of empirical approach to some problems of meta-science. *Theory and Decision, 3*, 262-297.

Rodrigo, M.A., & Ato, M. (2002) Testing the group polarization hypothesis by using logit models. *European Journal of Social Psychology, 32*, 3-18.

Rodrigo, M.A., & Ato, M. (2002,a) Polarization is first and foremost a categorical phenomenom: a comment on Kiers: *European Journal of Social Psychology, 32*, 27-34.

Witte, E.H. & Arez, A. (1974). The cognitive structure of choice dilemma decisions. *European Journal of Social Psychology, 4*, 313-328.

Witte, E.H. & Lutz, D.H. (1982). Choice-shift as a cognitive change? In: H. Brandstätter, J.H. Davis, & G. Stocker-Kreichgauer (Eds.) *Group decision making*. (215-233). London: Academic Press.

Witte, E.H. (1971) Das "risky-shift"-Phänomen: Eine kritische Untersuchung der bestehenden Hypothesen. (The risky-shift-phenomenon: a critical inspection of the existing hypotheses.) *Psychologie und Praxis, 15*, 104-117

Witte, E.H. (1971,a) Das "risky-shift"-Phänomen: Eine kritische Untersuchung der Dimensionalität von hypothetischen Entscheidungssituationen. *Psychologie und Praxis, 15*, 22-26.

Witte, E.H. (1990). Social influence: A discussion and integration of recent models into a general situation theory. *European Journal of Social Psychology, 17*, 403-429.

Witte, E.H. (1996). The extended group situation theory (EGST): Explaining the amount of change. In: E.H. Witte & J.H. Davis (Eds.), *Understanding group behavior (Vol.1): Consensual action by small groups* (pp. 253-291). Mahwah: Erlbaum.

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