Statistical treatment of multivariate constructs in social psychology

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Statistical treatment of multivariate constructs in social psychology

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Contents

Introduction

The present document comprises the manuscripts „Reconsidering the ‚relative' in relative ingroup prototypicality" and "Attitude ambivalence or statistical artifact? Multivariate constructs require multivariate analyses", both of which have been submitted to peer-reviewed psychological journals. While each manuscript is self-contained and deals with a separate area of substantive research, the methodological critique and proposal is identical. This introduction distills and integrates the methodological aspects of these manuscripts and was written for the reader who is going to read (or has read) both manuscripts. In the following, I will summarize the methodological argument underlying both manuscripts and point to other constructs in social psychology where it also applies.

Statistical treatment of multivariate constructs in social psychology

The main goals of the present work are to identify confounds resulting from the current practice of analyzing multivariate constructs as index variables and to suggest an alternative procedure that avoids these confounds. Therefore, I begin by explaining the concepts *confounds*, *multivariate constructs* and *index variables*, which are central to my argument.

Confounds

According to its Latin roots (*confundere*), the verb *to confound* literally means *to pour together, mingle, mix,* and figuratively, it means *to make indistinct or unrecognizable* (Glare, 1969). Psychologists typically use the term *confound* in causal inference situations. For instance, the so-called "Mozart effect" (Rauscher, Shaw, & Ky, 1993) – enhanced spatial-temporal performance after exposure to music composed by Mozart – has been criticized for confounding Mozart music with participants' preferences. For participants who prefer a short story by Stephen King over Mozart, the "Mozart effect" does not obtain after exposure to Mozart music but after exposure to the King story (Nantais & Schellenberg, 1999). As this example shows, confounds call into question the label of an effect, creating a threat to the validity of the conclusions that researchers draw from their research. Confounds often arise in quasi-experiments, and it is well known how they can be eliminated or reduced in scope by experimental design or statistical analysis (e.g., Reichardt, 2006; Shadish, Cook, & Campbell, 2002).

Likewise, confounds arise at the more basic step of measuring psychological constructs. The very idea of measurement implies unidimensionality (Thurstone, 1928). In the context of psychological measurement, confounds refer to construct-irrelevant contamination such as socially desirable responding (e.g., Paulhus, 2001). As in causal inference situations, it is well known how measurement confounds can be eliminated or reduced in scope either by measurement design, or by explicitly modeling the relationship between a construct and a measure as in latent variable modeling, or by statistically controlling for the confound if it is known.

In contrast, the confounds addressed by my work must be called more fundamental for two different reasons. First, they would still threaten the validity of a researcher's conclusions even if the researcher were to dispose of perfectly unidimensional measures and the causal nature of the relationships between the constructs under investigation were beyond doubt. Second, as will become clear in the next section, the confounds discussed below are also more consistent with the literal meaning of *pouring together* different things. But of course they share with the confounds mentioned above the basic implication for the conduct of science: They call into question the validity of a label for an effect (or more generally, statistical relationship) and spur intellectual curiosity as to what their possible remedies might be.

Multivariate constructs

The confounds discussed below can arise in the study of multivariate constructs. For present purposes, I call multivariate constructs those constructs that locate a person (or any unit of analysis) in a psychological space defined by more than one dimension (although measures of each dimension may be truly unidimensional). This definition includes the constructs of relative ingroup prototypicality (RIP, Manuscript #1) and attitude ambivalence (AA, Manuscript #2) but is clearly broad enough to invite a closer look at other social psychological constructs that I did not specifically investigate but will briefly discuss below.

RIP is a multivariate construct because it positions a person in psychological space according to his or her judgments about the typicality of an ingroup and an outgroup. RIP is assumed to increase to the extent that an ingroup is associated with higher prototypicality for a superordinate social category that is shared with the outgroup, *and* to the extent that the outgroup is associated with lower prototypicality for the superordinate category (Mummendey & Wenzel, 1999; Waldzus, Mummendey, Wenzel, & Weber, 2003). For instance, it has been found that Germans' attitudes toward people from Eastern European countries are less positive, the more they perceive Germans to be relatively more prototypical for Europeans in general than Eastern Europeans (Ullrich, Christ, & Schlüter, 2006).

AA is a multivariate construct because it positions a person according to his or her degree of positivity and negativity toward an attitude object. AA is assumed to increase to the extent that (among other things) positivity *and* negativity toward the attitude object increase (Kaplan, 1972; Priester & Petty, 1996; Thompson, Zanna, & Griffin, 1995). For instance, a central theme of 80's and 90's research on intergroup relations in the United States was the notion that many European Americans hold ambivalent attitudes toward African Americans. According to Katz and Hass (1988), many European Americans feel sympathy with African Americans as a group that has been and continues to be discriminated against, but they also blame them for not doing enough to help themselves. These dual perceptions are assumed to derive from the conflicting ideologies of humanitarianism-egalitarianism and the individualistic Protestant work ethic.

Index variables of multivariate constructs

It is a natural tendency of language to reify and simplify empirical observation. In scientific language, it is doubtlessly convenient and justifiable to talk about RIP or AA as if *it* – note the singular form – could be "more" or "less" or "higher" or "lower", or as if *it* could influence (or be influenced by) other entities. However, the crux of this practice is that researchers can easily be led into thinking that what is singular and univariate in scientific language may (or even must) be translated into a single numerical index, as is documented by the literature cited in Manuscripts #1 and #2.

The history of index numbers in the social sciences begins with the practice of averaging across prices of different commodities to index price changes (Stigler, 1999, Chapter 3). Index numbers were only reluctantly admitted into the social sciences. The 19th century economists asked: "what meaning could an average price possibly have when each and every price carried with it a set of special circumstances, excuses, or causes for its peculiar behavior?" (Stigler, 1999, p. 73) Today, index numbers are widely accepted throughout the social sciences. One need only browse the journal *Social Indicators Research*, for example, to find a cornucopia of indices of any conceivable aspect of societies. One recently proposed index of violence and harm in the United States consists of the geometric mean of the percent change of homicides, suicides, substance abuse deaths, and other things, relative to a baseline (Brumbaugh-Smith, Gross, Wollman, & Yoder, in press).

There is nothing wrong with using such an index in order to, for instance, trace violence and harm across time, as long as one is content with not knowing specifically what changes or stays the same: Homicides, suicides, and/or substance abuse deaths? However, I would argue that researchers can rarely (if ever) be content with not knowing if the things they “pour together” behave in the way the choice of their index suggests. The violence and harm index tells us the rate of change in homicides, suicides, substance abuse deaths, et cetera, *if all these things were changing at the same rate*, but it does not tell us if they are. Researchers usually make careful theoretical choices when constructing their index variable, be it a mean, a difference as in research on RIP (see Manuscript #1), or a more complicated composite index as in research on AA (see Manuscript #2). These choices are valuable – the index is not. The problem with statistical analyses involving commonly used indices of RIP and AA is very simple: Results are ambiguous with regard to the conceptual hypothesis (e.g., RIP influences outgroup attitudes or AA moderates attitude effects). The solution to the problem is equally simple: The assumptions that would enter into a given index must be spelled out as a multivariate model. No index need be constructed. As Meehl remarked in a different context:”[O]ne should initially disaggregate, leaving open the possibility of reaggregation if the subdivision turns out not to matter; whereas if one begins by aggregation, one may be throwing away important information that is not recapturable.” (Meehl, 1997, p. 394)

Other multivariate constructs in social psychology

The foregoing definition of multivariate constructs applies to a whole range of constructs that are perhaps more mainstream than RIP and AA. My focus on RIP and AA owes to my personal interest in the ideas that the quality of intergroup relations depends on the definition of a superordinate prototype (Mummendey & Wenzel, 1999) and that attitudes vary on a strength dimension that is often orthogonal to the valence dimension (Petty & Krosnick, 1995). In this section, I briefly discuss other contructs where my methodological argument applies with the same force: ingroup bias, and prevention and promotion focus.

Ingroup bias

The construct of ingroup bias – preferential treatment or evaluation of an ingroup relative to an outgroup – arguably is one of social psychology's dearest constructs. An internet search for articles mentioning the term "ingroup bias" (conducted with Google Scholar on May 27th, 2007) produced more than one thousand results. As a result of the fruitfulness of Social Identity Theory (Tajfel & Turner, 1979) for generating hypotheses about its conditions and effect-size factors, ingroup bias has often assumed the status of a dependent variable. The variable is typically constructed by subtracting outgroup evaluations from ingroup evaluations, such that scores greater than zero represent a preference for the ingroup. The implicit assumption underlying a difference score as dependent variable is that the independent variable has equal but opposite effects on the components of the difference score. However, when a single variable is used to index ingroup bias, it is impossible to test if the independent variable has a positive (or negative) effect on ingroup evaluations and a negative (or positive) effect on outgroup evaluations that is of the same absolute magnitude. A discussion of a more appropriate multivariate approach to testing these hypotheses can be found in Edwards (1995).

It is interesting to note that research on ingroup bias has been influenced only minimally by reviews indicating that ingroup and outgroup evaluations are *not* reciprocally related (at least in the absence of a realistic conflict), and that consistent effects of the factors typically studied in the minimal group paradigm can only be observed for ingroup evaluations (Brewer, 1979, 1999). The use of difference scores – which confound effects of or on ingroup and outgroup evaluations – persists even in Brewer's own work (e.g., Leonardelli & Brewer, 2001). A simple explanation of this form of scientific inertia (which parallels the modest resonance of Cohen's power studies, see Sedlmeier & Gigerenzer, 1989) would be that the practice of constructing index variables of multivariate constructs constitutes a well-learned habit that is nurtured by tradition.

Implicit prejudice

Consider a related debate about a modern variant of the ingroup bias construct, i.e. implicit prejudice as measured via the Implicit Association Test (IAT, Greenwald, McGhee, & Schwartz, 1998). Blanton and colleagues (Blanton, Jaccard, Christie, & Gonzales, 2007; Blanton, Jaccard, Gonzales, & Christie, 2006) bring essentially the same arguments as above to bear on the validity of the IAT score, which is based on the difference of reaction times from different experimental conditions. Proponents of the IAT do not agree with their criticism (Nosek & Sriram, 2007). Consistent with my habit interpretation, their first counterargument refers to the widespread use of *d'*, the established sensitivity measure derived from Signal Detection Theory (Green & Swets, 1966). This measure is calculated by subtracting the *z*-score that corresponds to the false-alarm rate from the *z*-score that corresponds to the hit rate in a signal detection task. However, without further assumptions, the correlation between false-alarm rates and hit rates can vary considerably (O'Toole, Bartlett, & Abdi, 2000). Thus, false-alarm rates and hit rates can contribute quite independently to a correlation of *d'* with another variable, and not necessarily according to the equal-but-opposite effects model implied by a difference score.

Promotion and prevention focus

Higgins' (1998) Regulatory Focus Theory (RFT) constitutes another source of multivariate constructs. According to RFT, there exist both situational and individual differences in people's orientations toward goals. A promotion focus implies an orientation toward accomplishment and gains, whereas a prevention focus implies an orientation toward security and non-losses. The chronic aspects of these orientations (which form as a result of individual histories of goal pursuit) can be measured with the Regulatory Focus

Questionnaire (RFQ, Higgins et al., 2001). According to the authors, “[r]egulatory focus can be analyzed as a continuous variable with separate orthogonal scales for promotion pride and prevention pride. Additionally, it is possible to examine individual differences in either predominant promotion pride or prevention pride. In this case, the RFQ can be computed as a single categorical variable using a median split on the difference score of promotion pride minus prevention pride.” (Cesario, Grant, & Higgins, 2004, p. 392) This quote is intriguing because it juxtaposes the wrong and the right methodological approaches and assumes that they are equivalent. The expression “predominantly” suggests that the authors conceptualize promotion and prevention focus as multivariate constructs. However, the strategy involving a difference score is inadequate to actually test the equal-but-opposite effects hypothesis implied by a difference score. Furthermore, the information loss incurred by median splits is almost never justifiable (MacCallum, Zhang, Preacher, & Rucker, 2002).

Conclusion

As the previous section has shown, the multivariate approach put forward in Manuscripts #1 and #2 may profitably be extended to other social psychological constructs other than RIP and AA. Another indicator of the broad applicability of this approach is that similar methodological arguments already exist in the literature. However, like Manuscripts #1 and #2, they are tied to a specific substantive area of research, for instance, empathy (Cronbach, 1955) or the implicit ingroup bias discussed above (Blanton et al., 2006). Hopefully, the more general formulation of the problem presented in this introduction can help researchers to understand that *any* multivariate construct cannot be represented by a single numerical index and is better analyzed as a multivariate model, if the goal is to interpret relationships and effects without confounds.

As explained in further detail in Manuscripts #1 and #2, appropriate statistical analyses constitute the remedy against the confounds associated with index variables of multivariate constructs. Theoretically distinct components of an index can and should be treated as separate variables in a multivariate model, and the assumptions entering into a given index should be tested as a series of distinct statistical hypotheses and constraints. Nevertheless, I noted above that psychologists usually worry about other types of confounds, and I should note here that these are no less important than the specific confounds associated with multivariate constructs. For instance, in examining the relationship between positivity and negativity on the one hand, and subjective reports of ambivalence on the other (Manuscript #2), I tacitly assume that the sums or averages of the items are valid *unidimensional* representations of the underlying constructs. I also assume that the positive and negative memory contents associated with the attitude object are *causally* responsible for subjective reports of ambivalence. However plausible these assumptions may be, it remains a task for future research to examine their veridicality. I will discuss possible directions for future research in greater depth in the section “Final discussion and outlook”.

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Reconsidering the 'relative' in relative ingroup prototypicality

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Abstract

Relative ingroup prototypicality (RIP) is an important concept in the ingroup projection model of social discrimination and tolerance. This paper reviews measures of RIP currently in use and critically examines how the notion of *relative* ingroup prototypicality is captured by statistical tests treating RIP as a single variable. It is concluded that composite measures of RIP imply multiple statistical hypotheses that have previously been confounded. The value of an alternative multiple regression approach is illustrated in a study testing the hypothesis of a negative relationship between RIP and outgroup attitudes. Results based on the conventional univariate analyses would have confirmed or disconfirmed the hypothesis depending on the scoring method. In contrast, the multiple regression approach described in this paper resolves this ambiguity by suggesting that only outgroup prototypicality may be necessary to predict outgroup attitudes.

Reconsidering the 'relative' in relative ingroup prototypicality

A perennial question of social psychology concerns the conditions of prejudice and discrimination against outgroups. Although social categorizations are everywhere, not every social category different from our own attracts our hostility (Park & Judd, 2005). Sometimes we perceive ourselves as stereotypically distinct from other groups but get along well with outgroup members. How are these peaceful encounters different from the endless examples of groups in conflict for no apparent reason?

The ingroup projection model (IPM, Mummendey & Wenzel, 1999) directly addresses this question by elaborating on one of the central assumptions of self-categorization theory (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), namely "[t]hat the comparison of different stimuli depends upon their categorization as identical (the same, similar) at a higher level of abstraction, and takes place on dimensions which define their higher level identity" (Turner et al., 1987, p. 48). It follows that groups are evaluated more positively when they compare favorably with other groups on the dimensions defining the higher level identity.

Thus, the IPM hypothesizes "that an outgroup's difference will be evaluated negatively if both ingroup and outgroup are sufficiently included in a more abstract social category and if the ingroup's attributes are perceived as prototypical of the inclusive category" (Mummendey & Wenzel, 1999, p.164). Translated into a quantitative hypothesis, this means that the outgroup should be evaluated more negatively to the extent that it is less similar to the prototype of the superordinate category than the ingroup. A shorthand expression of this hypothesis would be that *relative ingroup prototypicality (RIP) is negatively correlated with attitudes toward the outgroup*.

This paper is about the complexities involved in testing this hypothesis. To be sure, the IPM provides a richer theoretical analysis of intergroup discrimination and tolerance than that, but a review of research on the model as a whole is beyond the scope of this paper. The only additional assumption of the IPM that we need to consider here is that people can have a propensity to project attributes defining their ingroup onto the superordinate category, which, as it were, stacks the deck in favor of the ingroup. As a result of this projection, the outgroup is in fact compared against the standards of the ingroup – and is unlikely to measure up.

In this paper, I am not concerned with the antecedents and the nature of the projection process. Instead, I will review different strategies to measure the result (i.e., RIP) and test its relationship with outgroup attitude. As will be indicated by this review, most previous research has employed rather complex measures of RIP that were constructed by integrating multiple component measures. I will demonstrate that such measures can lead into interpretational ambiguities jeopardizing the validity of conclusions. Therefore, in the empirical part of this paper, I will illustrate an alternative strategy to assess the empirical validity of the hypothesis of a negative relationship between RIP and outgroup attitude.

RIP measures

The simplest measures of RIP rely on blatant items asking group members how prototypical they think their ingroup and the outgroup are for the superordinate category that includes them both (Hegarty & Chryssochoou, 2005; Waldzus & Mummendey, 2004; Waldzus, Mummendey, Wenzel, & Weber, 2003). However, to avoid social desirability concerns and other problems inherent to the blatant character of these measures, previous research has predominantly used one of two more indirect procedures to measure RIP. One indirect method of measuring RIP first identifies in a pretest *n* attributes including stereotypically distinct ingroup and outgroup attributes (and possibly some attributes relevant or irrelevant to both groups). Pretesting also establishes that these four types of attributes are balanced in terms of valence. Participants are then presented with this list of attributes and asked to rate each attribute with regard to its typicality for ingroup, outgroup, and the superordinate category, yielding a set of $n * 3$ ratings (Ullrich, Christ, & Schlüter, 2006, Study 2; Waldzus, Mummendey, & Wenzel, 2005; Wenzel, 2001, Study 2; Wenzel, Mummendey, Weber, & Waldzus, 2003, Studies 1 and 2). Finally, RIP is typically calculated as the difference between the Euclidean distances between the superordinate category and ingroup and outgroup, respectively, in an *n*-dimensional attribute space:

$$RIP_A = \sqrt{\sum (T(SUPER)_i - T(OUT)_i)^2} - \sqrt{\sum (T(SUPER)_i - T(IN)_i)^2}\ , \tag{1}$$

where $T(OUT)_i$ refers to the rating of how typical attribute *i* is for the outgroup, $T(IN)_i$ refers to the rating of how typical attribute *i* is for the ingroup, $T(SUPER)_i$ refers to the rating of how typical attribute *i* is for the superordinate category, and the squared differences are summed over the entire set of *n* attributes. Thus, higher scores on the RIP measure reflect perceptions of greater dissimilarity between outgroup and superordinate category than between ingroup and superordinate category, that is, greater relative prototypicality of the ingroup for the superordinate category.

In other studies (Waldzus & Mummendey, 2004; Waldzus et al., 2003; Weber, Mummendey, & Waldzus, 2002; Wenzel et al., 2003, Study 3), participants first named distinct ingroup and outgroup attributes and then rated these self-generated attributes as to their typicality for the superordinate category. RIP was then calculated as the difference of directly rated prototypicality of ingroup attributes minus prototypicality of outgroup attributes:

$$RIP_B = \frac{\sum T(SUPER)_j}{n} - \frac{\sum T(SUPER)_k}{m}\ , \tag{2}$$

where $T(SUPER)_j$ refers to the rating of how typical ingroup attribute *j* is for the superordinate category, $T(SUPER)_k$ refers to the rating of how typical outgroup attribute *k* is for the superordinate category, and the typicality ratings are summed over the entire set of *n* ingroup and *m* outgroup attributes.

Is there a reason to prefer one measure over the other? One way to address this question is to compare the predictive power of both measures with regard to the central variable to be explained. The published studies cited above that used one of the indirect measures of RIP reported a total of nine correlations between outgroup attitude and RIP_A or RIP_B. I performed a small meta-analysis of these correlations to gauge the overall predictive power of RIP_A and RIP_B. The average correlation between RIP_A and outgroup attitude was r = -.25 (N = 592, k = 4), and the average correlation between RIP_B and outgroup attitude was r = -.21 (N = 264, k = 5).[1] Thus, the measures appear to be interchangeable in terms of their predictive power, yielding a small to moderate effect size.

One might argue that given equal predictive power, the measure that is easier to administer is superior to the other. Although measure RIP_B is in fact both procedurally and computationally simpler, its drawback is that the researcher has little control if the idiosyncratically generated ingroup and outgroup attributes are balanced in terms of valence, which might be problematic when RIP is tested as a predictor of attitudes towards the outgroup. However, this potential confound could be eliminated if a similarly pre-tested set of attributes were used as described above for measure RIP_A in an analysis that follows the model of RIP_B. Since RIP_B, leaving out the typicality ratings with regard to ingroup and outgroup, requires only one third of the ratings required for RIP_A, it would still be procedurally and computationally simpler.

Are the typicality ratings with regard to ingroup and outgroup really necessary when the attributes have already been pretested? The advantage of RIP_A can be appreciated by comparing it to the efficiency gain associated with covariates in a randomized experiment. Although randomization ensures that the expectation of the difference between experimental and control groups before treatment is zero, experimental and control groups are likely to differ (if slightly) in any particular experiment. Similar to covariates, then, the purpose of measuring the attributes' typicality for the ingroup and outgroup in spite of the pretest is to correct for minor differences between the pretest sample and the experimental sample, thereby increasing precision and power.

By and large then, RIP_A and RIP_B appear to be equally viable ways of measuring RIP. Compared with the more direct measures of RIP briefly discussed at the beginning of this section, the strengths of RIP_A and RIP_B reside in the likely absence of social desirability concerns. There is only a minor tradeoff to be made between a slightly more precise measure (RIP_A) and a slightly more economical measure (RIP_B), the outcome of which depends on the researcher's resources or theoretical interests. Having identified the two most promising measures of previous RIP research, I will now analyze more closely how the theoretical construct – *relative* ingroup prototypicality – is captured in statistical analyses relating RIP_A and RIP_B to other measures.

Statistical tests of the RIP-attitude link

At its core, the construct RIP refers to a difference between judgments. When ingroup projection occurs, the ingroup is seen as *more* prototypical of a superordinate category than the outgroup, and to the extent that this is true, that is, the more relatively prototypical the ingroup is perceived to be, the more negative should be the outgroup attitudes. In operationalizing the construct, researchers have accordingly relied on difference scores as shown in equations (1) and (2). General problems inherent to the use of difference scores include their potentially reduced reliability (e.g., Cronbach & Furby, 1970, pp. 70-71), the inevitability of (spurious) correlations with their component scores (e.g., Cohen & Cohen, 1983, pp. 414-417; Cronbach, 1955), and the related problem that correlations of difference scores with an external variable are confounded with the correlations of the component scores with that variable (e.g., Johns, 1981). In this paper, I address the implications of the latter problem, the confounding of the RIP score with its components. My goal is to draw attention to the possible confounds and equip future research with an alternative analytic strategy that circumvents these confounds.

Confounds of RIP$_B$

The problem can be seen more clearly when the correlation between RIP and outgroup attitude (or any other external variable) is expressed in terms of the constituents of the RIP formula. Let us turn to the simpler case of the correlation (denoted as ρ) between outgroup attitude and RIP$_B$ first:

$$\rho_{ATT,RIP} = \frac{\sigma_{ATT,T(IN)} - \sigma_{ATT,T(OUT)}}{\sigma_{ATT}\sqrt{(\sigma^2_{T(IN)} + \sigma^2_{T(OUT)} - 2\sigma_{T(IN),T(OUT)})}} \quad , \tag{3}$$

where *T(IN)* and *T(OUT)* refer to the mean perceived typicality of ingroup and outgroup attributes, respectively, for the superordinate category, and *ATT* refers to outgroup attitude. Equation (3) shows that the correlation of RIP$_B$ and outgroup attitude is given by the difference between the covariances of outgroup attitude with ingroup and outgroup prototypicality, respectively, divided by the standard deviations of outgroup attitude and the RIP$_B$ difference score.

A first insight offered by equation (3) is that nothing enters into the RIP$_B$ - ATT correlation that would carry information about individuals' perceiving the ingroup to be *relatively more* prototypical than the outgroup. If we assume standard deviations of 1 for ATT and RIP$_B$ to simplify the formula, the average correlation obtained in previous research (r = -.21), could be the result of any combination of correlations between ATT and T(IN) and ATT and T(OUT) that satisfies the condition that $r_{ATT,T(ING)} - r_{ATT,T(OUT)} = -.21$. Consider the following four situations that could have brought about the overall correlation between ATT and RIP$_B$:

(a) a correlation of r = -.21 between ATT and T(IN) and a zero correlation between ATT and T(OUT), (b) a correlation of r = .21 between ATT and T(OUT) and a zero correlation between ATT and T(IN), (c) a stronger negative correlation between ATT and T(IN) than between ATT and T(OUT), or (d) a stronger positive correlation between ATT and

T(OUT) than between ATT and T(IN). The differences between these four cases reveal the extent of information loss implied by calculating a difference score between T(IN) and T(OUT). But does the loss of information really matter? After all, the IPM makes predictions about RIP, not about T(IN) and T(OUT) separately. The cost of these confounds becomes apparent if we recall that the IPM is geared towards the amelioration of real-world conflicts. Although correlations provide only a weak basis for social interventions (Box, 1966), they are currently the best evidence we have regarding the effects of RIP on outgroup attitude. If we were to design an intervention aimed at improving people's outgroup attitudes, it would be of crucial importance to know the separate effects of T(IN) and T(OUT). Should we try to reduce T(IN), e.g. by convincing people that the world does not revolve around them, possibly at the expense of their collective self-esteem? Or would it be sufficient to increase T(OUT), e.g. by convincing people that the prototype of a given superordinate category is flexible enough to include both ingroup and outgroup attributes?

As these unanswered questions show, relationships obtained with difference scores can obscure what is really going on in the data and provide suboptimal guidance for social interventions. How, then, can we test the relationship between ATT and RIP without relying on difference scores? Following Edwards (e.g., 1994), I propose that a more adequate test of a relationship involving a difference-based construct is possible if the implied hypotheses about the constituents of the difference are made explicit. Toward that end, we merely need to expand the simple regression (Equation 4 below) to a multiple regression (Equation 5 below):

$$ATT = b_0 + b_1\left[T(IN) - T(OUT)\right] + error\,, \tag{4}$$

$$ATT = b_0 + b_1 T(IN) - b_1 T(OUT) + error\,. \tag{5}$$

With regard to the simple regression of ATT on RIP_B calculated as in (3), it is apparent that the IPM hypothesizes that the regression coefficient for RIP_B will be negative, that is, $b_1 < 0$ in equation (4). Expanding this equation yields (5), from which the implied hypotheses about the constituents of the difference score can be ascertained. Specifically, it can be seen that the test of the IPM by means of a difference score not only hypothesizes that the effect of T(IN) is negative, but also constrains the effect of T(OUT) to be positive and of the same size as T(IN), for equation (5) shows that the parameter for T(OUT) is the same as for T(IN) except for the reversal of sign. Thus, a strict test of the ATT-RIP_B relationship requires estimating a multiple regression model predicting ATT on the basis of T(IN) and T(OUT), and answering the following questions: Is the effect of T(IN) negative? Is the effect of T(OUT) positive? Do the effects of T(IN) and T(OUT) sum to zero? If either of the first two questions does not lead to an affirmative answer, the hypothesis of a negative relationship between ATT and RIP_B is rejected and it is unnecessary to test the equality of the absolute magnitudes of T(IN) and T(OUT).

Finally, a caveat should be borne in mind regarding the internal consistencies of T(IN) and T(OUT). As pointed out by Cronbach (1992), “similarity in the abstract may be an unprofitable focus” (p. 390). It is worth examining if the effects of T(IN) and T(OUT) are roughly similar across attributes before the typicality ratings are averaged to represent ingroup and outgroup prototypicality in the more abstract sense. For instance, if there are four ingroup attributes and four outgroup attributes, one could randomly define four pairs and examine if the pairs are interchangeable in terms of the effects of T(IN) and T(OUT). Merely calculating the internal consistencies of T(IN) and T(OUT), treating attributes as items, may be too simple because these items are likely to contain systematic measurement error reflecting participants’ idiosyncratic usage of the response scale.

Confounds of RIP_A

Because RIP_A, too, is a difference score and its effects are confounded with the effects of its components, it cannot be used to adequately test the hypothesis of a negative relationship between ATT and RIP. At first blush, the remedy appears to be the same as the one outlined for RIP_B, that is, to estimate the effects of the constituents of the difference score separately. Indeed, at the conceptual level, the Euclidean distances that are subtracted from one another in equation (1) should be the exact opposite of the mean typicality ratings in equation (2) because the typicality ratings are intended to represent similarity between the subgroups and the superordinate category, and the distances are intended to represent dissimilarity. However, the Euclidean distances introduce a new source of confounds, namely the set of difference scores calculated by subtracting a given attribute's typicality for the subgroup from its typicality for the superordinate category. If we focus on one attribute at a time (as suggested at the end of the previous section and indicated below by the subscript *i*), we can rewrite equation (1) using the absolute difference instead of the root of the squared difference:

$$RIP_{A(i)} = \left|T(SUPER)_i - T(OUT)_i\right| - \left|T(SUPER)_i - T(IN)_i\right|. \quad (6)$$

Equation (6) shows that RIP_A is made up of a difference between absolute differences. Thus, in order to deconfound this index, we need to combine the analytic strategies suggested by Edwards (1994) for differences and absolute differences. We begin by inserting the right-hand side of (6) into a simple regression equation predicting ATT:

$$ATT = b_0 + b_1\left[\left|T(SUPER)_i - T(OUT)_i\right| - \left|T(SUPER)_i - T(IN)_i\right|\right] + error. \quad (7)$$

Then we multiply the index through by the regression coefficient b_1, equivalently to the procedure applied to RIP_B, though leaving the absolute differences intact for the moment:

$$ATT = b_0 + b_1\left[\left|T(SUPER)_i - T(OUT)_i\right|\right] - b_1\left[\left|T(SUPER)_i - T(IN)_i\right|\right] + error. \quad (8)$$

Finally, we rewrite (8) according to Edwards' suggestions for absolute differences:

$$\begin{aligned} ATT = b_0 &+ b_1(1-2V)(T(SUPER)_i - T(OUT)_i) \\ &- b_1(1-2W)(T(SUPER)_i - T(IN)_i) + error \end{aligned}, \quad (9)$$

where *V* is a dummy variable that equals 0 when the $T(SUPER)_i - T(OUT)_i$ difference is positive, equals 1 when the $T(SUPER)_i - T(OUT)_i$ difference is negative, and is randomly set to 0 or 1 when $T(SUPER)_i - T(OUT)_i$ equals 0. By the same logic, *W* is set to

0 or 1 as a function of the $T(SUPER)_i - T(IN)_i$ difference. Expanding (9) and rearranging terms yields:

$$\begin{aligned} ATT &= b_0 + b_1 T(IN)_i - b_1 T(OUT)_i - 2b_1 WT(IN)_i + 2b_1 VT(OUT)_i \\ &+ 2b_1 WT(SUPER)_i - 2b_1 VT(SUPER)_i + error \end{aligned} \quad . \tag{10}$$

As a result of the last step in our algebra, the term $T(SUPER)_i$ drops out of the equation, which indicates the first constraint implied by RIP_A (namely, that its coefficient is zero). Thus, to test this constraint along with the others, we put the term back into the unconstrained equation. Similarly, it is necessary to include the main effects of the dummy variables *V* and *W* to rule out their confounding the interaction terms. The hypothesis of a negative relationship between ATT and $RIP_{A(i)}$ would then be tested by estimating the following multiple regression model:

$$\begin{aligned} ATT &= b_0 + b_1 T(SUPER)_i + b_2 T(OUT)_i + b_3 T(IN)_i + b_4 V + b_5 W \\ &+ b_6 VT(SUPER)_i + b_7 VT(OUT)_i + b_8 WT(SUPER)_i + b_9 WT(IN)_i + error \end{aligned} \tag{11}$$

The conceptual hypothesis implies the following statistical hypotheses: $b_2 > 0$, $b_3 < 0$, $b_6 > 0$, $b_7 < 0$, $b_8 < 0$, and $b_9 > 0$, and the following constraints: $b_1 = b_4 = b_5 = 0$, $b_2 = -b_3$, $b_6 = -b_7$, $b_8 = -b_9$, $b_6 = 2b_2$, and $b_8 = 2b_3$. The large number of null and alternative hypotheses implied by the conceptual hypothesis – which will further increase with the number of times equation (11) is estimated for different attributes – suggests that it may be wise to adjust the nominal α-level of the significance tests (downward for the alternative hypotheses, upward for the null hypotheses), in order to avoid spurious results. However, this strategy is only useful if a large enough sample is available, because the large number of (probably highly multicollinear) variables reduces power and destabilizes the parameter estimates (Cohen & Cohen, 1983). It is thus doubtful if theoretical interest will warrant such a tedious and expensive investigation.

Summary and preliminary conclusions

The previous two sections have made explicit what kinds of confounds are to be expected when RIP_A and RIP_B are used to predict a dependent variable. The first lesson to be drawn from these considerations is that RIP_A and RIP_B should be treated as multivariate models rather than single variables. With regard to RIP_B, it appears quite feasible to translate it into a multivariate model so as to determine to what extent T(IN) and T(OUT) conform to the theoretical predictions of the RIP hypothesis. In fact, I will demonstrate this analytic strategy below with empirical data. With regard to RIP_A, the complexity of the model that would be required for an adequate test of the RIP hypothesis may be too taxing on the resources (i.e., sample size, complexity of analyses) that researchers are willing to invest. Nevertheless, I hope that by making explicit the entire set of hypotheses and constraints underlying RIP_A, I have drawn sufficient attention to the number of potential confounds of RIP_A so as to argue against its use as a single variable.

In sum, the most adequate statistical test of the RIP-outgroup attitude link appears to consist of estimating the independent effects of T(IN) and T(OUT) in a multiple regression equation and to test the three hypotheses implied by RIP_B. Lest all the advantages of RIP_A should be sacrificed in light of the complex model it implies, I would suggest that T(IN) and T(OUT) should refer to a set of pretested ingroup and outgroup attributes as in previous studies employing the RIP_A measure.

Empirical demonstration of the alternative approach

The goal of the study reported below was to test the hypothesis that RIP is negatively related to participants' attitude toward an outgroup while improving on two aspects of previous research. First, I applied the analytic strategy outlined above for RIP_B so that the resulting conclusions about the RIP-outgroup attitude link could be compared with those based on the composite measures of previous studies. Second, although previous research has relied on single-item measures of attribute typicality, I sought to obtain more reliable multi-item measurements to make a possible divergence of conclusions more readily interpretable.

Method

Participants

One-hundred and twenty-seven students were recruited on the campus of a middle-sized German university for an online survey on "personality traits of students", in which they provided typicality ratings of several personality dimensions for male students, female students, and students in general. They were told that two cinema coupons would be raffled among participants. Although it would have been more desirable to have a balanced number of male and female students, the sample was predominantly female. Furthermore, many participants did not complete the relatively long survey (ca. 30 min. duration). Thus, in the interest of conclusive results I restrict the following analyses to a subsample of $N = 72$ female students with complete data on all relevant items. The age range of participants was from 18 to 42 years, 76% of participants were younger than 23 years.

Procedure

Participants first provided demographic information and completed several scales unrelated to the present research question. They were then presented with a series of items from the observer form of the revised NEO Personality Inventory (NEO-PI-R, Ostendorf & Angleitner, 2004) and asked to indicate how much each trait description applied to their ingroup (female students), the outgroup (male students), and the superordinate category (students in general). For instance, an item from the aesthetics subscale was presented as follows: "What would you say about female students in general? Aesthetics and art mean a lot to them." The response options were "strong disagreement" (1) to "strong agreement" (5). Each scale consists of 8 items, some of which are reversed. The order of judgments referring to different targets (i.e., ingroup, outgroup, or superordinate category) was randomly determined, as was the order of items within targets. After completing 192 personality ratings, participants were asked to indicate their attitude toward male students and enter their e-mail address if they wished to take part in the raffle and/or wished to receive further information about the study.

Measures

NEO-PI-R subscales. The German manual of the NEO-PI-R includes gender norms of the subscales (of the observer form) for young adults, which constitute the "pretest" of the subscales' typicality for ingroup and outgroup. I selected at least one of the subscales of each of the Big 5 personality dimensions, namely those that exhibit the largest differences between genders and appeared to be reasonably applicable to large social categories: *Anxiety* (more pronounced among young women = F), *aesthetics* (F), *self-consciousness* (F), *ideas* (more pronounced among young men = M), *gregariousness* (F), *excitement-seeking* (M), *altruism* (F), and *dutifulness* (F). These gender differences were also perceived as such by my participants, as is confirmed by the ingroup and outgroup typicality ratings for the personality facets, which differed significantly in the expected directions (see Table 1 for the means and effect sizes).

RIP_A. The conventional RIP_A index was calculated as in equation (1).

RIP_B. The conventional RIP_B index was calculated as in equation (2).

Outgroup attitude. Attitude toward male students was measured with eight items that were roughly based on items used in previous research on the IPM (e.g. Wenzel et al., 2003). However, preliminary analyses indicated that the item intercorrelations were quite low, so I selected only three items that together formed an acceptably internally consistent scale (Cronbach's $\alpha = .63$): (1) "I like working together with male students", (2) "I enjoy the company of male students", (3) "I would like to work in a long-term project together with a male student". Interestingly, all of these items refer to a willingness for close contact with male students. The response options were from "strong disagreement" (1) to "strong agreement" (5). The items were averaged to yield an index of outgroup attitude. Higher scores represent a more positive attitude.

Results

In order to establish a baseline against which to compare the analytic strategy I am proposing, I first calculated the correlations between outgroup attitude and RIP_A (r = .02, p = .85) and RIP_B (r = -.25, p = .04). Interestingly, results of the indices do not converge as in the meta-analysis reported above. Note, however, that these indices are compared here for the first time within one and the same study. Because both indices operationalize the same construct, this suggests that the data are not consistent with the assumptions implied by a literal reading of the *relative* ingroup prototypicality hypothesis.

In order to better understand what aspects of the complex construct of RIP are borne out by the data, I employed the regression-based strategy described above for RIP_B. I first screened if results would largely agree across the different combinations of ingroup and outgroup personality facets. Thus, I estimated twelve regression models with outgroup attitude as the dependent variable and ingroup and outgroup prototypicality for the superordinate category as separate predictors. For each regression model, I selected a different combination of the 6 (Ingroup) x 2 (Outgroup) personality facets. As can be seen in Table 2, the results of ten out of twelve regression models were consistent with a model that included the average T(IN) and T(OUT) scores as in equation (2). Thus, to simplify the presentation of results, I describe only the results of the latter model. Recall that this model was estimated to test the following hypotheses: Is the effect of T(IN) negative? Is the effect of T(OUT) positive? Do the effects of T(IN) and T(OUT) sum to zero?

Overall, the regression model explained 13% of the variance in outgroup attitudes, $F(2,69) = 5.08$, $p = .01$. The effect of T(IN) was $b = .10$, $t(69) = .37$, $p = .71$, which disconfirms Hypothesis 1 (and renders a test of Hypothesis 3 unnecessary). Note that the absence of an effect of ingroup prototypicality can neither be attributed to lower reliabilities of the measures of ingroup attributes (see Cronbach's alphas in Table 1), nor to a ceiling effect, for the means shown in Table 1 are in the middle range of the scale ranging from 1 to 5. The effect of T(OUT) was $b = .51$, $t(69) = 2.75$, $p = .01$, supporting Hypothesis 2. Thus, while ingroup prototypicality did not explain more than a trivial amount of variance in outgroup attitude, results indicated that the more prototypical participants judged the outgroup to be for the superordinate category, the more positive was their attitude toward the outgroup.

Discussion

Studies in social perception are notoriously prone to statistical confounds because they rely on judgments made by multiple perceivers with regard to multiple targets (e.g., Blanton, Jaccard, Gonzales, & Christie, 2006; Cronbach, 1955; Krueger, 1996). This paper has critically examined how the notion of *relative* ingroup prototypicality (RIP) is captured by the use of difference scores in the statistical tests of a negative relationship between RIP and outgroup attitude. The theoretical part of the paper has made explicit the multitude of hypotheses that are implied by the indices RIP_A and RIP_B that have most frequently been used in previous research. It was concluded that when these indices are used as single variables, it is impossible to disentangle effects of ingroup and outgroup typicality or to identify spurious correlations. Therefore, an alternative approach was described which treats RIP as a multivariate model implying multiple statistical hypotheses.

The results of an empirical study support the superiority of the alternative approach by revealing the following findings: (1) RIP_A and RIP_B, which have been compared here for the first time directly, led to different conclusions about the RIP-outgroup attitude link, with the former measure being unrelated and the latter being negatively related to the attitude measure. This divergence was to be expected given the number of (different) confounds underlying each index. (2) Whereas RIP_A and RIP_B would have supported or weakened the IPM's hypothesis as a whole, the alternative approach revealed a more fine-grained pattern that appears to be more useful for devising social interventions. More specifically, ingroup prototypicality (and, by implication, *relative* ingroup prototypicality) was unrelated to outgroup attitude, but outgroup prototypicality for the superordinate category was positively associated with outgroup attitude. This finding would have been overlooked if ingroup and outgroup prototypicality had been collapsed into a single measure (although the correlations reported in Waldzus and Mummendey 2004, Experiment 2, Inclusion condition, point in the same direction). If this pattern of results turned out to be robust in future studies, it would be more parsimonious to postulate a positive association between outgroup prototypicality and outgroup attitude than to postulate a more complex model about RIP as implied by the indices RIP_A and RIP_B.

While these are clearly speculative points that have to be addressed by future research, the main contribution of the present paper was to alert researchers to the possibility of confounds underlying the RIP measures, and to suggest an alternative approach that would allow for the detection of these confounds. Models do not have to be exactly true to be useful (Box, 1979), but to make cumulative progress it is vital that researchers discover where their models depart from empirical reality. It is possible that the relative roles of ingroup and outgroup prototypicality depend on the nature of the specific intergroup relations that are being studied, and I hope that the multivariate approach I am proposing is useful to identify these circumstances.

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Footnote

1 These correlations are weighted averages, taking sample size into account. From Waldzus and Mummendey (2004), I discarded the reported correlation from the exclusion condition (because a correlation different from zero was not predicted) and averaged the correlations from the inclusion condition that were reported separately for outgroup attitude as measured on relevant and irrelevant dimensions.

Table 1

Means, Cronbach's alphas, and effect sizes of the NEO-PI-R subscales for all target categories (N = 72 female students)

	Target category						Effect sizes of paired comparisons		
	(1) Ingroup		(2) Outgroup		(3) Superordinate		(1) vs. (2)	(1) vs. (3)	(2) vs. (3)
	M	*α*	*M*	*α*	*M*	*α*	*d*	*d*	*d*
Anxiety	3.51	.81	2.96	.76	3.28	.79	1.03	0.57	-0.71
Aesthetics	3.61	.81	2.96	.69	3.34	.75	1.10	0.92	-0.80
Self-consciousness	3.30	.53	2.71	.68	2.91	.59	1.10	0.93	-0.54
Ideas	3.36	.75	3.57	.78	3.62	.74	-0.43	-0.72	-0.13*
Gregariousness	3.62	.71	3.52	.73	3.67	.78	0.25	-0.18*	-0.37
Excitement-seeking	3.13	.48	3.58	.71	3.40	.64	-1.09	-0.76	0.61
Altruism	3.45	.71	3.00	.71	3.17	.76	0.84	0.73	-0.37
Dutifulness	3.16	.80	2.68	.78	2.81	.78	0.96	0.91	-0.35

Note. The standardized mean difference *d* was calculated as $\frac{t}{\sqrt{n}}$, where *t* refers to the *t*-values of the paired comparisons between the category-specific subscales, and *n* is equal to 72. Positive values indicate a higher mean in the left column of each comparison.

All effect sizes are significant at α = .05, except those marked with an asterisk.

Table 2

Regression coefficients of ingroup and outgroup prototypicality predicting outgroup attitude

Combination of personality facets	Ingroup prototypicality (F)		Outgroup prototypicality (M)	
	b	*SE*	*b*	*SE*
Aesthetics (F) and ideas (M)	0.13	0.17	0.27	0.16
Gregariousness (F) and ideas (M)	0.30*	0.14	0.18	0.15
Altruism (F) and ideas (M)	0.10	0.13	0.29*	0.14
Dutifulness (F) and ideas (M)	0.05	0.12	0.31*	0.14
Anxiety (F) and ideas (M)	-0.13	0.12	0.33*	0.14
Self-consciousness (F) and ideas (M)	-0.12	0.16	0.31*	0.14
Aesthetics (F) and exc.-seek. (M)	0.12	0.16	0.44*	0.17
Gregariousness (F) and exc.-seek. (M)	0.21	0.16	0.32	0.20
Altruism (F) and exc.-seek. (M)	0.12	0.12	0.46**	0.16
Dutifulness (F) and exc.-seek. (M)	0.13	0.12	0.50**	0.16
Anxiety (F) and exc.-seek. (M)	-0.11	0.12	0.48**	0.16
Self-consciousness (F) and exc.-seek. (M)	-0.10	0.15	0.47**	0.16
T(IN) and T(OUT)	**0.10**	**0.28**	**0.51****	**0.19**

Note. exc.-seek. = Excitement-seeking, T(IN) = mean ingroup prototypicality, T(OUT) = mean outgroup prototypicality, *b* = Unstandardized regression coefficient, *SE* = Standard Error, * $p < .05$, ** $p < .01$.

Attitude ambivalence or statistical artifact?

Multivariate constructs require multivariate analyses

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Abstract

The attitude strength literature suggests that ambivalent attitude structures are associated with less stable and consequential attitudes. Such claims are often tested on the basis of an ambivalence index derived from largely independent measures of positivity and negativity. In the present article, we demonstrate that statistical analyses treating ambivalence as a single variable can produce misleading results and do not allow for competitive tests between different ambivalence models. We illustrate how a Multivariate Approach to Ambivalence Models (MAAM) can be used instead to examine ambivalence as the conceptual independent or moderator variable, comparing the Conflicting Reactions Model (CRM) and the Similarity-Intensity Model (SIM) of ambivalence. Studies 1 and 2 indicated that the CRM explains subjective ambivalence toward outgroups better than the SIM. Studies 3 and 4 replicated the typical moderator effect of ambivalence on stability of outgroup attitudes using a univariate ambivalence index, but multivariate analyses based on the MAAM revealed this effect to be spurious. Study 5 used Monte Carlo methodology to verify that this moderator artifact occurs above chance levels. Our results should caution researchers against treating ambivalence as a single variable.

Keywords: Attitude Ambivalence, Attitude Strength, Data Analysis, Moderator Effects, Intergroup Attitudes

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In their milder forms, pathologies often feel familiar. Such is the case with the feeling of ambivalence. When Swiss psychiatrist Eugen Bleuler (1911) introduced the concept to psychology, it was primarily meant to capture pathological forms of loving and hating the same object, such as can be observed among schizophrenics. Yet Bleuler already pointed out the ubiquity of ambivalence in the everyday life of normal people, foreshadowing the current surge of ambivalence research in many areas of psychology, including social psychology (e.g., Jost & Burgess, 2000; Maio, Greenland, Bernard, & Esses, 2001; Mucchi-Faina & Cicoletti, 2006; Newby-Clark, McGregor, & Zanna, 2002; Petty, Tormala, Briñol, & Jarvis, 2006; van Harreveld, van der Pligt, de Vries, Wenneker, & Verhue, 2004), organizational psychology (e.g., Brooks, Highhouse, Russell, & Mohr, 2003; Piderit, 2000), health psychology (e.g., Armitage, Povey, & Arden, 2003; Dahl, Darke, Gorn, & Weinberg, 2005; Dormandy, Hankins, & Marteau, 2006; Sparks, Conner, James, Shepherd, & Povey, 2001), and political psychology (e.g., Basinger & Lavine, 2005; Craig & Martinez, 2005; Rudolph, 2005).

Indeed, people often see both favorable and unfavorable aspects in a person, thing, or idea. However, they may not always be aware of this ambivalence or willing to express it (cf. Conner & Sparks, 2002; Jonas, Brömer, & Diehl, 2000; Nisbett & Wilson, 1977). At the very least, as Bleuler already observed, non-schizophrenic people tend to tally an object’s favorable and unfavorable aspects and ascertain the balance. Thus, when social psychologists adopted the concept for non-clinical applications, they had to find a way of measuring ambivalence other than just asking people how ambivalent they feel about something. In fact, social psychologists began to collect data in such a way that would allow them to do the tallying themselves. For instance, the new data collection procedures would require people to list the salient positive and negative attributes of an object (Scott, 1969), or to rate separately the extent of their positivity and negativity with regard to an object using split semantic differentials (Kaplan, 1972).

Various mathematical formulae have been proposed for quantifying ambivalence toward an object based on such separately obtained measures of positivity and negativity.[1] Each formula rests on a model of the nature of ambivalence that contains several testable assumptions about the effects of positivity and negativity (for an overview, see Breckler, 1994; Jonas et al., 2000; Priester & Petty, 1996; Riketta, 2000; Thompson, Zanna, & Griffin, 1995). However, we argue here that these assumptions have never been rigorously tested because previous research has combined the separate measures of positivity and negativity into a single index. To be sure, it is admissible and convenient to refer to the scores resulting from the ambivalence formulae by the shorthand "ambivalence", just like one would refer to one of Hull's equations by the shorthand "habit strength" (cf. MacCorquodale & Meehl, 1948). However, as the present paper will demonstrate, it can be seriously misleading to test hypotheses about ambivalence by subjecting such ambivalence scores to statistical analyses involving other variables (which is where ambivalence researchers differ from Hull).

Results obtained from standard analyses widely used by ambivalence researchers are in general open to multiple interpretations because ambivalence scores are necessarily confounded with their constituents. As a result, it is difficult to compare empirical results across laboratories, which impedes cumulative progress in research on ambivalence. The crucial problem of the confounding of a composite variable is of a more general nature and has long been recognized in the area of discrepancy or change scores (Campbell & Kenny, 1999; Cohen & Cohen, 1983; Cronbach, 1955; Edwards, 1994; Johns, 1981; Stelzl, 1982). But ambivalence scores are not change scores, and the consequences of this confounding may be less obvious. Therefore, the goals of the present paper are (1) to systematically examine the threats to validity arising from this confounding, (2) to propose a new data-analytic procedure that will allow researchers to unambiguously test hypotheses about the effects of ambivalence, and (3) to illustrate the superiority of the new approach both in empirical and computer simulation studies.

The concept of validity is used here in the general sense of a *match* between a numerical estimate (e.g., a regression coefficient) and the label given to that estimate (Reichardt, 2006). The labels that ambivalence researchers typically want to attach to their estimates are (1) “Ambivalence influences *Y*” or (2) “Ambivalence moderates the relationship between *X* and *Y*”. In the present paper, we are less concerned with the causal undertones of the terms “influences” or “effect” – the effect may or may not be causal. Rather, our goal is to show that, given current data-analytic practice, researchers may misattribute an effect to ambivalence when it is due to (possibly trivial) effects of positivity or negativity alone. Although the theoretical argument of this paper does not depend on the specific meaning of *X* or *Y*, the examples and empirical studies presented below join the majority of previous studies that consider ambivalence as a measure of attitude strength. Therefore, we begin with a brief review of the theoretical assumptions underlying the attitude strength concept.

The Attitude Strength Perspective

The paramount interest in the attitude concept is rooted in the idea that to know or change a person’s attitude is to know or change how that person will likely act within a given behavioral domain. Toward that end, social psychologists infer a person’s standing on a latent attitude continuum from the observed covariation between stimuli representing the attitude object and a person’s evaluative responses to it (Eagly & Chaiken, 1993). However, more often than not, the sample of observed responses available to the researcher consists of little more than a few probes of attitudinal responses obtained at a single point in time. Whether these attitude probes prove predictive of future behavior should depend, first of all, on the temporal stability of the measured attitude (Ajzen, 1996; Eagly & Chaiken, 1995; Lord & Lepper, 1999; Schwartz, 1978). If the attitude no longer exists when behavior is assessed, it can exert no influence. Yet the degree of temporal stability of attitude toward a given object may be as uncertain as the correspondence of the measured attitude and future behavior.

Thus, to enhance prediction of behavior, personality and social psychologists alike have invoked a concept of *strength*, although applying it to different parts of psychology's fundamental behavioral equation. As stated by Lewin (e.g., 1951), this equation simply holds that responses in any given situation are jointly determined by factors internal to the person (in the present context, the person's prior attitude) and situational factors that constitute the psychological environment. This means that prediction will be enhanced to the extent that situational factors are weak and internal factors are strong. Personality psychologists have applied the strength concept to situations. Weak situations are characterized by a lack of implicit or explicit guidelines for appropriate behavior (Mischel, 1977), which leaves more variance in behavior to be explained by personality variables. Social psychologists, on the other hand, have applied the strength concept to the internal attitude variable. Attitudes are said to be strong to the extent that they persist over time, resist pressures to change, and impact on judgment and behavior (Krosnick & Petty, 1995; Petty & Cacioppo, 1986).

Paralleling interest in the attitude concept, a substantial literature has evolved examining several attitude attributes that would allow inferences regarding attitude strength (for reviews, see Petty & Krosnick, 1995; Raden, 1985; Scott, 1968; Visser, Bizer, & Krosnick, 2006). For instance, such inferences may be based on asking people how certain they are regarding their attitude (Abelson, 1988; Gross, Holtz, & Miller, 1995) or observing how quickly they respond to the attitude probe (Fazio, 1995). Attitude ambivalence is one of the proposed attributes whose relation to the strength concept is relatively obvious. By definition (see below), ambivalent attitudes are characterized by equally strong positive and negative evaluative tendencies and thus a lack of directional guidance. Hence, when people holding ambivalent attitudes introspect about their attitudes or prepare for action, they have to resort to situational cues to decide which of the evaluative tendencies to give more weight (Katz, 1981). This will diminish the impact of their existing attitude.

In sum, the nomological network of the attitude strength construct relates ambivalence to a variety of other strength indicators (e.g., certainty or accessibility), and to the defining features of strong attitudes (e.g., attitude's impact on judgment and behavior). Empirical investigations of the construct validity of attitude strength therefore often use correlation or regression analyses to examine how well ambivalence predicts (or is predicted by) other attitude attributes, and moderated multiple regression analyses to test the postulated moderating influence of ambivalence on attitude effects. In the following sections, we will analyze in some detail why the statistical analyses called for by the attitude strength perspective lead to ambiguous conclusions about ambivalence models.

Models of Attitude Ambivalence and Statistical Analyses

Numerous ways have been proposed to model ambivalence as a function of positive and negative object evaluations. For instance, in their influential paper, Priester and Petty (1996) reviewed eight such models. For space limitations, only Kaplan's (1972) Conflicting Reactions Model (CRM) and the Similarity-Intensity Model (SIM, Thompson et al., 1995) are considered here in detail. We opted for the CRM because it is the simplest ambivalence model, and for the SIM because it is the most widely employed one (Conner & Sparks, 2002). However, the following demonstrations should by and large be easy to apply to other ambivalence models.

In the following, we assume (1) that separate measures of positivity and negativity (P, N) toward an attitude object form the basis for statistical calculations, (2) that these measures are scored in the same direction, such that higher scores of P reflect more positivity and higher scores of *N* reflect more negativity. We begin with the case of ambivalence as the conceptual independent variable and then consider the most frequently occurring but somewhat more complicated case of ambivalence as a moderator variable of attitude effects. Within each subsection, we first discuss the assumptions of the ambivalence model, then we explore possible confounds underlying the current data-analytic approach, and finally, we suggest a viable alternative approach.

Ambivalence as the Independent Variable

Ambivalent attitude structures are often viewed as causes of other measures of attitude strength. Most prominently, perceiving one's attitude as ambivalent (i.e., subjective ambivalence) should be a function of ambivalence as assessed with separate measures of positivity and negativity. To measure ambivalence, researchers rely on one of several formulae to combine the measures of positivity and negativity into a single index. Let us first turn to the formula suggested by the CRM.

Assumptions of the CRM

Kaplan (1972) defines ambivalence as "the amount of exactly counterbalancing positive and negative affect" (pp. 368-369) and provides a formula for it:

$$A = (P + N) - |P - N|. \qquad (1)$$

He calls the sum of positivity and negativity a person feels toward an attitude object "total affect" and the absolute difference between these measures "polarization". Formula (1) suggests that ambivalence increases as total affect $(P + N)$ increases and decreases as polarization ($|P - N|$) increases. As pointed out by Priester and Petty (1996), who also coined the term CRM, this formula essentially suggests that ambivalence (*A*) is a positive linear function of the measure (*P*, *N*) exhibiting the smaller value, i.e. the *conflicting* reaction. Thus,

$$A = 2 * Min(P, N). \qquad (2)$$

To illustrate the CRM formula, let us assume that P and N can only take on either a low (e.g., 0) or a high (e.g., 1) value, yielding a 2 × 2 table of possible combinations. In this scenario, we would find an A score of 1 when P and N are both high, and an A score of 0 in all other cases. Thus, the CRM attributes the highest degree of ambivalence to people with strong positive and strong negative feelings toward the attitude object – which is an assumption shared by all ambivalence models – but does not distinguish between all other cells of the simplifying 2 × 2 table. Although the latter assumption is directly challenged by the SIM (discussed below), let us assume for the moment that the theoretical assumptions of the CRM are perfectly reasonable.

Testing the Assumptions of the CRM: Problems with the Current Approach

A straightforward way to test the assumptions of the CRM would be to examine how well it predicts subjective ambivalence. Although a perfect correspondence is not to be expected (cf. Jonas et al., 2000), subjective ambivalence is in fact often regarded as the "gold standard" for ambivalence models (e.g., Thompson et al., 1995).

To begin exploring the possible confounds of the ambivalence index, consider again the simplifying 2 × 2 table. If we have an experimental design with P and N as orthogonal factors and subjective ambivalence as the dependent variable, the assumptions of the CRM could be tested by comparing the High/High cell mean against the other three cell means. The expected pattern would reveal that subjective ambivalence would be greater in the High/High condition than in the remaining conditions, but that subjective ambivalence would not differ across the remaining conditions. It is well known that a hypothesized pattern such as this cannot be tested with a single-degree-of-freedom test (e.g., Abelson, 1996). For instance, although a contrast coded [+3, -1, -1, -1] may suggest itself in the case of the CRM, the associated null hypothesis may be trivially rejected because of a main effect of P or N or both. This is because the above contrast would not be orthogonal to a contrast for a main effect, e.g., [+1, -1, +1, -1]. In a similar vein, Rosnow and Rosenthal conclude from their discussion of omnibus tests: "Whenever we have tested a fixed effect with *df* > 1 for chi square or for the numerator of *F*, we have tested a question in which we almost surely are not interested." (Rosnow & Rosenthal, 1989, p. 1281)

As this example shows, it is useful to distinguish between conceptual and statistical hypotheses. In the example, there is a single conceptual hypothesis: Ambivalence will be positively related to subjective ambivalence. However, at the statistical level, this hypothesis translates into several separate tests. The CRM would be fully supported only if the High/High cell was found (in separate tests) to be different from the remaining cells, and the remaining cells in turn were judged (in separate tests) to be equivalent in terms of subjective ambivalence.

Although ambivalence research operates differently from this simplified example in that it typically relies on continuous measures of *P* and *N*, current data-analytic practice essentially runs the same risk of misspecifying a conceptual hypothesis at the level of statistics. The problem arises when a single variable is constructed on the basis of an equation such as (1) and then used to predict another variable. This more realistic scenario is considered next.

In order to examine the conceptual hypothesis that ambivalence is positively related to some dependent variable (e.g., subjective ambivalence), researchers commonly estimate Pearson's correlations (e.g., Holbrook & Krosnick, 2005; Priester & Petty, 1996; Riketta, 2000; Thompson et al., 1995):

$$\rho_{Y,A} = \frac{\sigma_{Y,A}}{\sigma_Y \sigma_A}, \tag{3}$$

where $\rho_{Y,A}$ denotes the correlation between the conceptual dependent variable *Y* and ambivalence (*A*) calculated according to one of several ambivalence formulae, $\sigma_{Y,A}$ is the covariance of *Y* and *A*, and σ_Y and σ_A are the standard deviations of *Y* and *A*, respectively. Support for the conceptual hypothesis would be claimed if the statistical hypothesis could be rejected that $\rho_{Y,A}$ is zero or less. However, because the ambivalence index is necessarily correlated with the component scores of positivity and negativity, a positive $\rho_{Y,A}$ could simply reflect the fact that either one of the component measures is positively related to the dependent variable. This can be seen more clearly if we write the covariance of *Y* and *A* in such a way that the components of *A* become explicit. In order to do so, we need to reformulate the absolute difference expression in Equation (1) to yield a linear combination of *P* and *N*. The following Equation reproduces the nonlinear CRM formula on the left-hand side, and a mathematically equivalent expression on the right-hand side:

$$(P+N)-|P-N| = (P+N)-(1-2W)(P-N)\,. \tag{4}$$

where, following Edwards (1994), *W* is a dummy variable that is set to 0 when *P* > *N*, and set to 1 when *P* < *N*, and randomly set to 1 or 0 when *P* = *N*. This preserves the qualities of the absolute difference (i.e., polarization) expression in Kaplan's (1972) ambivalence formula, for it guarantees that the smaller value of *P* and *N* will always be subtracted from the larger value. Expanding (4) and simplifying yields

$$(P+N)-|P-N| = (2N+2WP-2WN)\,. \tag{5}$$

Because the covariance of a sum is given by the sum of the covariances, the covariance between the dependent variable and ambivalence can be written as follows:

$$\sigma_{Y,A} = 2\sigma_{Y,N} + 2\sigma_{Y,WP} - 2\sigma_{Y,WN}\,. \tag{6}$$

Equation (6) can be used to determine under which conditions $\rho_{Y,A}$ will be positive. If we assume multivariate normality for *Y*, *P*, and *N*, and expected values of zero (which is tantamount to centering the variables at their means), the terms $\sigma_{Y,WP}$ and $\sigma_{Y,WN}$ can be shown to depend straightforwardly on $\sigma_{Y,P}$ and $\sigma_{Y,N}$, respectively. Given the above assumptions, the distribution of (Y, P, N) is symmetric and thus the same as the distribution of $(-Y, -P, -N)$. It follows that the expectation of W is equal to .5, the covariance of Y and WP is half the covariance of Y and P, and the covariance of Y and WN is half the covariance of Y and N ($\sigma_{Y,WP} = \frac{\sigma_{Y,P}}{2}$ and $\sigma_{Y,WN} = \frac{\sigma_{Y,N}}{2}$).

Substituting these results for $\sigma_{Y,WP}$ and $\sigma_{Y,WN}$ in (6), it becomes evident that the covariance between Y and A calculated according to the CRM will be positive (and therefore, $\rho_{Y,A} > 0$) whenever the sum of the covariances of Y with P and N, respectively, is greater than zero:

$$\sigma_{Y,A} = \sigma_{Y,P} + \sigma_{Y,N} \,. \qquad (7)$$

Since this condition can be satisfied in multiple ways, the correlation coefficient $\rho_{Y,A}$ is difficult to interpret. For instance, subjective ambivalence may be a sole function of negativity. In fact, positivity may even be negatively correlated with subjective ambivalence. All that is required for $\rho_{Y,A}$ to be positive in this case would be that the absolute value of the covariance between subjective ambivalence and negativity is larger than the absolute value of the covariance between subjective ambivalence and positivity. In sum, the current data-analytic approach does not seem to test the intended conceptual hypothesis, which jeopardizes the validity of the conclusions that researchers draw from their research.

Assumptions of the SIM

Thompson, Zanna, and Griffin (1995) argue that Kaplan's (1972) formula does not match his theoretical characterization of ambivalence in terms of (high) total affect and (low) polarization. As we have seen, according to the CRM formula, ambivalence increases as a linear function of the smaller value of P and N. However, if a low degree of polarization of attitude components is an independent aspect of ambivalence, then the measure with the higher value of *P* and *N* should also affect (i.e., decrease) the amount of ambivalence. Thus, the SIM formula slightly modifies the CRM formula as follows:

$$A = \frac{(P+N)}{2} - |P-N|. \quad (8)$$

The assumed impact of the higher value of *P* and *N* becomes apparent if we rewrite (8) according to Priester and Petty (1996):

$$A = 1.5 * Min(P, N) - 0.5 * Max(P, N). \quad (9)$$

To illustrate the SIM formula, consider again the simplifying 2 × 2 table defined by high (e.g., 1) and low (e.g., 0) values of *P* and *N*. Like the CRM, the SIM postulates that the High/High cell would exhibit the highest level of ambivalence. However, the SIM does not assign equal status to the remaining cells. In contrast to the CRM, the Low/Low cell would reveal intermediate levels of ambivalence, because it is equally unpolarized as the High/High cell. Thus, the SIM treats what Kaplan (1972) calls indifference as a milder form of ambivalence, sharing the similarity of *P* and *N*, but lacking the overall intensity of affect.

Testing the Assumptions of the SIM: Problems with the Current Approach

Previous research on the SIM has used the same correlational strategy as research on the CRM, where *P* and *N* are collapsed into a single ambivalence variable that is then used to predict a dependent variable. Thus, in order to reveal possible confounds underlying this strategy, we repeat the steps from Equations (4) to (6) for the SIM. The following Equation reproduces the nonlinear SIM formula on the left-hand side, and a mathematically equivalent expression on the right-hand side:

$$\frac{(P+N)}{2} - |P-N| = \frac{(P+N)}{2} - (1-2W)(P-N), \quad (10)$$

where *W* again is a dummy variable with the same meaning as before. Multiplying out the inner brackets and simplifying yields

$$\frac{(P+N)}{2} - |P-N| = (1.5N - 0.5P + 2WP - 2WN)\,. \tag{11}$$

Under which circumstances will $\rho_{Y,A}$ be positive when ambivalence is calculated according to the SIM, i.e. Equation (11)? As the following equation for the covariance between *Y* and *A* reveals,

$$\sigma_{Y,A} = 1.5\sigma_{Y,N} - 0.5\sigma_{Y,P} + 2\sigma_{Y,WP} - 2\sigma_{Y,WN}\,, \tag{12}$$

$\rho_{Y,A}$ depends on the same terms as in an analysis based on the CRM. Moreover, making the same distributional assumptions as above, we find that the covariance between *Y* and *A* will be positive (and therefore, $\rho_{Y,A} > 0$) under exactly the same circumstances as above, namely whenever the sum of the covariances of *Y* with *P* and *N*, respectively, is greater than zero:

$$\sigma_{Y,A} = \frac{\sigma_{Y,P}}{2} + \frac{\sigma_{Y,N}}{2}\,. \tag{13}$$

Thus, the conditions for a positive correlation between *Y* and *A* are identical whether the CRM or the SIM is used to calculate *A*. However, this conclusion pertains only to the *sign* and not to the *size* of the correlation. A comparison of Equations (7) and (13) reveals that the covariance between *Y* and *A* is twice as large when the CRM rather than the SIM is used to calculate *A*. In order to determine if there is any a priori difference in the size of $\rho_{Y,A}$ given the CRM or the SIM, we need to take into account the standard deviations of the ambivalence indices. As we demonstrate in Appendix B, the a priori ratio of the standard deviations is

$$\frac{\sigma_{A(CRM)}}{\sigma_{A(SIM)}} = \frac{\sqrt{4-\frac{4}{\pi}}}{\sqrt{2.5-\frac{4}{\pi}}} \approx \frac{3}{2}\,. \tag{14}$$

Thus, we can write the correlation between Y and A calculated according to the CRM as a function of the correlation between Y and A calculated according to the SIM:

$$\rho_{Y,A(CRM)} = 2\rho_{Y,A(SIM)}\left(\frac{\sqrt{2.5-\frac{4}{\pi}}}{\sqrt{4-\frac{4}{\pi}}}\right) \approx \frac{4}{3}\rho_{Y,A(SIM)}. \qquad (15)$$

This means that ambivalence calculated according to the CRM is bound to correlate higher with a dependent variable (e.g., subjective ambivalence) than ambivalence calculated according to the SIM. Given the distributional assumptions we made, the CRM correlation is equal to $\frac{4}{3}$ times the SIM correlation. This analytic result contradicts previous empirical comparisons of the CRM and the SIM indices in terms of how well they predict subjective ambivalence. In previous research, the SIM index has often been found to correlate higher (although only slightly so) with subjective ambivalence than the CRM index (e.g., Priester & Petty, 1996; Riketta, 2000; Thompson et al., 1995). This discrepancy can occur because the assumptions we have made are unlikely to be met exactly in any given empirical study. Recall that we assumed a multivariate normal distribution with expected values of zero for the variables *Y*, *P*, and *N*, and in Appendix B we made the further assumption that *P* and *N* are statistically independent. Deviations from these assumptions can change the ratio of the SIM and CRM correlations. However, the important lesson to be drawn from our analytic results is that bivariate correlations not only fail to test the assumptions of a given ambivalence model, but they also fail to distinguish between divergent ambivalence models. It would certainly be an unfair comparison to adjudicate between the CRM and the SIM on the basis of bivariate correlations because the CRM index can be predicted to produce higher correlations than the SIM index (under plausible, if ideal conditions) *before any data are collected*.

Overcoming Problems with Current Data-Analytic Approaches: The Multivariate Approach to Ambivalence Models

The crucial problem revealed above is that the current data-analytic approach treats ambivalence as a single variable, which renders statistical results ambiguous with regard to the validity of the conceptual hypothesis. As a result, it is difficult to tell if the CRM or the SIM can fit empirical data, or – equally important – if one of the models can fit the data better than the other (cf. Roberts & Pashler, 2000).

In the following, we propose a new data-analytic procedure to overcome these difficulties. Our approach is based on a simple rule: *Analyze separately what you measure separately!* Thus, in contrast to what may be called the Univariate Approach to Ambivalence Models (UAAM) which prevails in the ambivalence literature, we propose a Multivariate Approach to Ambivalence Models (MAAM) which allows for unambiguous and competitive tests of and between ambivalence models.

Analyzing the CRM with the MAAM

In order to make the statistical hypotheses and constraints underlying the CRM transparent and testable, we begin with a simple linear regression

$$Y = b_0 + b_1 A + \varepsilon, \qquad (16)$$

where Y refers to the measured dependent variable, A refers to ambivalence calculated according to one of several ambivalence formulae, b_0 is the regression intercept, b_1 is the regression slope, and ε represents the random error term. By substituting (5) for A into (16) and expanding (cf. Edwards, 1994), we obtain

$$Y = b_0 + 2b_1 N + 2b_1 WP - 2b_1 WN + \varepsilon\,. \qquad (17)$$

The implications of Equation (17) are more readily understood by comparing it against a general moderated multiple regression model with the same variables, complemented by the missing lower-order terms W and P that feature in the two-way interactions (see Cohen, Cohen, West, & Aiken, 2003), and the interaction between P and N (see Yzerbyt, Muller, & Judd, 2004),

$$Y = b_0 + b_1 P + b_2 N + b_3 W + b_4 PN + b_5 WP + b_6 WN + \varepsilon \,. \qquad (18)$$

A comparison of Equations (17) and (18) shows that the CRM constrains the general equation in the following ways. First, the effects of *N* and the interaction term *WP* on *Y* are assumed to be equal ($b_2 = b_5$). Second, the effects of *N* and the interaction term *WN* on *Y* are assumed to be opposite in sign but equal in absolute magnitude ($b_2 = -b_6$). These constraints reflect the model assumption that ambivalence effects are carried by the smaller component or conflicting reaction (*P* or *N*) *regardless of valence*. The model assumptions would be violated if either component had a stronger influence on the dependent variable than the other. Finally, the CRM implies that the effects of *P*, *W*, and *PN* should be zero ($b_1 = b_3 = b_4 = 0$). Note that the hypothesized zero effect of *P* does not imply that *P* is irrelevant. Rather, it is due to the coding of *W*, which makes the coefficient associated with *P* a test of the effect of *P* on the outcome when *P* > *N*. According to the CRM, this effect should in fact be zero.

Thus, when ambivalence is assumed to predict an outcome, we recommend that researchers estimate the multiple regression model shown in (18) and test the constraints implied by the ambivalence model. In the case of the CRM, researchers should examine (a) if $b_2 - b_5 = 0$, (b) if $b_2 + b_6 = 0$, and (c) if $b_1 = b_3 = b_4 = 0$, which would be required to claim full support for the ambivalence model. It is also essential to check the effects of single coefficients. For instance, if the conceptual hypothesis states a positive effect of ambivalence, b_2 and b_5 should differ from zero in a positive direction, and b_6 should differ from zero in a negative direction. An overview of the expected pattern of regression coefficients is presented in Table 1. The opposite pattern would be required if the conceptual hypothesis states a negative effect of ambivalence.

However, even if empirical observations were in line with the hypotheses and constraints of the CRM, we would merely know that the model can fit the data. To provide strong support for a theoretical model, it is essential to show that the data cannot be fitted by at least one other plausible model (cf. Roberts & Pashler, 2000). Judging by how often it has been applied, the SIM appears to be the most plausible alternative model. In the next section, we unravel the implications of the MAAM for analyzing the SIM.

Analyzing the SIM with the MAAM

The statistical hypotheses and constraints of the SIM can be derived in the same way as for the CRM, that is, by substituting (11) for *A* into (16) and expanding, which yields:

$$Y = b_0 + 1.5b_1N - 0.5b_1P + 2b_1WP - 2b_1WN + \varepsilon \,. \qquad (19)$$

By comparing (19) with the general moderated multiple regression equation (18) restated here,

$$Y = b_0 + b_1P + b_2N + b_3W + b_4PN + b_5WP + b_6WN + \varepsilon \,,$$

we find that the model assumptions of the SIM translate into the following statistical hypotheses. First, the effect of *N* should be three times the size of the effect of *P*, but opposite in sign ($3b_1 + b_2 = 0$). Second, the effects of *WP* and *WN* should be four times the size of *P*, but with different signs ($4b_1 + b_5 = 0$, and $4b_1 - b_6 = 0$). Third, the effects of *W* and the interaction of *P* and *N* are assumed to be zero ($b_3 = b_4 = 0$). Table 1 presents an overview of the regression coefficients expected on the basis of the CRM and the SIM, assuming a positive effect of ambivalence on a dependent variable. This juxtaposition confirms our earlier conclusion that the CRM and the SIM make quite different predictions regarding the effects of *P* and *N*. In contrast to the univariate approach dominating the ambivalence literature, the MAAM we are proposing makes these predictions transparent and allows for competitive testing between the models. We will illustrate our approach in Studies 1 and 2 below, using the relationship between ambivalence and subjective ambivalence as an empirical example.

Ambivalence as a Moderator Variable

The most frequently investigated (and generally corroborated) hypothesis about ambivalence is that it will attenuate attitude effects (e.g., Armitage & Conner, 2000; Bassili, 1996; Brömer, 2002; Cavazza & Butera, in press; Conner, Povey, Sparks, James, & Shepherd, 2003; Conner, Sherlock, & Orbell, 1998; Conner et al., 2002; Costarelli & Colloca, in press; Dormandy et al., 2006; Moore, 1973, 1980; Povey, Wellens, & Conner, 2001; Sparks et al., 2001; Sparks, Harris, & Lockwood, 2004; Zemborain & Johar, 2007). In other words, the impact of attitudes on subsequently reported attitudes (i.e., attitude stability) or on cognition or behavior should decrease with increasing ambivalence (for reviews in the context of the Theory of Planned Behavior, see Armitage & Conner, 2004; Cooke & Sheeran, 2004). In this section we reveal problems with the current approach to testing this hypothesis.

Testing a Moderator Hypothesis Based on the CRM or the SIM: Problems with the Current Approach

The modal data-analytic strategy to test this moderator hypothesis is to estimate the interaction effect of attitude and an index of ambivalence on some dependent variable, using the following general moderated multiple regression equation (note that our arguments below would similarly apply to ANOVAs after dichotomization of ambivalence, sometimes used in older studies, and multi-sample structural equation models sometimes used in more recent studies):

$$Y = b_0 + b_1 X + b_2 A + b_3 XA + \varepsilon, \qquad (20)$$

where *Y* is the dependent variable, *X* is the independent variable (i.e., in our example, attitude), *A* is ambivalence calculated according to a given ambivalence model, and *XA* is the product term whose coefficient b_3 represents the interactive effect of *X* and *A* on *Y*.

A negative b_3 is typically interpreted as support for the hypothesis that ambivalence attenuates attitude effects. Thus, we deem it essential to understand the conditions under which the hypothesized negative interaction can occur, that is, when estimates of b_3 will be negative. As we demonstrate in Appendix C, a negative b_3 is likely to occur even if no interaction effect exists in the population! Furthermore, the answer to this question does not depend on whether the CRM or the SIM is used to calculate *A*. The formal proofs for this assertion (provided in Appendices A and C) can be summarized in conceptual terms as follows. First, we use a general formula for the interaction effect b_3 and express all terms involving *A* as linear combinations of the underlying variables *P*, *N*, and the dummy variable *W* (see above). Second, we assume multivariate normality for the measured variables *Y*, *X*, *P* and *N*. Although all interactions of centered variables are zero under multivariate normality (Aiken & West, 1991), a linear expression of the ambivalence models necessitates the use of a dummy variable *W*, for which normality cannot be assumed. In order to determine the sign of b_3 in the population model, we must make further assumptions. The simplest assumption to make in this context is that the covariances of *P* and *X* or *Y* are positive, and the covariances between *N* and *X* or *Y* are negative but of the same absolute magnitude as those of *P* ($\sigma_{\mathrm{Y,P}} = -\sigma_{\mathrm{Y,N}}$ and $\sigma_{\mathrm{X,P}} = -\sigma_{\mathrm{X,N}}$). Furthermore, we assume that *P* and *N* are uncorrelated.

Given these assumptions, the sign of b_3 must be negative when $\frac{\rho_{Y,N}}{\rho_{X,N}} > \rho_{X,Y}$ (see Appendix C). This means that a moderator artifact can occur when the correlation of *N* and *X* is equal to the correlation of *N* and *Y*, or more generally, when the ratio of these correlations is larger than the correlation of *X* and *Y*. By substituting plausible values into the inequality $\frac{\rho_{Y,N}}{\rho_{X,N}} > \rho_{X,Y}$, one can verify that this condition is easily met. For instance, the inequality holds if *X* (attitude) and *Y* (e.g., a corresponding behavior) correlate at $\rho = .3$, and *N* (the negative attitude component) correlates with *X* and *Y* at $\rho = -.2$ and $\rho = -.1$, respectively.

This result is simple – but it depends on a number of assumptions we have made in the course of its derivation which are worth restating here. First, *Y*, *X*, *P* and *N* are multivariate normal with zero means and unit variances. Second, *P* and *N* are uncorrelated, which is a widespread theoretical assumption (Cacioppo & Berntson, 1994; Kaplan, 1972; Scott, 1969) and confirmed by our own empirical results reported below. Third, the covariances between *P* and *X* or *Y* are positive, and the covariances between *N* and *X* or *Y* are negative but of the same absolute magnitude as those of *P*. The symmetry of the effects of *P* and *N* on attitude is not an assumption we made lightly. Most notably, the model of evaluative space by Cacioppo and colleagues (Cacioppo & Berntson, 1994; Cacioppo, Gardner, & Berntson, 1997) suggests a steeper activation function for negativity vs. positivity. However, Cacioppo and colleagues also note that the symmetry assumption is made by all ambivalence models (Cacioppo et al., 1997), so we considered it appropriate to use it in deriving the implications of two of these models. The symmetry assumption is also supported by the average correlations across six different samples reported in Kaplan (1972, p. 370) as well as our own data.

In sum, our decomposition of b_3 reveals three serious problems with the current univariate approach to testing a moderator hypothesis about ambivalence. First, a negative b_3 is difficult to interpret because its conditions can be satisfied in multiple ways. Second, it follows that the univariate approach is unable to adjudicate between the competing predictions of the CRM and the SIM. Third, since a sufficient condition for a negative b_3 does not include any product terms, researchers may easily interpret a moderator artifact as support for their theory. We will discuss this artifact in greater depth in Studies 3 to 5.

Using the MAAM to Test a Moderator Hypothesis About Ambivalence

A multivariate treatment of ambivalence is necessary to circumvent the three problems revealed above. Therefore, we again use the rule *Analyze separately what you measure separately!* and derive the multivariate hypotheses and constraints implied by the CRM and the SIM. Substituting the CRM formula (5) into Equation (20), we obtain

$$\begin{aligned} Y &= b_0 + b_1 X + 2b_2 N + 2b_2 WP - 2b_2 WN \\ &+ 2b_3 XN + 2b_3 XWP - 2b_3 XWN + \varepsilon \end{aligned} . \tag{21}$$

Analogously substituting the SIM formula (11) into Equation (20), we obtain

$$\begin{aligned} Y &= b_0 + b_1 X - 0.5b_2 P + 1.5b_2 N + 2b_2 WP - 2b_2 WN \\ &- 0.5b_3 XP + 1.5b_3 XN + 2b_3 XWP - 2b_3 XWN + \varepsilon \end{aligned} . \tag{22}$$

The implied constraints follow from comparisons of Equations (21) and (22) with a general moderated multiple regression equation, complemented by the lower-order terms and all interactions (Cohen et al., 2003; Yzerbyt et al., 2004) absent from (21) and (22):

$$\begin{aligned} Y &= b_0 + b_1 X + b_2 P + b_3 N + b_4 W + b_5 XP + b_6 XN + b_7 XW + b_8 PN \\ &+ b_9 WP + b_{10} WN + b_{11} XWP + b_{12} XWN + b_{13} XPN + b_{14} WPN + \varepsilon \end{aligned} \tag{23}$$

This is the multiple regression model that we suggest researchers should estimate in order to test for moderator effects of ambivalence. By estimating the model shown in (23), researchers can rule out spurious moderator effects as revealed above. Furthermore, unlike the UAAM, this approach allows for direct comparisons of the CRM and SIM. Table 2 provides an overview of the expected regression coefficients given the CRM and the SIM.

Summary and Overview of Empirical Studies

The discussion of ambivalence models so far has shown that current data-analytic procedures can produce ambiguous and misleading results under plausible assumptions. This alone suggests that researchers are discarding information about the validity of the reported effects by relying on ambivalence indices in the way discussed above. However, the consequences of this information loss would be less dramatic if the substantive conclusions about actual empirical data would remain unchanged by the proposed alternative data-analytic procedure. Thus, we conducted four studies to examine empirically if the Univariate Approach to Ambivalence Models (UAAM) does in fact lead to different conclusions than the proposed Multivariate Approach to Ambivalence Models (MAAM).

All studies were conducted in the context of intergroup attitudes where ambivalence is assumed to play a major role. Prejudice research has long recognized the potential of outgroups to elicit ambivalence. Many different sources of ambivalence have been specified for different outgroups, including moral dilemmas (e.g., Allport, 1954, pp. 326-339; Crandall & Eshleman, 2003, pp. 433-434; Katz, 1981; Katz, Glass, & Wackenhut, 1986; Katz & Hass, 1988; Myrdal, 1944) and mixed stereotype content (e.g., Fiske, Cuddy, Glick, & Xu, 2002; Glick & Fiske, 1996). However, ambivalence in intergroup contexts has never been explicitly addressed from an attitude strength perspective, which is perhaps due to social psychology's general trend toward fragmentation (Kruglanski, 2001).

In order to foster integration of research on attitude strength and intergroup attitudes, Studies 1 and 2 examine ambivalence as a predictor of subjective reports of ambivalence toward an outgroup. Study 3 then tests the attitude strength hypothesis that ambivalence would moderate the temporal stability of outgroup attitudes. Study 4 examines a hypothesis jointly derived from the intergroup and attitude strength literatures, namely that ambivalence would lead to more extreme responses to outgroup members, that is, response amplification (Katz et al., 1986). All four studies compare predictions of the CRM and the SIM and results obtained based on the UAAM and the MAAM. Finally, we present a simulation study to gauge the likelihood that the UAAM produces statistical artifacts that could be interpreted as support for a moderator hypothesis about ambivalence.

Study 1: Ambivalence Predicting Subjective Ambivalence

Our first study tested if the CRM or the SIM would better account for the relationship between ambivalence and subjective ambivalence in German participants' attitudes toward Turks living in Germany.

Method

Participants

One-hundred and forty-four (27 male, 117 female) undergraduates from a middle-sized German university filled out a questionnaire booklet at the end of a social psychology lecture in exchange for research credit points. The age range was from 18 to 29 years with a median of 21 years.

Measures

Positivity and Negativity. We used the standard split semantic differential method (Kaplan, 1972) to measure positivity and negativity towards Turks in Germany. For instance, participants were asked to "consider only the positive qualities of Turks and ignore their negative ones. How positive are the positive qualities of Turks in Germany?" The unipolar response scale had four options: 0 (not at all positive), 1 (slightly positive), 2 (quite positive), 3 (extremely positive). Then they were asked the corresponding question about the negative qualities of Turks. A second item pair referred to pleasant vs. unpleasant qualities. The order of positively and negatively valenced items was counterbalanced. Responses to individual items were averaged to obtain measures of positivity ($\alpha = .78$) and negativity ($\alpha = .83$) which were slightly positively correlated at $r = .14$, $p = .10$. Higher values indicate more positivity or negativity, respectively.

Subjective Ambivalence. Participants responded to the following three items on a scale from 1 (do not agree at all) to 6 (agree completely): "Regarding the issue 'Turks in Germany', I find it hard to be pro or contra", "I have mixed feelings toward Turks in

Germany", "My opinion of Turks in Germany is undecided." Subjective ambivalence scores were obtained by averaging across items ($\alpha = .77$).

Results

In the interest of comparability with previous research, we first estimated the bivariate correlations between subjective ambivalence and the indices suggested by the CRM and the SIM, respectively. In line with previous findings (e.g., Priester & Petty, 1996; Riketta, 2000; Thompson et al., 1995), these correlations were highly similar in direction and size. Subjective ambivalence was correlated at $r = .33$ with the CRM-index and at $r = .36$ with the SIM index. However, as we have demonstrated above, these correlations are mute with regard to the validity of the conceptual hypothesis of a positive relationship of ambivalence with subjective ambivalence.

In order to test the predictions of the CRM and the SIM, we estimated the moderated multiple regression model shown in Equation (18), using subjective ambivalence as dependent variable. The measures of positivity and negativity were standardized to achieve comparability of scale units before the dummy variable W and the product terms were constructed. The model explained 30 % of the variance in subjective ambivalence, $F(6, 137) = 9.64$, $p < .001$. The estimates of the regression coefficients along with the 95% confidence interval for the population value are shown on the left-hand side of Table 2.

A comparison of Tables 1 and 2 indicates that the results are fully consistent with the assumptions of the CRM. Specifically, the confidence intervals of the effects of N and WP include only positive values, and the confidence interval of the effect of WN includes only negative values. As a set, these results represent a necessary condition for the validity of the claim of a positive effect of ambivalence on subjective ambivalence from the perspective of both the CRM and the SIM. However, the SIM additionally stipulates a negative effect of P, which is in line with the sample value we obtained but inconsistent with the confidence interval which also includes positive values. At the same time, this result satisfies the first constraint of the CRM. In addition, the CRM constrains the effect of *W* to be zero. Although this null hypothesis cannot be rejected, considering that the associated confidence interval includes zero, it should be noted that the interval extends well into the positive range so that a larger sample size would have likely resulted in a rejection. Conceptually, the positive coefficient of *W* suggests that people with a relative excess of negativity towards Turks (versus positivity) report higher levels of subjective ambivalence toward Turks than people with a relative excess of positivity.

Equality constraints were tested as follows. Coefficients that a model postulates to be equal were subtracted from another and divided by the standard error of this linear combination. The resulting test statistic is *t*-distributed with $n - k - 1$ degrees of freedom, where n is the sample size and k the number of variables in the multiple regression model (including the regression intercept). In formal terms, this procedure is summarized by the following equation

$$t_{n-k-1} = \frac{w_1 * b_1 + w_2 * b_2}{\sqrt{w_1^2 * \sigma_{b1}^2 + w_2^2 * \sigma_{b2}^2 + 2 * w_1 * w_2 * \sigma_{b1,b2}}}$$

where w refers to the weight a coefficient is supposed to receive according the ambivalence model, and σ_{b1}^2, σ_{b2}^2, and $\sigma_{b1,b2}$ refer to the variances and covariance of the regression coefficients as can be obtained from the variance/covariance matrix that can be requested from most statistical software packages. For instance, the first CRM constraint $b_5 = b_2$ (see Table 1) would suggest that one take the sum of b_2 and b_5 with weights (1) and (-1). A more detailed discussion of such linear hypothesis tests can be found in Fox (1997). Results of our tests of the equality constraints of the CRM indicate that the effects of *N* and *WP* are equal, $t(137) = .65$, $p = .52$, and that the effects of *N* and *WN* sum to zero, $t(137) = .12$, $p = .90$.

In sum, we found support for all of the CRM's statistical hypotheses and constraints, including those that are at odds with the SIM. This can be confirmed by inspecting Figure 1 which shows three-dimensional depictions of the theoretical predictions of the CRM (top left) and the SIM (top right) as well as the response surface suggested by our data (bottom left). The empirical response surface was obtained by plotting the subjective ambivalence values implied by local regression fitting (see Cohen et al., 2003). This procedure allows us to examine the multivariate relationship between subjective ambivalence and the independent variables positivity and negativity without imposing any restrictions on the form of the relationship (except that we only modeled a polynomial fit of the first degree for the sake of interpretability). Note especially the lines extending from the main diagonal line connecting the minimum/minimum and maximum/maximum corners. According to the SIM, these lines should have a downward slope (see the top right panel). In the bottom-left panel, these lines have a slight downward slope only for increases in positivity.

Discussion

Participants of Study 1 whose positive attitude components were weaker than their negative ones reported more ambivalence about Turks the stronger the positive components of their attitude toward Turks. Similarly, participants whose negative attitude components were weaker than their positive ones reported more ambivalence the stronger their negative attitude components. This pattern of results confirms the CRM which says that ambivalence is a sole function of the minimum of positivity and negativity, that is, the conflicting reaction.

In contrast, evidence for the (negative) effect of the maximum of positivity and negativity as predicted by the SIM was much weaker. We found only a non-significant negative effect of positivity for participants with a relative excess of positivity versus negativity toward Turks. These results suggest that the CRM explains subjective ambivalence better than the SIM. From a methodological perspective, Study 1 illustrates the superiority of our multivariate approach over the bivariate correlations between the ambivalence index variables and subjective ambivalence which could not detect the violations of the assumptions of the SIM.

Study 2: Ambivalence Predicting Subjective Ambivalence (Replication)

To assess the generalizability of the results of Study 1, we conducted a replication study within a different intergroup context. More specifically, we examined the mutual attitudes of German and Jewish adults.

Method

Participants

One-hundred and seventeen (65 German, 52 Jewish) people volunteered to complete an internet survey about "political messages in the context of German and Jewish relations". They were recruited via e-mail invitations sent by students at a large German university and a large university in Israel. Group membership was verified by asking participants to self-categorize as "German", "Jewish", or "other" at the beginning of the survey. The age range was from 18 to 58 years with a median of 27 years (6 participants chose not to report their age). The German subsample was roughly 4 years younger than the Jewish subsample, $t(109) = 7.05$, $p < .001$. Seventy-six percent of participants were female. Gender composition did not differ across subsamples, $\chi^2(1) = .15$, $p = .84$.

Measures

Positivity and Negativity. Positive and negative attitude components were assessed in the same way as in Study 1, except that we used only one item for each construct (i.e., positive vs. negative qualities of the outgroup) for economical reasons. Response options were from 1 (not at all positive/negative) to 5 (extremely positive/negative). The order of presentation of these items was randomly determined. Positivity and negativity were slightly negatively correlated at $r = -.17$, $p = .07$.

Subjective ambivalence. We assessed subjective ambivalence with one Likert item "I have mixed feelings toward [outgroup]". Out of the three items that we used in Study 1, this was the item with the highest item-total correlation and the highest face validity. The response scale was from 1 to 5, with higher values indicating more subjective ambivalence.

Results

Bivariate correlations between subjective ambivalence and the index variables suggested by the CRM and the SIM were computed for comparison purposes. As in Study 1, these correlations were very similar, $r = .37$ ($p < .001$) for the CRM, and $r = .38$ ($p < .001$) for the SIM.

In order to test the assumptions of the CRM and the SIM, we estimated the same moderated multiple regression model as in Study 1. Preliminary analyses indicated that none of our predictor variables interacted with a dummy variable distinguishing the German from the Jewish subsample (all t's < .51, all p's > .61). It should be noted that our sample size afforded us relatively low power for detecting such interactions. However, our motivation was less to test for cross-cultural differences in and of themselves than to make sure we would not miss any large group differences if they existed. Thus, we dropped the group variable from our analyses.

The multiple regression model explained 23% of the variance in subjective outgroup ambivalence, $F(6, 110) = 5.43$, $p < .001$. Compared with Study 1, this lower R^2 value is probably attributable to the lower reliability of our single-item measures. The estimated regression coefficients are shown on the right-hand side of Table 3. The pattern of results was very similar to that obtained in Study 1. All hypotheses of the CRM found support with the exception of a negative effect for the term *WN*, the confidence interval of which extended slightly into the positive range. Overall, however, results again favor the CRM. The SIM's prediction of a negative effect for the maximum of positivity and negativity was not supported, although there was again a tendency for positivity to decrease subjective ambivalence for participants with an excess of positivity vs. negativity (i.e., a negative but non-significant b_1). Therefore, we proceeded to test the constraints of the CRM only. Results indicated that the coefficients of *N* and *WP* can be regarded as equal, $t(110) = .28$, $p = .78$, and that the coefficients of *N* and *WN* sum to zero, $t(110) = .06$, $p = .95$.

Discussion

Study 2 largely replicated the results of Study 1 in a different intergroup context. We found greater support for the CRM than for the SIM, which can be confirmed by inspecting the empirically derived response surface of the Study 2 data shown in the bottom right panel of Figure 1. Note again the lines extending from the main diagonal connecting the minimum/minimum and maximum/maximum coordinates. A slight downward slope is discernible only for extreme increases in positivity.

In sum, Studies 1 and 2 support the CRM more than the SIM and reveal the superiority of the MAAM over the UAAM which would have been unable to differentiate between the SIM and the CRM. Studies 3 and 4 will now apply the MAAM to the case of ambivalence as a moderator variable.

Study 3: Ambivalence as Moderator of Attitude Stability

A frequent finding in ambivalence research is the attenuating influence of ambivalence on attitude-behavior or attitude-intention relations (cf. Armitage & Conner, 2004; Cooke & Sheeran, 2004). Likewise, ambivalence has been found to decrease the temporal stability of attitudes in such diverse domains as dieting (Armitage & Conner, 2000) and voting behavior (Lavine, 2001). Although the time lag between attitude measurements has typically been much longer in previous research, the literature on context effects in attitude surveys (e.g., Sudman, Bradburn, & Schwarz, 1996; Tourangeau & Rasinski, 1988) strongly suggests that attitude reports can exhibit low stability within one and the same interview or questionnaire. Thus, we examined ambivalence as a moderator of the short-term stability of attitude reports. If such a moderator effect existed, ambivalence measures could be profitably included in attitude surveys to assess the stability of other attitude measures.

Method

Participants and Procedure

The data for the present study are from a larger telephone survey project conducted in 2005 by a middle-sized German university. A relatively long questionnaire (average duration = 22.6 minutes, SD = 7.85) about minority groups in Germany was administered by a university-based survey institute. The interviewers used a random digit dialing procedure to obtain a random sample of Western German adults. Out of the 599 participants initially willing to complete the survey, a total of N = 385 participants were of German nationality and answered all questions relevant to our analyses. The age range of these participants was from 17 to 91 years with a median of 47 years. Due to a technical error, we have no records of the gender composition of the sample. In terms of formal education, the sample was quite diverse. Twenty-one percent of participants had a university or college degree, 23% had a school degree qualifying for university or college, and 52% had a lower school degree.

Measures

Attitude toward Turks. One of the first questions of the survey was “In general, how sympathetic do you find Turks living in Germany?” The response options were 1 (very unsympathetic), 2 (rather unsympathetic), 3 (neither sympathetic nor unsympathetic), 4 (rather sympathetic), and 5 (very sympathetic). This is our measure of outgroup attitude at Time 1. The same question was asked again at the end of the survey. Interviewers were instructed to inform participants who were suspicious of the real purpose of repeating this question that a data recording error had occurred during the first time. The attitude measurements at Time 1 and Time 2 were correlated at $r = .59$, $p < .001$.

Positivity and negativity. Our measures of positivity and negativity were the same two items as in Study 1. The order of positively and negatively valenced items was counterbalanced across participants. Responses to items were averaged to obtain overall scores of positivity (α = .76) and negativity (α = .84) and then standardized to put them on the same scale. Positivity and negativity were virtually uncorrelated, r = -.06, p = .24.

Results

As in Studies 1 and 2, we first used the traditional method of constructing index variables of ambivalence to test the moderator hypothesis. More specifically, for the CRM as well as the SIM, we estimated a moderated multiple regression model with attitude at Time 2 as dependent variable and attitude at Time 1, the ambivalence index, and the product of attitude at Time 1 and the ambivalence index as predictor variables. Attitude at Time 1 and the ambivalence index were standardized before the interaction term was calculated (Cohen et al., 2003; Dimitruk, Schermelleh-Engel, Kelava, & Moosbrugger, in press).

Compared with a model containing only additive effects, the moderated multiple regression explained 1% (using the CRM index variable) or 2% (using the SIM index variable) more variance in attitude at Time 2, indicating a moderator effect, F_{Δ}’s(1, 381) > 11.79, p’s < .001. As can be seen in Table 4, the coefficients were consistent with the hypothesis of an attenuating moderator effect of ambivalence.

However, as our decomposition of this moderator effect has shown, we cannot trust this result. In order to actually test the predictions of the CRM and the SIM, we estimated the moderated multiple regression model shown in Equation (23). This model explained 44% of the variance in outgroup attitudes at Time 2, F(14,370), = 20.67, p < .001. Although this result suggests a sizeable increase in explained variance of 10% compared with the squared bivariate correlation between attitudes at Time 1 and Time 2 ($r^2 = .34$), it is important to note that this increase is due to the inclusion of k = 13 additional predictor variables.

Inspection of the individual coefficients of these variables indicates a complete absence of a moderator effect (see Table 5). More specifically, the only coefficient whose confidence interval does *not* include zero is b_1, i.e. the unsurprising positive effect of attitude at Time 1.

Thus, we found no support for the assumed moderator effect of ambivalence, which would have unfolded as the pattern of regression coefficients shown in Table 2, if the assumptions of the CRM or the SIM were true. The moderator effect previously obtained with the ambivalence-index variables must be considered a statistical artifact in line with our earlier analytic results.

Discussion

In Study 3, we found a relatively low temporal stability of a single-item outgroup measure (r = .59) that was administered twice within one and the same telephone survey of approximately 20 minutes duration. Based on the attitude strength perspective we expected that ambivalence would moderate this attitude-attitude effect such that higher stability estimates would obtain for less ambivalent participants. Indeed, the UAAM would have supported this hypothesis. However, the MAAM we have proposed revealed the moderator effect to be spurious. Thus, the empirical results of Study 3 confirm that the information loss implied by the UAAM actually leads to different substantive conclusions than the MAAM.

Study 4: Ambivalence as Moderator of Response Amplification

As we noted above, research on intergroup attitudes and prejudice has often considered ambivalence as a cause of extreme responses toward outgroup members. Interestingly, the attitude strength perspective would generate exactly the same predictions as prejudice theory for one extensively studied intergroup phenomenon: *response amplification* among people holding ambivalent outgroup attitudes (Bell & Esses, 1997, 2002; Carver, Gibbons, Stephan, Glass, & Katz, 1979; Gibbons, Stephan, Stephenson, & Petty, 1980; Hass, Katz, Rizzo, Bailey, & Eisenstadt, 1991; Hass, Katz, Rizzo, Bailey, & Moore, 1992; Katz, Cohen, & Glass, 1975; Katz et al., 1986; MacDonald & Zanna, 1998). Response amplification is indicated when responses to members of stigmatized groups are more extreme (in a positive or negative direction, depending on the situation) than responses to members of non-stigmatized groups (Katz et al., 1986), or equivalently, when people with ambivalent attitudes toward an outgroup exhibit more extreme responses to members of this group than people with non-ambivalent outgroup attitudes (Bell & Esses, 2002). Traditional explanations would focus on threat to self-esteem or guilt (Katz, Glass, Lucido, & Farber, 1979), negative affect (Hass et al., 1992), or a motivation to reduce ambivalence (Bell & Esses, 2002) as mediators of the link between ambivalence and response amplification.

In contrast, the attitude strength perspective would account for response amplification as follows. Ambivalent outgroup attitudes should be less accessible from memory (Bargh, Chaiken, Govender, & Pratto, 1992; Eagly & Chaiken, 1998; Lavine, Borgida, & Sullivan, 2000). Thus, when ambivalent people encounter an “attitudinally relevant behavioral opportunity” (Fazio, 1995, p.272), such as an interaction with an outgroup member, their attitudes will be less likely to guide their behavior than the cues inherent in the immediate situation. This is exactly the typical finding in the response amplification paradigm. The difference in responses between positively and negatively framed experimental situations is larger for target individuals from a stigmatized outgroup (i.e., one that is viewed in more ambivalent terms) or for participants with ambivalent attitudes toward the target group.

In Study 4, we put this reasoning to an empirical test, comparing the UAAM and the MAAM. We predicted that differences between participants’ responses in positively and negatively framed situations would be larger the higher their ambivalence (cf. Bell & Esses, 2002; MacDonald & Zanna, 1998).

Method

Participants and Design

The sample used for the present study is a subsample of the Study 1 sample. A total of 86 (73 female and 13 male) undergraduates participated in individual testing sessions conducted one to three days after Study 1. A comparison of the full sample used in Study 1 and the reduced sample indicates that slightly more men than women dropped out of the sample, although this difference was not significant, $\chi^2(1) = 1.85$, $p = .20$. The reduced sample was on average .80 years younger than the full sample, $t(142) = 2.43$, $p < .01$. We also checked for systematic differences in positivity and negativity between the full and the reduced samples but found only negligible differences, t's(142) < .54, p's > .59.

Participants were randomly assigned to read a positive ($n = 49$) or a negative ($n = 37$) description of a Turkish woman.

Procedure

Upon their arrival in the laboratory, participants were instructed to read a short newspaper article about a Turkish woman filing a lawsuit and to answer a few questions about the woman described therein. The articles were taken from actual newspapers. In the positive frame condition, participants read about a Turkish woman who was denigrated by a TV show host who made her appear as a drug dealer (*Die Zeit*, 06/08/2005). The article was supposed to elicit sympathy towards the Turkish woman. In the negative frame condition, participants read about a Turkish employee of a German kindergarten who got fired because she celebrated Ramadan with the kindergarten children without obtaining consent from the parents (*Oberhessische Presse*, 06/06/2005). The article was supposed to elicit blame towards the Turkish woman. After reading the newspaper article, participants answered a few items about the Turkish woman and were thanked, debriefed and dismissed.

Measures

The measures of positivity and negativity that we used to model ambivalence had been taken as part of Study 1 and are described above. Responses to the Turkish woman were measured with three bipolar items with the endpoints of the 7-point response scale labeled as "was treated fairly" (1) and "was treated unfairly" (7), "is unsympathetic" (1) and "is sympathetic" (7), and "does not deserve compensation" (1) and "deserves compensation" (7). The internal consistency of these items was good ($\alpha = .85$), so we averaged responses across items.

Results

Preliminary analyses indicated that the two Turkish women described in the newspaper articles elicited positive vs. negative responses as desired. In the positive frame condition the responses ($M = 6.04$, $SD = .77$) were clearly more positive than in the negative frame condition, $M = 3.63$, $SD = 1.03$, $t(84) = 12.41$, $p < .001$.

As our main interest lay in a comparison of the UAAM and the MAAM, we first estimated the moderator effect of ambivalence based on the index variables suggested by the CRM and the SIM using Equation (20). In this example, X is a dummy variable coding the experimental condition (0 for the positive frame condition, and 1 for the negative frame condition). The ambivalence index variables were standardized before the product term was calculated.

Compared with a model containing only additive effects ($R^2 = .68$), this moderated multiple regression explained 2% additional variance in responses to the Turkish woman (regardless of the index used), indicating a moderator effect, F_{Δ}'s$(1, 82) > 4.29$, p's $< .05$. As can be seen in Table 6, the coefficients were consistent with the hypothesis of an attenuating moderator effect of ambivalence. The negative sign of the coefficient for the interaction suggests that the difference in responses between the positive and negative frame conditions increased with participants' level of ambivalence.

In order to be able to compare the results of the UAAM with the MAAM, we estimated a moderated multiple regression model based on Equation (23). Note that the coding of experimental condition X was chosen so that a negative moderator effect could be expected for ambivalence and Table 2 would again provide the expected pattern of regression coefficients given the MAAM. The full model implied by the MAAM explained 71% of the variance in responses to the Turkish woman, $F(14,71) = 12.69$, $p < .001$. The estimated regression coefficients are shown on the right-hand side of Table 5. Apart from the known effect of X (experimental condition), the only coefficient whose confidence interval did not include zero was the one associated with the product term XN. This coefficient was signed in accordance with the predictions of the CRM and the SIM. However, in the absence of support for all other hypotheses of the CRM or the SIM, this effect cannot be attributed to ambivalence. Rather, the effect is more appropriately interpreted as suggesting a larger difference in responses between positive and negative situations for participants who reported more negativity towards Turks. Thus, the moderator effect previously obtained with the ambivalence-index variables must be considered a statistical artifact in line with our earlier analytic results.

Discussion

Based on the attitude strength perspective (e.g., Petty & Krosnick, 1995) as well as intergroup ambivalence theory (e.g., Katz et al., 1986), we predicted that participants exhibiting greater outgroup ambivalence would respond more extremely to an outgroup member, resulting in a larger difference in evaluations between positively and negatively framed target persons. Whereas the UAAM would have supported our prediction, the MAAM revealed this moderator effect to be spurious. It is important to note the present study's low statistical power to detect effects in accordance with the CRM or the SIM (which is due to the fact that the study was designed when we still considered the UAAM with its lower number of parameters a viable approach). Although we hesitate to reject the moderator hypothesis about ambivalence based on the present study, the pattern of results was virtually identical to those obtained in Study 3. Thus, like Study 3, the present study demonstrates that conclusions based on the UAAM and the MAAM can differ strikingly.

Study 5: Monte Carlo Simulation

We have shown that the validity of conclusions based on the UAAM is seriously undermined by the confounding of the ambivalence indices with their components. Studies 1 to 4 have illustrated how conclusions about actual empirical data change when the proposed MAAM is used. Considered together, our empirical findings may seem to cast a shadow of doubt on previously accepted findings in the ambivalence literature. First, it may appear doubtful that the SIM can explain subjective ambivalence as well as the CRM. Second, it may appear doubtful that ambivalence moderates attitude effects as reported in the literature cited above. At the same time, the studies reported here are the only studies relying on the MAAM, and a reinterpretation of the weight of evidence in favor of the established hypotheses should not be based on this limited set of studies.

Nevertheless, we considered it important to gauge the likelihood that previous studies have erroneously interpreted a moderator artifact such as was obtained in Studies 3 and 4 as support for the theory. Therefore, we conducted Monte Carlo simulations of the moderator effect as it would be analyzed by the UAAM and the MAAM. These simulations allow us to quantify the likelihood of false positive reports.

Method

We drew k = 100,000 samples with sample size n = 150 from a population in which X was correlated at r = .4 with P and at r = -.4 with N. All variances were set equal to one. Based on our own data as well as typical reports in the literature (e.g., see Cacioppo et al., 1997; Jonas et al., 2000), P and N were assumed to be independent. Note that k was selected with respect to accuracy of the results, and n was selected with respect to the typical sample size in the literature. For simplicity, we assumed all variables to be continuous and measured without error, which cannot be assumed in practice. Therefore, we chose somewhat lower values of the population covariances than might be expected on a theoretical basis when *X* represents attitude and *P* and *N* refer to the positive and negative attitude bases, respectively.

For each sample, the dependent variable *Y* was first calculated as a linear combination of *X*, *P*, and *N* ($Y = X + P - N$) which results in a variance of *Y* equal to 4.6. Then we added random errors with a mean of zero and a variance of nine times the size of the variance of Y (i.e., 41.4), so that a total of 11.11% of the variance of Y would on average be explained by X, P, and N. This value was chosen to reflect the fact that most studies reporting moderator effects of ambivalence have looked at dependent variables that are typically only moderately correlated with attitude (e.g., a corresponding behavior or attitude observed on a later occasion).

After each sample was drawn, we estimated the regression models suggested by the UAAM, i.e. Equation (20), using the ambivalence indices based on the CRM and the SIM, and the MAAM, i.e. Equation (23). We stored the vectors containing the regression estimates and calculated the lower and upper bounds of the 95% confidence interval around each estimate.

Results and Discussion

Table 7 provides the results for the UAAM, including the average regression coefficients as well as the percentage of Type I and Type II errors. Because *Y* was modeled as an additive function of *X*, *P* and *N* ($Y = X + P - N + \varepsilon$), the true moderator effect b_3 is equal to zero and all rejections of this null hypothesis constitute Type I errors. We counted Type I errors by observing the number of times that the upper bound of the 95% confidence interval for a parameter was lower than zero. In a correctly specified model, the percentage of Type I errors should be around 2.5%. As can be seen in Table 7, the UAAM produces a strikingly inflated Type I error rate with 9.4% false rejections when the CRM index is used and 14.6%

false rejections when the SIM index is used. Thus, the Monte Carlo simulations confirm our suspicion that the moderator artifacts obtained in Studies 3 and 4 can occur quite frequently, namely up to six times more frequently than would be expected on the basis of an alpha level of .05.

In contrast, inspection of Table 8 indicates that the MAAM recovered the true effects. Note that the averages of all regression coefficients other than those of X, P and N are zero with reasonable Type I error rates. However, as a result of the increased number of estimated parameters, the Type II error rates for the true effects were above 90%. In light of the conventional levels of 20%, these values are clearly unacceptable. These results serve to remind us that applications of the MAAM require much larger samples than the sample size of n = 150 used here, which produced acceptable Type II error rates for the UAAM.

General Discussion

Ambivalence is a central concept in research on intergroup relations (e.g., Katz et al., 1986) and, more generally, within the attitude strength perspective (e.g., Petty & Krosnick, 1995) which is applied across many psychological subdisciplines. The current surge of ambivalence research owes much to a minority of scholars consistently arguing for the theoretical independence of positive and negative attitude bases (Cacioppo & Berntson, 1994; Kaplan, 1972; Scott, 1969), and the proliferation of several models of how positivity and negativity combine to produce the feeling of ambivalence. Apparently, the field could not be better prepared to improve our understanding of the nature of ambivalence. However, the methodology used in previous research suffers from a set of shortcomings that undermine the validity of empirical conclusions that can be drawn from this research.

The core of the problem is that previous research has tended to combine two theoretically and empirically independent variables (i.e., positivity and negativity) into a single ambivalence index, which is then subjected to further statistical analyses. It is important to note that there are exceptions to this approach. Probably the most sophisticated and comprehensive empirical analysis of attitude ambivalence can be found in the research of Priester and Petty (1996), who examined subjective ambivalence as a function of the positive and negative attitude bases in order to derive a new ambivalence formula. However, their statistical analyses were based on dominant vs. conflicting reactions, that is, variables reflecting the minimum vs. maximum of positivity and negativity. Like the ambivalence indices, in fact, like any variable that is constructed on the basis of theoretically distinct component variables, the dominant vs. conflicting reactions confound the separate effects of their components. Mutatis mutandis, then, even this otherwise excellent work can be subsumed under what we have termed the Univariate Approach to Ambivalence Models (UAAM) which dominates the literature to date.

In the present article, we have pointed out three serious problems with the UAAM, focusing on the Conflicting Reactions Model (CRM, Kaplan, 1972) and the Similarity-Intensity Model (SIM, Thompson et al., 1995) of ambivalence. First, and most generally, statistical relationships obtained by the UAAM are difficult to interpret because the ambivalence index is necessarily confounded with the separate effects of its components. For instance, we have demonstrated that a positive relationship between an ambivalence index and subjective reports of ambivalence can be obtained when the sum of the covariances of subjective ambivalence with positivity and negativity is larger than zero $(\sigma_{\mathrm{Y,N}} + \sigma_{\mathrm{Y,P}} > 0)$. Thus, the effect of either positivity or negativity may be solely responsible for the statistical estimate of a positive relationship between an ambivalence index and subjective ambivalence, in which case the estimate (i.e., a correlation coefficient) and the label (i.e., "effect of ambivalence") would not match and the conclusion would be invalid (cf. Reichardt, 2006). In the same vein, the UAAM makes it difficult for researchers from different laboratories to be sure if they have obtained the same results, which impedes the production of cumulative knowledge (e.g., via meta-analysis).

The second problem of the UAAM follows directly from the first: The UAAM fails to adjudicate between mutually exclusive ambivalence models. Although the CRM and the SIM differ in one important aspect, research relying on multiple ambivalence indices including the CRM and the SIM has generally reported converging results (e.g., Jost & Burgess, 2000; Petty et al., 2006; Priester & Petty, 2001; Riketta, 2000; Spencer-Rodgers, Peng, Wang, & Hou, 2004). However, this empirical convergence is unsatisfying because the theoretical assumptions of the CRM and the SIM differ with regard to the influence of the dominant reactions. Whereas the CRM postulates that subjective ambivalence is a positive linear function of the minimum of positivity and negativity (i.e., the conflicting reaction), the SIM postulates an additional negative effect of the maximum of positivity and negativity (i.e., the dominant reaction) on subjective ambivalence. Consistent with previous research, our Studies 1 and 2 have found support for both of these contradictory assumptions using the UAAM.

Third, the UAAM can produce a moderator effect of ambivalence where none exists. We have examined in some detail the conditions amenable to finding a negative interaction effect between attitude and an ambivalence index on some outcome variable (e.g., a related behavior or attitude measured at a later point in time) such as has often been reported in the literature (Armitage & Conner, 2004; Cooke & Sheeran, 2004). With regard to the CRM and the SIM we have found that a sufficient condition for a negative interaction effect to occur is implied by positive covariances of positivity with attitude and the dependent variable and negative covariances of negativity with attitude and the dependent variable. Study 5 used Monte Carlo simulations to demonstrate that when this condition is met, Type I errors are seriously inflated with respect to the interaction as modeled by the UAAM.

All of these problems can be solved by following a simple rule, namely *Analyze separately what you measure separately!* Building on the illuminating work of Edwards (1994), we have proposed an alternative data-analytic approach that we have called the Multivariate Approach to Ambivalence Models (MAAM). The MAAM consists of translating the assumptions of a given ambivalence model into a multivariate pattern of statistical hypotheses and constraints. Our empirical studies confirm the superiority of the MAAM. In Studies 1 and 2, we have used the MAAM to compare the ability of the CRM and the SIM to explain subjective ambivalence. Across both studies, the assumptions of the CRM were more strongly supported than those of the SIM. Thus, unlike the UAAM, the MAAM can differentiate empirically between competing ambivalence models. In Studies 3 and 4, we have used the MAAM to test the hypothesis of a negative interaction effect between attitude at Time 1 and ambivalence on attitude at Time 2 (Study 3) and between positively or negatively framed experimental situations and ambivalence on responses to an outgroup member (Study 4). Whereas the UAAM would have corroborated the hypothesis, the MAAM revealed the interaction effect to be spurious. In Study 3, attitude at Time 2 was only predicted by attitude at Time 1, and in Study 4, responses to the outgroup member were only predicted by experimental condition and an interaction between condition and negativity.

In sum, our analytic as well as our empirical results strongly suggest that the MAAM produces more valid conclusions about the effects of ambivalence than the UAAM. Nevertheless, our research is limited by several features that future research may profitably address.

Limitations and Future Directions

We have confined our analyses to the CRM and the SIM of ambivalence. Although the CRM and the SIM represent the simplest and the most frequently applied ambivalence models, respectively, there are a number of competing models (for reviews, see Breckler, 1994; Jonas et al., 2000; Priester & Petty, 1996; Riketta, 2000; Thompson, Zanna, & Griffin, 1995). It would be desirable to explicate the multivariate pattern of statistical hypotheses and constraints for these models as well, so that their empirical validity could be jointly tested. For instance, the ambivalence model by Katz and colleagues (1986) stipulates that ambivalence would result from a multiplicative effect of positivity and negativity. In order to test it against the predictions of the CRM and the SIM, one could use our Equation (23) and examine if the coefficient associated with the product of positivity and negativity is positive $(b_4 > 0)$ and if all other coefficients are zero (which was not the case in our Studies 1 and 2). However, other models imply more complicated nonlinear functions that deserve a more detailed discussion than can be provided here. In addition to the aforementioned reviews, there is also a unique ambivalence model implicit in the evaluative space model (Cacioppo & Berntson, 1994; Cacioppo et al., 1997) briefly discussed above. Thus, a priority of future research should be to empirically compare the different ambivalence models more exhaustively.

Furthermore, we only briefly discussed the issue of statistical power. To be sure, power estimates are always important to be able to confidently interpret null findings. However, two aspects of the MAAM increase the importance of power considerations. First, the MAAM centrally involves statistical constraints which reverse the logic of significance testing. Second, the number of parameters that need to be estimated is obviously larger for a multivariate approach. For the simulated samples of size $n = 150$, Study 5 has found an alarmingly high rate of Type II errors when examining a moderator effect. This means that the MAAM requires much larger samples than are typically used in psychological research. Future research would thus benefit from explicitly considering the size of the expected effects and sample size required to reject the null hypotheses associated with the full multivariate model.

Finally, we have presented all of our analyses in terms of observed variables, neglecting the issue of measurement error. Whereas (random) measurement error generally deflates effect size estimates, the influence of measurement error on parameter estimates from regression models including nonlinear terms is unpredictable (Baron & Kenny, 1986). Although the MAAM achieves greater unbiasedness of the parameter estimates than the UAAM, there is certainly room for improving on the bias resulting from measurement error. Considering the nuanced mathematical predictions of the diverse ambivalence models, it would be especially desirable if future research would take advantage of the possibilities of latent nonlinear structural equation modeling (Jöreskog & Yang, 1996; Kenny & Judd, 1984; Klein & Moosbrugger, 2000; Klein & Muthén, in press; Marsh, Wen, & Hau, 2004), which seems well suited for the analysis of the kinds of multivariate models called for by the MAAM (for a recent review of advantages and challenges of latent nonlinear structural equation modeling, see Dimitruk et al., in press).

Conclusion

Across many areas of psychology, there has been a surge of recent research on ambivalence. Based on the analytic and empirical results presented in this paper, we would conclude that much less is known about ambivalence than the number of existing studies might seem to suggest. The Univariate Approach to Ambivalence Models dominating the literature cannot but produce ambiguous results. Nevertheless, we hope that the Multivariate Approach to Ambivalence Models we have proposed will be helpful in reorienting the field toward more conclusive tests of (and between) models of ambivalence.

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Appendix A

In the following, we derive all expected values and covariances involving the dummy variable *W* that are used in the main text and Appendices B and C, i.e. $\sigma_{W,P}$, $\sigma_{W,N}$, $\sigma_{Y,XWP}$, $\sigma_{X,XWP}$, $\sigma_{WP,WN}$, and $\mathbb{E}(WP^2)$. As explained in the text, we assume (a) that the measures *X*, *Y*, *N* and *P* are from a multivariate normal distribution with zero means and unit variances, (b) that *N* and *P* are independent, (c) that the correlations of *N* and *P* with *X* and *Y* are symmetric around zero. In formal terms, we start with a multivariate normal distributed vector

$$\begin{pmatrix} X \\ Y \\ N \\ P \end{pmatrix} \text{ with mean } \begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \end{pmatrix} \text{ and covariance } \begin{pmatrix} 1 & \sigma_{X,Y} & \sigma_{X,N} & -\sigma_{X,N} \\ \sigma_{X,Y} & 1 & \sigma_{Y,N} & -\sigma_{Y,N} \\ \sigma_{X,N} & \sigma_{Y,N} & 1 & 0 \\ -\sigma_{X,N} & -\sigma_{Y,N} & 0 & 1 \end{pmatrix}$$

and define

$$W := \begin{cases} 0, P \geq N \\ 1, P < N \end{cases}.$$

Let $\begin{pmatrix} S \\ T \\ U \\ V \end{pmatrix}$ be a standard normal distributed vector, i.e. it has the density

$$f(s,t,u,v) = \frac{1}{4\pi^2} e^{-\frac{1}{2}(s^2+t^2+u^2+v^2)}$$

Then

$$\begin{pmatrix} \sigma_{X,N}(T-S) + c_1 U + c_2 V \\ \sigma_{Y,N}(T-S) + \sqrt{1-2\sigma_{Y,N}^2}\,U \\ T \\ S \end{pmatrix} \stackrel{d}{=} \begin{pmatrix} X \\ Y \\ N \\ P \end{pmatrix} \qquad \text{(A1)}$$

with $c_1 = \dfrac{\sigma_{X,Y} - 2\sigma_{X,N}\sigma_{Y,N}}{\sqrt{1-2\sigma_{Y,N}^2}}, c_2 = \sqrt{1-2\sigma_{X,N}^2 - c_1^2}$. Note that all terms are well defined, since a covariance matrix is positive definite. The equality in (A1) is an equality in distribution, which can be easily checked by calculating the mean and covariance of both vectors. Note that both vectors are multivariate normal distributed. A simple consequence of (A1) is the following equation

$$W \overset{d}{=} \tilde{W} := \begin{cases} 0, S \geq T \\ 1, S < T \end{cases}.$$

Using this and (A1) we can calculate

$$\sigma_{W,P} = \mathbb{E}(WP) = \mathbb{E}(\tilde{W}S)$$

$$= \int_{-\infty}^{\infty} s \int_{s}^{\infty} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \frac{1}{4\pi^2} e^{-\frac{1}{2}(s^2+t^2+u^2+v^2)} \, dv\, du\, dt\, ds \tag{A2}$$

$$= \int_{-\infty}^{\infty} s \int_{s}^{\infty} \frac{1}{2\pi} e^{-\frac{1}{2}(s^2+t^2)} \, dt\, ds = -\frac{1}{2\sqrt{\pi}},$$

$$\mathbb{E}((N-P)^2 WP) = \mathbb{E}((T-S)^2 \tilde{W} S) = \int_{-\infty}^{\infty} s \int_{s}^{\infty} (t-s)^2 \frac{1}{2\pi} e^{-\frac{1}{2}(s^2+t^2)} \, dt\, ds = -\frac{2}{\sqrt{\pi}}, \tag{A3}$$

$$\mathbb{E}(WP^2) = \mathbb{E}(\tilde{W} S^2) = \int_{-\infty}^{\infty} s^2 \int_{s}^{\infty} \frac{1}{2\pi} e^{-\frac{1}{2}(s^2+t^2)} \, dt\, ds = \frac{1}{2}, \tag{A4}$$

$$\mathbb{E}(WPN) = \mathbb{E}(\tilde{W} ST) = \int_{-\infty}^{\infty} \int_{s}^{\infty} st \frac{1}{2\pi} e^{-\frac{1}{2}(s^2+t^2)} \, dt\, ds = 0. \tag{A5}$$

Because $W^2 = W$, we can use (A5) to calculate the covariance of *WP* and *WN*:

$$\sigma_{WP,WN} = \mathbb{E}(W^2 PN) - \mathbb{E}(WP)\mathbb{E}(WN) = 0 - \left(-\frac{1}{2\sqrt{\pi}}\right)\left(\frac{1}{2\sqrt{\pi}}\right) = \frac{1}{4\pi}. \tag{A6}$$

Finally we get

$$\sigma_{Y,XWP} = \mathbb{E}(YXWP)$$

$$= \mathbb{E}\left((\sigma_{X,N}(T-S) + c_1 U + c_2 V)(\sigma_{Y,N}(T-S) + \sqrt{1-2\sigma_{Y,N}^2}\, U)\tilde{W} S \right)$$

$$= \sigma_{X,N}\sigma_{Y,N}\,\mathbb{E}\left((T-S)^2\,\tilde{W}\,S\right) + c_1\sqrt{1-2\sigma_{Y,N}^2}\,\mathbb{E}\left(U^2\,\tilde{W}\,S\right) \qquad \text{(A7)}$$

$$= \sigma_{X,N}\sigma_{Y,N}\,(-\frac{2}{\sqrt{\pi}}) + (\sigma_{X,Y} - 2\sigma_{X,N}\sigma_{Y,N})(-\frac{1}{2\sqrt{\pi}})$$

$$= -\frac{\sigma_{X,N}\sigma_{Y,N}}{\sqrt{\pi}} - \frac{\sigma_{X,Y}}{2\sqrt{\pi}},$$

where we used (A1) for the second equality. The third equality is due to the linearity of the expectation and the independence of S,T,U and V. For the next line we used the values from (A2) and (A3). By the same steps we also get

$$\sigma_{X,XWP} = \mathbb{E}(XXWP)$$

$$= \mathbb{E}\left((\sigma_{X,N}(T-S) + c_1U + c_2V)^2\,\tilde{W}\,S\right)$$

$$= \sigma_{X,N}^2\,\mathbb{E}\left((T-S)^2\,\tilde{W}\,S\right) + c_1^2\,\mathbb{E}\left(U^2\,\tilde{W}\,S\right) + c_2^2\,\mathbb{E}\left(V^2\,\tilde{W}\,S\right) \qquad \text{(A8)}$$

$$= \sigma_{X,N}^2\,(-\frac{2}{\sqrt{\pi}}) + (1-2\sigma_{X,N}^2)(-\frac{1}{2\sqrt{\pi}})$$

$$= -\frac{\sigma_{X,N}^2}{\sqrt{\pi}} - \frac{1}{2\sqrt{\pi}}.$$

Appendix B

This Appendix derives the ratio of the standard deviations of the composite ambivalence variable *A* calculated according to the CRM and the SIM. Without any further assumptions, the variance of *A* when calculated according to the CRM and expressed as in Equation (5) is given by the following sum of variances and covariances:

$$\sigma^2_{A(CRM)} = 4\sigma^2_N + 4\sigma^2_{WP} + 4\sigma^2_{WN} + 8\sigma_{N,WP} - 8\sigma_{N,WN} - 8\sigma_{WP,WN} . \qquad \text{(B1)}$$

Analogously, the variance of *A* when calculated according to the SIM and expressed as in Equation (11) is as follows:

$$\begin{aligned} &\sigma^2_{A(SIM)} = 2.25\sigma^2_N + 0.25\sigma^2_P + 4\sigma^2_{WP} + 4\sigma^2_{WN} \\ &-1.5\sigma_{P,N} + 6\sigma_{N,WP} - 6\sigma_{N,WN} - 2\sigma_{P,WP} + 2\sigma_{P,WN} - 8\sigma_{WP,WN} \end{aligned} . \qquad \text{(B2)}$$

To simplify the derivations, we assume unit variances for *P* and *N* $(\sigma^2_P = \sigma^2_N = 1)$ and let *P* and *N* be uncorrelated $(\sigma_{P,N} = 0)$. It follows that *P* is also uncorrelated with the product of *W* and *N*, and that *N* is also uncorrelated with the product of *W* and *P* ($\sigma_{P,WN} = \sigma_{N,WP} = 0$). Under the assumption of multivariate normality and expected values of zero for *P* and *N*, the covariances of *P* and *N* with the products of these variables and *W* are equal to the variances of *P* and *N* divided by 2 ($\sigma_{P,WP} = \sigma_{N,WN} = \frac{\sigma^2_P}{2} = \frac{1}{2}$), see (A4). Given these assumptions, the desired ratio of the standard deviations can be preliminarily written as

$$\frac{\sigma_{A(CRM)}}{\sigma_{A(SIM)}} = \frac{\sqrt{c}}{\sqrt{-1.5 + c}} , \qquad \text{(B3)}$$

where $c = 4\sigma^2_{WP} + 4\sigma^2_{WN} - 8\sigma_{WP,WN}$.

Because the variance of a product is given by the expectation of the square of the product minus the squared expectation of the product (e.g., $\sigma^2_{WP} = \mathbb{E}(WP^2) - (\mathbb{E}[WP])^2$), we may use the expectations (A2) and (A4) found in Appendix A to find

$$\sigma_{WP}^{2} = \sigma_{WN}^{2} = \frac{1}{2} - \frac{1}{4\pi}. \qquad \text{(B4)}$$

Furthermore, (A6) in Appendix A has found the covariance of *WP* and *WN* to be equal to $\frac{1}{4\pi}$, so that

$$c = 4 - \frac{4}{\pi}. \qquad \text{(B5)}$$

Note that the results (A2), (A4), and (A6) also hold when the assumption of symmetric covariances of *P* and *N* with *X* and *Y*, respectively (made for other purposes of Appendix A), is dropped. By substituting (B5) into (B3), we find that the ratio of the standard deviations of *A* calculated according to the CRM and the SIM is

$$\frac{\sigma_{A(CRM)}}{\sigma_{A(SIM)}} = \frac{\sqrt{4 - \frac{4}{\pi}}}{\sqrt{2.5 - \frac{4}{\pi}}} \approx \frac{3}{2}, \qquad \text{(B6)}$$

which is the expression given in the main text.

Appendix C

In the following, we prove that a negative moderator effect of ambivalence (*A*) in the context of the UAAM follows from the assumption of symmetric covariances of positivity (*P*) and negativity (*N*) with the focal predictor (*X*) and the dependent variable (*Y*), respectively ($\sigma_{\mathrm{Y,N}} = -\sigma_{\mathrm{Y,P}}$ and $\sigma_{\mathrm{X,N}} = -\sigma_{\mathrm{X,P}}$). The parameter of interest is the coefficient b_3 that is associated with the interaction term in the following general moderated multiple regression model

$$Y = b_0 + b_1 X + b_2 A + b_3 XA + \varepsilon \ , \qquad \text{(C1)}$$

where *A* is calculated according to the CRM or the SIM as detailed in the main text. We begin by stating b_3 in (C1) in terms of a general formula for the interaction effect (e.g., McNemar, 1962)

$$b_3 = \frac{\sigma_Y \left[\rho_{Y,XA}(1-\rho_{X,A}^2) + \rho_{Y,X}(\rho_{X,A}\rho_{A,XA} - \rho_{X,XA}) + \rho_{Y,A}(\rho_{X,XA}\rho_{X,A} - \rho_{A,XA})\right]}{\sigma_{XA}\left[1 - \rho_{X,XA}^2 - \rho_{A,XA}^2 - \rho_{X,A}^2 + 2\rho_{X,A}\rho_{X,XA}\rho_{A,XA}\right]}, \qquad \text{(C2)}$$

where ρ denotes a correlation and σ denotes a standard deviation. This rather unwieldy formula can be simplified at the expense of a limited set of assumptions. As before, we assume that *Y*, *X*, *P* and *N* follow a multivariate normal distribution with zero means and unit variances. Additionally, we can take advantage of our earlier result that the covariance of *A* and *Y* will be equal to 0 when $(\sigma_{\mathrm{Y,N}} + \sigma_{\mathrm{Y,P}}) = 0$. Thus, if we assume that the covariance of *Y* and *P* is equal in absolute magnitude to the covariance of *Y* and *N*, but oppositely signed ($\sigma_{\mathrm{Y,N}} = -\sigma_{\mathrm{Y,P}}$), and likewise for the covariances involving *X* ($\sigma_{\mathrm{X,N}} = -\sigma_{\mathrm{X,P}}$), the terms $\rho_{Y,A}$ and $\rho_{X,A}$ are zero and Equation (C2) simplifies considerably, yielding

$$b_3 = \frac{\sigma_Y(\rho_{Y,XA} - \rho_{Y,X}\rho_{X,XA})}{\sigma_{XA}(1-\rho_{X,XA}^2 - \rho_{A,XA}^2)} \ . \qquad \text{(C3)}$$

Note that we may reasonably expect this simplifying assumption about symmetric correlations of *P* and *N* to hold up empirically when *X* represents attitude measured on a bipolar scale and *Y* is an attitudinally relevant variable (e.g., attitude measured at a later point in time or a corresponding behavior). Under these circumstances, we would expect a positive correlation between *P* and *X* or *Y*, and a negative correlation between *N* and *X* or *Y*, while there would be no theoretical reason to assume a difference in absolute size between these correlations. This line of reasoning is supported by results reported in Kaplan (1972) as well as our own data.

The numerator of the ratio defining the interaction effect b_3 as shown in Equation (C3) would be zero if all the variables involved followed a multivariate normal distribution with zero means (Aiken & West, 1991). However, we assume only the measured variables *Y, X, P*, and *N* to be multivariate normal. The coefficient b_3 cannot in general be expected to be zero once we introduce a new variable *A* which is calculated according to an ambivalence formula. In order to see more clearly how b_3 can deviate from zero, it is helpful to rewrite the correlations $\rho_{Y,XA}$ and $\rho_{X,XA}$ in (B3) in terms of the linear combinations suggested by the CRM and the SIM. To recapitulate, according to the CRM, ambivalence is calculated as $A = 2N + 2WP - 2WN$, and according the SIM, ambivalence is calculated as $A = 1.5N - 0.5P + 2WP - 2WN$, where *W* is a dummy variable that is set to 0 when $P > N$, and set to 1 when $P < N$, and randomly set to 1 or 0 when $P = N$.

Thus, according to the CRM, the correlations involving the product term *XA* are given by

$$\rho_{Y,XA} = \frac{\sigma_{Y,XA}}{\sigma_Y \sigma_{XA}} = \frac{2\sigma_{Y,XN} + 2\sigma_{Y,XWP} - 2\sigma_{Y,XWN}}{\sigma_Y \sigma_{XA}}, \qquad \text{(C4)}$$

$$\rho_{X,XA} = \frac{\sigma_{X,XA}}{\sigma_X \sigma_{XA}} = \frac{2\sigma_{X,XN} + 2\sigma_{X,XWP} - 2\sigma_{X,XWN}}{\sigma_X \sigma_{XA}}. \qquad \text{(C5)}$$

According to the SIM, the above correlations are given by

$$\rho_{Y,XA} = \frac{\sigma_{Y,XA}}{\sigma_Y \sigma_{XA}} = \frac{1.5\sigma_{Y,XN} - 0.5\sigma_{Y,XP} + 2\sigma_{Y,XWP} - 2\sigma_{Y,XWN}}{\sigma_Y \sigma_{XA}}, \qquad \text{(C6)}$$

$$\rho_{X,XA} = \frac{\sigma_{X,XA}}{\sigma_X \sigma_{XA}} = \frac{1.5\sigma_{X,XN} - 0.5\sigma_{X,XP} + 2\sigma_{X,XWP} - 2\sigma_{X,XWN}}{\sigma_X \sigma_{XA}}. \tag{C7}$$

Given our assumptions that *Y*, *X*, *P*, and *N* are multivariate normal with means of zero, the covariance between a product of any two of these variables and a third variable will be zero (Aiken & West, 1991), so that only the last two terms remain in the numerators of the four preceding equations.

$$\rho_{Y,XA} = \frac{2\sigma_{Y,XWP} - 2\sigma_{Y,XWN}}{\sigma_Y \sigma_{XA}}, \tag{C8}$$

$$\rho_{X,XA} = \frac{2\sigma_{X,XWP} - 2\sigma_{X,XWN}}{\sigma_X \sigma_{XA}}. \tag{C9}$$

This proves that $\rho_{Y,XA}$ and $\rho_{X,XA}$ are invariant whether the CRM or the SIM is chosen to calculate *A*. Moreover, the last two terms can be collapsed because we defined $\rho_{X,P} = -\rho_{X,N}$ and $\rho_{Y,P} = -\rho_{Y,N}$, so that $\sigma_{Y,XWP} = -\sigma_{Y,XWN}$. Thus, we obtain

$$\rho_{Y,XA} = \frac{4\sigma_{Y,XWP}}{\sigma_Y \sigma_{XA}}, \tag{C10}$$

$$\rho_{X,XA} = \frac{4\sigma_{X,XWP}}{\sigma_X \sigma_{XA}}. \tag{C11}$$

Because the sign of b_3 depends only on the numerator of (C3), we can substitute (C10) and (C11) into (C3) while ignoring the denominator:

$$\sigma_Y \left(\frac{4\sigma_{Y,XWP}}{\sigma_Y \sigma_{XA}} - \frac{\sigma_{X,Y}}{\sigma_X \sigma_Y} \frac{4\sigma_{X,XWP}}{\sigma_X \sigma_{XA}} \right) = \frac{4\sigma_{Y,XWP}}{\sigma_{XA}} - \frac{\sigma_{X,Y} * 4\sigma_{X,XWP}}{\sigma_X^2 \sigma_{XA}}. \tag{C12}$$

The sign of b_3 will be negative when this expression is negative. Thus, we can restate it as the following inequality, which must hold for b_3 to be negative:

$$\sigma_{Y,XWP} < \frac{\sigma_{X,Y} \sigma_{X,XWP}}{\sigma_X^2}. \tag{C13}$$

The terms $\sigma_{Y,XWP}$ and $\sigma_{X,XWP}$ have been derived in Appendix A. By substituting these results into (C13), we obtain

$$-\frac{\sigma_{X,N}\sigma_{Y,N}}{\sqrt{\pi}}-\frac{\sigma_{X,Y}}{2\sqrt{\pi}}<\sigma_{X,Y}\left(-\frac{\sigma^2_{X,N}}{\sqrt{\pi}}-\frac{1}{2\sqrt{\pi}}\right), \tag{C14}$$

Which is equivalent to

$$-\frac{\sigma_{X,N}\sigma_{Y,N}}{\sqrt{\pi}}<-\frac{\sigma^2_{X,N}\sigma_{X,Y}}{\sqrt{\pi}}. \tag{C15}$$

Multiplying by $-\sqrt{\pi}$ yields

$$\sigma_{X,N}\sigma_{Y,N}>\sigma^2_{X,N}\sigma_{X,Y} \ . \tag{C16}$$

Because we defined $\sigma_{X,N} < 0$, we can divide (C16) by $\sigma_{X,N}$ and obtain

$$\sigma_{Y,N}<\sigma_{X,N}\sigma_{X,Y}. \tag{C17}$$

Recall that b_3 in (C1) will be negative when this inequality holds. Thus, assuming that $0<\sigma_{X,Y}<1$ (which is an almost trivial assumption because we defined $\sigma_X=\sigma_Y=1$ and use *X* to index attitude and *Y* to index an attitudinally relevant variable) this result informs us that b_3 in (C1) will be negative when $\frac{\sigma_{Y,N}}{\sigma_{X,N}}>\sigma_{X,Y}$. Note that we may write equivalently $\frac{\rho_{Y,N}}{\rho_{X,N}}>\rho_{X,Y}$ because we assume unit variances.

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Footnotes

1. The resulting measure has been variously called “potential ambivalence” (Newby-Clark et al., 2002), “operative ambivalence” (Bassili, 1996), or “objective ambivalence” (Priester & Petty, 1996). Because most of this paper is about formula-based measures of ambivalence, we will simply use the term “ambivalence” to refer to these measures and the term “subjective ambivalence” to refer to self-report measures.

2. Note that Priester and Petty (1996) also used more sophisticated analyses than merely correlation coefficients. Nevertheless, their analyses confounded the separate effects of positivity and negativity, which motivated our efforts to expose possible confounds and suggest a more viable alternative (see General Discussion).

Table 1

Patterns of Regression Coefficients Implied by the Conceptual Hypothesis of a Positive Effect of Ambivalence According to CRM and SIM

Coefficients	Hypotheses		Constraints	
	CRM	SIM	CRM	SIM
b_1P		< 0	$= 0$	
b_2N	> 0	> 0		$= -3b_1$
b_3W			$= 0$	$= 0$
b_4PN			$= 0$	$= 0$
b_5WP	> 0	> 0	$= b_2$	$= -4b_1$
b_6WN	< 0	< 0	$= -b_2$	$= 4b_1$

Note. CRM = Conflicting Reactions Model (Kaplan, 1972), SIM = Similarity Intensity Model (Thompson et al., 1995). *P* and *N* refer to commensurate measures of positivity and negativity toward the attitude object, *W* is a dummy variable that equals 0 when $P > N$, that equals 1 when $P < N$, and that is randomly set to 1 or 0 when $P = N$.

Table 2

Patterns of Regression Coefficients Implied by the Conceptual Hypothesis of a Negative (Attenuating) Moderator Effect of Ambivalence According to CRM and SIM

Coefficients	Hypotheses		Constraints	
	CRM	SIM	CRM	SIM
b_4W			= 0	= 0
b_5XP		>0	= 0	
b_6XN	<0	<0		$=-3b_5$
b_7XW			= 0	= 0
b_8PN			= 0	= 0
b_9WP			= 0	= 0
$b_{10}WN$			= 0	= 0
$b_{11}XWP$	<0	<0	$=b_6$	$=-4b_5$
$b_{12}XWN$	>0	>0	$=-b_6$	$=4b_5$
$b_{13}XPN$			= 0	= 0
$b_{14}WPN$			= 0	= 0

Note. CRM = Conflicting Reactions Model (Kaplan, 1972), SIM = Similarity Intensity Model (Thompson et al., 1995). *P* and *N* refer to commensurate measures of positivity and negativity toward the attitude object, *W* is a dummy variable that equals 0 when $P > N$, that equals 1 when $P < N$, and that is randomly set to 1 or 0 when $P = N$. The variables *X*, *P*, and *N* must be included in the regression model, although the moderator hypothesis does not imply any specific hypotheses or constraints regarding the associated coefficients.

Table 3

Estimated Regression Coefficients of the Model Predicting Subjective Ambivalence (Studies 1 and 2)

Coefficients	Study 1 (N = 144)		Study 2 (N = 117)	
	b	$CI_{95\%}$	b	$CI_{95\%}$
b_0	3.73	(3.32, 4.13)	2.71	(2.13, 3.29)
b_1P	-.34	(-.78, .10)	-.42	(-.91, .07)
b_2N	.87	(.52, 1.22)	.69	(.27, 1.12)
b_3W	.52	(-.03, 1.08)	-.02	(-.64, .60)
b_4PN	-.01	(-.23, .21)	-.05	(-.28, .19)
b_5WP	.69	(.09, 1.29)	.77	(.13, 1.40)
b_6WN	-.85	(-1.50, -.19)	-.68	(-1.42, .06)

Note. P and N refer to standardized measures of positivity and negativity toward the attitude object, W is a dummy variable that equals 0 when $P > N$, that equals 1 when $P < N$, and that is randomly set to 1 or 0 when $P = N$, PN, WP, and WN are the products of the individual variables. The column labelled b contains the unstandardized regression coefficients. The column labelled $CI_{95\%}$ contains the lower and upper limits of the 95% confidence interval for the population regression coefficient.

Table 4

Estimated Regression Coefficients of the Moderated Multiple Regression Model Using the Index-Variables Suggested by CRM and SIM (Study 3)

Coefficients	A = CRM index variable		A = SIM index variable	
	b	$CI_{95\%}$	*b*	$CI_{95\%}$
b_0	3.46	(3.40, 3.53)	3.46	(3.40, 3.53)
$b_1 X$	.45	(.39, .52)	.44	(.37, .50)
$b_2 A$	.03	(-.03, .10)	.03	(-.03, .09)
$b_3 XA$	-.11	(-.17, -.05)	-.13	(-.18, -.07)

Note. The dependent variable was attitude at Time 2. *X* = attitude at Time 1, *A* = ambivalence index calculated according to the Conflicting Reactions Model (CRM) or the Similarity Intensity Model (SIM), *XA* = product of *X* and *A*. The columns labelled *b* contain the unstandardized regression coefficients. The columns labelled $CI_{95\%}$ contain the lower and upper limits of the 95% confidence interval for the population regression coefficient.

Table 5

Estimated Regression Coefficients of the Multivariate Approach to Testing a Moderator Hypothesis About Ambivalence (Studies 3 and 4)

Coefficients	Study 3 (N = 385)		Study 4 (N = 86)	
	b	$CI_{95\%}$	b	$CI_{95\%}$
b_0	3.50	(3.32, 3.67)	6.36	(5.42, 7.30)
b_1X	.30	(.13, .47)	-3.78	(-5.36, -2.20)
b_2P	.20	(-.15, .56)	-.07	(-.87, .73)
b_3N	-.17	(-.55, .21)	.13	(-.77, 1.04)
b_4W	.04	(-.26, .34)	-.42	(-1.46, .62)
b_5XP	.15	(-.15, .46)	.97	(-.66, 2.59)
b_6XN	-.08	(-.43, .28)	-1.15	(-2.16, -.14)
b_7XW	.09	(-.15, .32)	1.45	(-.25, 3.14)
b_8PN	-.10	(-.72, .52)	-.15	(-.75, .46)
b_9WP	.29	(-.24, .83)	.06	(-.86, .97)
$b_{10}WN$	-.25	(-.80, .30)	-.40	(-1.52, .73)
$b_{11}XWP$	-.13	(-.65, .39)	-.94	(-2.76, .88)
$b_{12}XWN$	-.07	(-.64, .51)	1.27	(-.38, 2.92)
$b_{13}XPN$	-.10	(-.48, .28)	.17	(-.37, .70)
$b_{14}WPN$	.20	(-.58, .98)	.10	(-.55, .75)

Note. P and N refer to commensurate measures of positivity and negativity toward the attitude object, W is a dummy variable that equals 0 when $P > N$, that equals 1 when $P < N$, and that is randomly set to 1 or 0 when $P = N$. In Study 3, X represents attitude at Time 1, and the dependent variable was attitude at Time 2. In Study 4, X represents experimental condition (0 = positive frame, 1 = negative frame), and the dependent variable was responses to a Turkish woman. The columns labelled $CI_{95\%}$ contain the lower and upper limits of the 95% confidence interval for the population regression coefficient.

Table 6

Estimated Regression Coefficients of the Moderated Multiple Regression Model Using the Index-Variables Suggested by CRM and SIM (Study 4)

Coefficients	A = CRM index variable		A = SIM index variable	
	b	$CI_{95\%}$	*b*	$CI_{95\%}$
b_0	6.04	(5.80, 6.29)	6.04	(5.80, 6.29)
b_1X	-2.40	(-2.78, -2.03)	-2.40	(-2.78, -2.03)
b_2A	-.03	(-.27, .21)	-.01	(-.25, .22)
b_3XA	-.40	(-.78, -.02)	-.42	(-.80, -.03)

Note. The dependent variable was responses to the Turkish woman. *X* = experimental condition (0 = positive frame, 1 = negative frame), *A* = ambivalence index calculated according to the Conflicting Reactions Model (CRM) or the Similarity Intensity Model (SIM), *XA* = product of *X* and *A*. The columns labelled *b* contain the unstandardized regression coefficients. The columns labelled $CI_{95\%}$ contain the lower and upper limits of the 95% confidence interval for the population regression coefficient.

Table 7

Average Regression Coefficients and Percentage of Type I and Type II Errors Across 100,000 Simulated Samples Using the UAAM (each n = 150)

Coefficients	A = CRM index variable			A = SIM index variable		
	$\bar{b}$	*Type I (b<0)*	*Type II (b=0)*	$\bar{b}$	*Type I (b<0)*	*Type II (b=0)*
b_0	-.00			-.00		
$b_1 X$	1.73		12.7%	1.66		16.5%
$b_2 A$	-.00	2.5%		-.00	2.6%	
$b_3 XA$	-.34	9.4%		-.45	14.6%	

Note. *X* = simulated attitude variable, *A* = ambivalence index calculated according to the Conflicting Reactions Model (CRM) or the Similarity Intensity Model (SIM), *XA* = product of *X* and *A*. See Study 5 for details.

Table 8

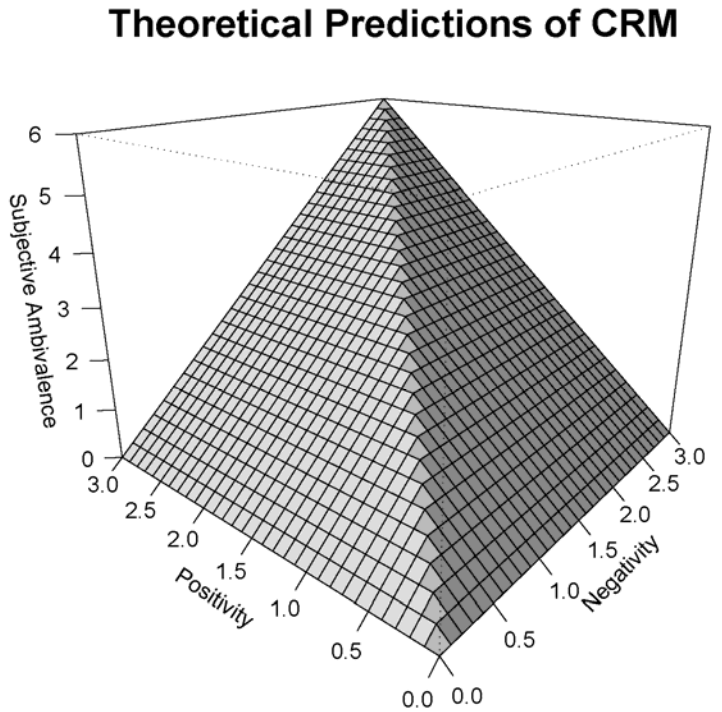
Average Regression Coefficients and Percentage of Type I and Type II Errors Across 100,000 Simulated Samples Using the MAAM (each n = 150)

Coefficients	$\bar{b}$	*Type I (b<0)*	*Type II (b=0)*
b_0	-.01		
b_1X	1.00		91.5%
b_2P	1.01		91.0%
b_3N	-1.01		91.0%
b_4W	-.00	2.6%	
b_5XP	-.01	2.6%	
b_6XN	.00	2.6%	
b_7XW	-.00	2.5%	
b_8PN	.01	2.4%	
b_9WP	-.02	2.6%	
$b_{10}WN$	.01	2.4%	
$b_{11}XWP$	.01	2.4%	
$b_{12}XWN$	-.01	2.6%	
$b_{13}XPN$	-.01	2.4%	
$b_{14}WPN$	-.00	2.5%	

Note. MAAM = Multivariate Approach to Ambivalence Models. X = simulated attitude variable, P = simulated positivity variable, N = simulated negativity variable, W = dummy variable that equals 0 when $P > N$, that equals 1 when $P < N$, and that is randomly set to 1 or 0 when $P = N$. See Study 5 for details.

Figure Caption

Figure 1. Theoretical response surfaces implied by the CRM and the SIM versus the empirical response surfaces derived from local regression fitting.


Theoretical Predictions of CRM
Subjective Ambivalence
0
1
2
3
4
5
6
Positivity
0.0
0.5
1.0
1.5
2.0
2.5
3.0
Negativity
0.0
0.5
1.0
1.5
2.0
2.5
3.0

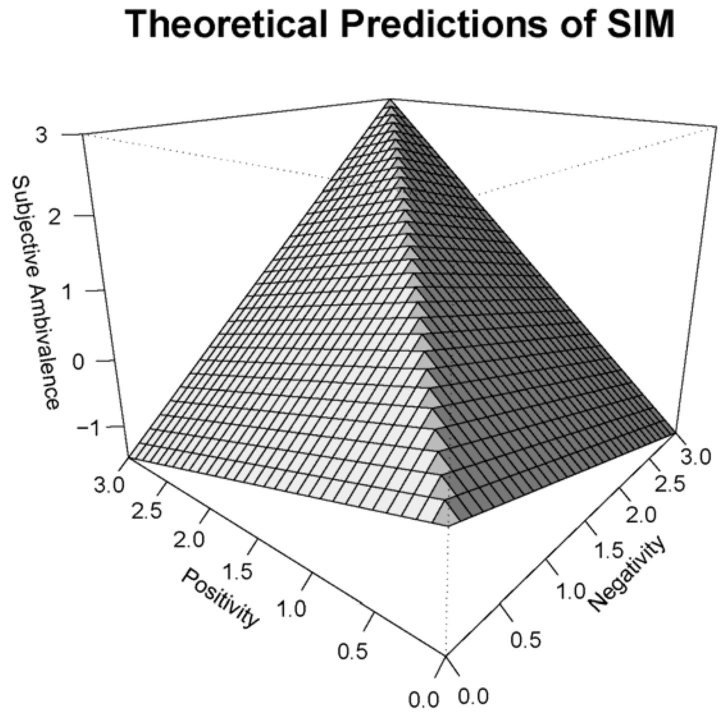
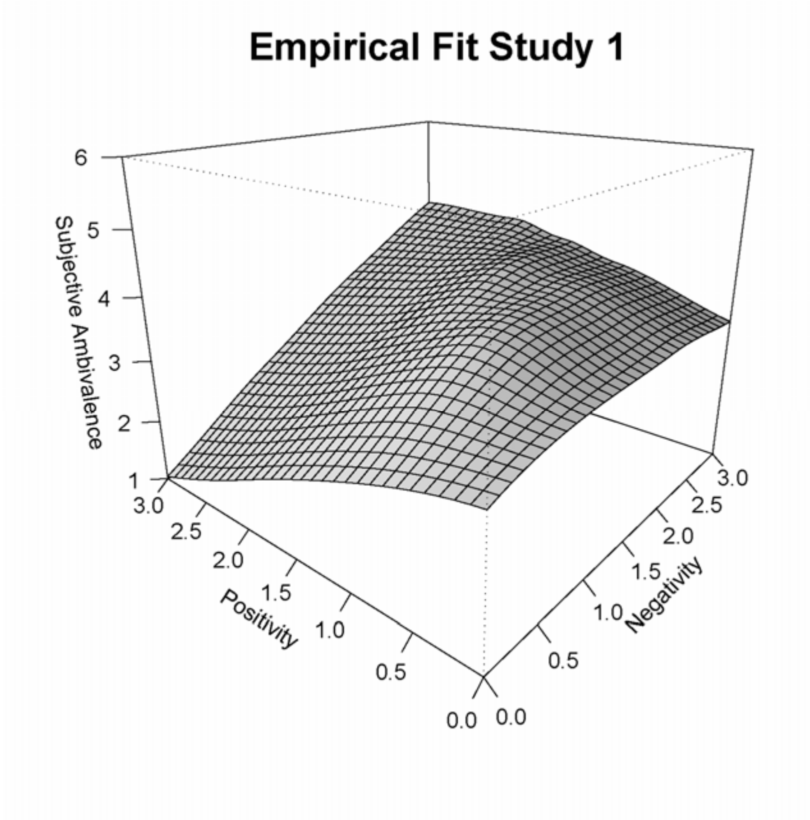
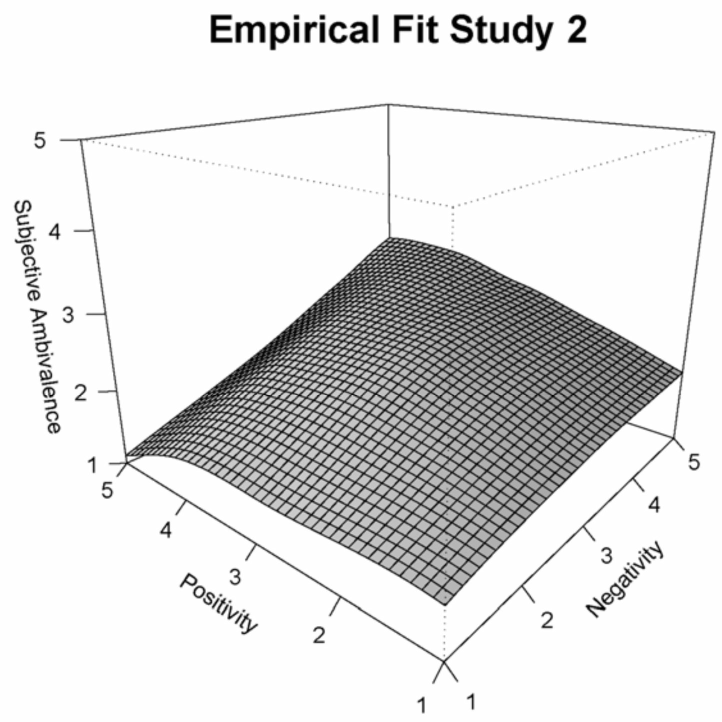

Theoretical Predictions of SIM
Subjective Ambivalence
−1
0
1
2
3
Positivity
0.0
0.5
1.0
1.5
2.0
2.5
3.0
Negativity
0.0
0.5
1.0
1.5
2.0
2.5
3.0

Figure 1 continues…





Final discussion and outlook

The preceding manuscripts have dealt with the validity of interpretational statements about the effects of multivariate constructs. I have proposed a new approach to testing hypotheses about the effects of attitude ambivalence (AA) and relative ingroup prototypicality (RIP). This approach eliminates the threats to validity that result from the confounding of index variables with their components and therefore increases the validity of interpretational statements about the effects of these index variables.

Compared with existing studies on the effects of RIP or AA, the benefits of the new approach are substantial. For instance, the empirical results associated with the multivariate approach presented in Manuscript #1 suggest that people’s attitudes toward outgroups depend on their perceptions of the outgroup’s prototypicality for a superordinate category that is shared with the ingroup. Yet, the construct of ingroup prototypicality does not seem to be necessary to explain differences in outgroup attitudes. Thus, the label ‘effect of relative ingroup prototypicality’ does not match the empirical pattern of results. This finding would have been overlooked without the multivariate approach advocated in my work, and future theoretical work and theory-based interventions would have been misguided.

Likewise, the new approach solves several problems in the ambivalence literature. First, previous studies have failed to adequately test the assumptions of different models of the emergence of subjective ambivalence. Second, they have been unable to adjudicate between mutually exclusive models, because bivariate correlations between subjective ambivalence and index variables derived from different models have yielded highly similar results. Finally, previous studies may have reported spurious moderator effects resulting from the confounding of the product term with a much simpler (but unmodeled) pattern of effects (e.g., symmetric correlations of positivity and negativity with the independent variable and the dependent variable). The empirical results associated with the multivariate approach presented in Manuscript #2 suggest that the Conflicting Reactions Model (CRM, Kaplan, 1972) offers a better account of the emergence of subjective ambivalence toward an outgroup than the Similarity-Intensity Model (SIM, Thompson, Zanna, & Griffin, 1995). Furthermore, the multivariate results did not support the frequently reported moderator effect of AA on attitude-attitude effects (i.e., differential attitude stability) or differences between positively and negatively framed intergroup situations (i.e., response amplification).

In spite of these advances in the areas of RIP and AA, the focus of my research has been confined to one particular type of confounds, namely the confounds resulting from attempts to reduce a multivariate construct to a single numerical index. In the following, I discuss other confounds that can arise in research on RIP and AA and sketch a strategy how future research can address these confounds.

Toward a more complete understanding of the effects of RIP and AA

As mentioned in the introduction, psychologists typically look for confounds in other places than index variables. Confounds are commonly associated with causal inferences and psychological measurement.

Causality

In the above manuscripts, I have treated the question of causality only superficially, although I have used a simple definition of validity from an article about the causal effects of treatments (Reichardt, 2006). In order to better understand the effects of RIP and AA, it is necessary to examine this definition more closely.

According to Reichardt (2006, p. 8), a statement about an effect is “invalid when the numerical estimate and the label given to that estimate do not match”. Reichardt calls such statements “size-of-effect statements” which are the “labels” researchers give to their numerical estimates of effects, where “effect” is defined as the difference between two treatments (one of which may be a condition where the other treatment is simply absent, i.e. a standard control condition). A size-of-effect statement formally reads as follows: “the size of the effect of treatment, Tx, for recipient, R, in setting, S, at time, T, on outcome variable, V, is equal to estimate, E”, or, in short, “the size of the effect for Tx, R, S, T, and V equals E.” (p. 8) Reichardt’s definition of validity covers all of the threats to validity that are well-known in the Campbellian tradition. For instance, without random assignment to treatments, there exists the possibility of selection differences, such that important (i.e., outcome-related) characteristics of recipients of one treatment differ from the characteristics of recipients of the other treatment. Furthermore, other aspects of a treatment might cause variation in the outcome variable than what the researcher believes (e.g., placebo effects instead of the active ingredients of a medication). Both situations would produce a “mismatch of the treatment”, for the label Tx would not match E.

In lieu of treatments, I have studied continuous variables as representations of constructs. However, the form and meaning of the size-of-effect statement remain the same. For instance, it is a meaningful statement to say “the size of the effect of RIP on attitude toward male students, using female undergraduates, working in front of a computer connected to the internet, in the winter of 2005, is r = -.25”. In this statement, the construct RIP assumes the role of Tx. As I have shown in Manuscript #1, this statement is potentially invalid when RIP is represented by a single variable because the assumptions implied by the construct (equal-but-opposite effects of ingroup and outgroup prototypicality) cannot be checked. In contrast, the multivariate approach entails three size-of-effect statements, in which ingroup prototypicality, outgroup prototypicality, and the sum of the effects of ingroup and outgroup prototypicality would be substituted for Tx (note that the “sum” in the latter statement does not refer to an index variable, but to a hypothesis about the relative size of two effects). Each size-of-effect statement is potentially more valid than the single size-of-effect statement referring to the index of RIP. Nevertheless, the statements retain a certain level of ambiguity, even if we disregard mismatches of R, S, and T (which pertain to problems of generalization or external validity, see Shadish, Cook, & Campbell, 2002).

Obviously, my research participants were not randomly assigned to different levels of RIP. Thus, other individual differences between participants may be responsible for the negative effect on outgroup attitudes, creating a threat to the construct validity of the cause. At present, there exists no experimental evidence for the effect of (manipulated) outgroup prototypicality on outgroup attitude. The validity of such a size-of-effect statement depends on the plausibility of the following general assumptions regarding causal relations (Mill, 1843): 1) cause and effect are distinct entities, 2) when the cause is present, the effect is also present (i.e., covariation of cause and effect), 3) the cause precedes the effect, 4) alternative explanations can be ruled out. It is important to note that I have not presented evidence for any of these assumptions, which means that we must hold off on any causal interpretation of the results presented in Manuscript #1 until these assumptions gain more plausibility through experimental studies – and the same is true for the results presented in Manuscript #2. One might argue that the second assumption is rendered plausible by the correlational results which revealed a positive relationship between outgroup prototypicality and outgroup attitude. However, this leads us to the second area where psychologists typically look for confounds.

Measurement

If the correlational results in Manuscript #1 and #2 were to be taken as support for Mill's second assumption, they would have to be valid for the underlying constructs as well. However, analyses reported in Manuscripts #1 and #2 were done on observed variables so that the main methodological argument would not be blurred by unnecessary mathematical complexity. This means that I have ignored the relationship between constructs and measures, where, using the nomenclature of Edwards and Bagozzi (2000), the term *measure* refers to the observed score on a measurement instrument (and not to the measurement instrument itself), and the term *construct* refers to the referents of researchers' definitions of their constructs, that is, to an empirical phenomenon that exists independent of theory. In fact, as Borsboom and colleagues (Borsboom, Mellenbergh, & van Heerden, 2003, 2004) have shown, the relation between constructs and measures (i.e., validity) is also a problem of causal inference: "If something does not exist, then one cannot measure it. If it exists but does not causally produce variations in the outcomes of the measurement procedure, then one is either measuring nothing at all or something different altogether." (Borsboom et al., 2004, p. 1061) Borsboom and colleagues provide several compelling arguments against traditional definitions of validity in terms of convergent, divergent, or criterion correlations (see especially p. 1066).

The problem of causal inference with regard to the relation between constructs and measures can be addressed in much the same way as the problem of causal inference with regard to treatment effects. We need a good theory, a design that allows for strong inferences regarding the theory, and appropriate statistical analyses. As noted by Borsboom and colleagues (Borsboom et al., 2003, 2004) and others (e.g., Michell, 1997), statistical evidence for unidimensionality is not enough for a measure to be considered a valid representation of a construct. Although I would have preferred to work with unidimensional measures of the constructs (i.e., measures satisfying the principle of local independence), I would not have gotten far (even if such measures existed), considering that it is possible to derive a unidimensional measure of "coin-tossing ability" (Wood, 1978).

The important question that first has to be addressed is: how can we conceive of observable responses being produced by the referents of our constructs? Let us consider the construct of outgroup attitude, for instance. According to a famous definition of attitude, it refers to a "psychological tendency" that is expressed by evaluating an outgroup with some degree of favor or disfavor (Eagly & Chaiken, 1993). What is the referent of a “psychological tendency”? Upon reflection, it is clear that this definition defines its referent somewhat tautologically. It is what is expressed. A more workable definition is offered by Fazio (1995, p. 247), who defines attitudes as “an association in memory between a given object and a given summary evaluation of the object”. Given that valence information about objects is highly important in our daily lives, it is plausible that such direct object-valence associations exist in memory. This assumption is further supported by evidence showing that evaluative responses to objects can be elicited even if the objects are presented very briefly or even subliminally (e.g., Fazio, Sanbonmatsu, Powell, & Kardes, 1986). Thus, it is theoretically plausible that attitudes (e.g, attitude toward male students) exist independently of their measures (e.g., responses to the item “I enjoy the company of male students”), and that they are causally responsible for variation in the measures. Similar considerations and conclusions would apply to the constructs of positivity and negativity.

Although it is obvious that other causes exist for variation in the measures, careful design choices such as random rotation of questionnaire items and balancing of items that require agreement or disagreement can in principle prevent systematic variation according to these extraneous causes. Thus, it is theoretically possible to model unidimensional latent variables from the observed measures of attitude, positivity, and negativity, e.g. via structural equation modeling. A more complicated case is presented by the constructs of prototypicality and subjective ambivalence. Unlike valence information, it is less plausible that representations of typicality and ambivalence regularly exist in memory. Rather, it is likely that judgments about typicality and ambivalence are constructed on the spot, that is, in the measurement situation itself.

With regard to the typicality construct, I would recommend the strategy that I adopted in the empirical study in Manuscript #1, that is, to separate analyses by typicality dimensions that can reasonably be expected to represented in memory. For instance, it is plausible that students have stored representations of how "dutiful" male students, female students, and students in general are. These different components can then be measured with multiple items and checked for unidimensionality.

With regard to the subjective ambivalence construct, I do not have a clear recommendation in the absence of a more elaborate theory about what this construct refers to. Although ambivalence research necessarily assumes that people often experience subjective ambivalence, it is unclear if people can consistently articulate this experience (Nisbett & Wilson, 1977). Similarly, in research on person-environment fit, a comprehensive recent study has found that there is little agreement between the theoretical logic underlying fit measures and people's actual reports of fit (Edwards, Cable, Williamson, Lambert, & Shipp, 2006). Perhaps the subjective ambivalence construct can be discarded until a more elaborate theory exists. In previous studies, the main purpose of the subjective ambivalence construct has been to validate different ambivalence indices. In Manuscript #2, I have provided both analytic and empirical arguments against using these indices, so that the subjective ambivalence construct appears dispensable. It appears most fruitful to concentrate on the putative effects of ambivalence (as represented by separate measures of positivity and negativity) on such interesting phenomena as the stability of attitudes, resistance to counterarguments, and the attitude-behavior relation.

Conclusion

The concepts of attitude and attitude strength are essential to understand the nature of the relations between members from different social groups. Thus, it is important to examine the validity of size-of-effect statements such as "relative ingroup prototypicality is negatively related to outgroup attitude" or "attitude ambivalence moderates attitude stability". The multivariate approach to testing such hypotheses proposed in the present work is an important first step in examining their validity. In this final discussion I have argued that a more complete understanding of the validity of these size-of-effect statements depends on a critical assessment of the assumptions of causality that pertain to the effects as well as the measurement of constructs involved. To the extent that future research adduces more conclusive evidence for the truth of these assumptions, research on intergroup relations will yield more meaningful results that can be straightforwardly applied in practice.

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Deutsche Zusammenfassung

Diese Dissertation beschäftigt sich mit der Validität von Schlussfolgerungen über empirische sozialpsychologische Forschung zu multivariaten Konstrukten. Multivariate Konstrukte implizieren einen mehrdimensionalen psychologischen Raum, in dem Untersuchungseinheiten verortet werden. Die bisherige Forschung zu den multivariaten Konstrukten *relative Eigengruppenprototypikalität* und *Einstellungsambivalenz* verwendet häufig Indexvariablen zur Repräsentation der Konstrukte, wobei mehrere gemessene Variablen auf eine reduziert werden. Die vorliegende Arbeit behauptet, dass diese Praxis zu uninterpretierbaren und irreführenden Ergebnissen führt. Erstens bleiben die Annahmen, die zur Konstruktion der Indexvariablen verwendet werden, ungeprüft. Zweitens können Modelle mit unterschiedlichen theoretischen Annahmen nicht gegeneinander getestet werden. Drittens kann diese Praxis zu Scheinergebnissen und Artefakten führen. Für beide Inhaltsbereiche wurde ein alternativer multivariater Ansatz vorgeschlagen, der diese Probleme löst.

Im Hinblick auf relative Eigengruppenprototypikalität wurde zunächst die Mehrdeutigkeit aufgezeigt, die ein statistischer Zusammenhang zwischen der entsprechenden Indexvariable und der Einstellung gegenüber einer Fremdgruppe beinhaltet. In einer empirischen Untersuchung wurde gezeigt, dass verschiedene Indexvariablen die Hypothese eines negativen Zusammenhangs nur scheinbar stützen (bzw. widerlegen) würden. Mit dem multivariaten Ansatz wurden Hinweise darauf erhalten, dass sich die Komponenten der Indexvariablen nicht wie theoretisch angenommen verhalten. Für den negativen Zusammenhang scheint allein die Variable der Fremdgruppenprototypikalität verantwortlich zu sein.

Im Hinblick auf Einstellungsambivalenz wurden exemplarisch zwei verschiedene Modelle verglichen, die in der Literatur oft zur Konstruktion von Indexvariablen verwendet werden: Das Similarity-Intensity-Modell (SIM) und das Conflicting Reactions Modell (CRM). Es wurde analytisch gezeigt, dass bivariate Korrelationen ungeeignet sind, um diese

Modelle gegeneinander zu testen, da bereits a priori Korrelationen unterschiedlicher Höhe zu erwarten sind. In zwei empirischen Studien wurde mit dem vorgeschlagenen multivariaten Ansatz gezeigt, dass das CRM subjektive Ambivalenz besser erklärt als das SIM, während die Modelle auf der Grundlage von bivariaten Korrelationen mit Indexvariablen ununterscheidbar gewesen wären. Weiterhin wurde analytisch gezeigt, dass der in der Literatur zu Einstellungsstärke häufig berichtete Moderatoreffekt von Einstellungsambivalenz unter plausiblen Bedingungen ein reines Artefakt sein kann. In zwei empirischen Studien wurde gezeigt, dass der bisherige methodische Ansatz der Konstruktion von Indexvariablen tatsächlich einen Moderatoreffekt nahegelegt hätte, während der multivariate Ansatz die trivialen Zusammenhänge zum Vorschein bringt, die für dieses Artefakt verantwortlich sind. Eine Monte-Carlo Simulation wurde benutzt, um die Wahrscheinlichkeit von Fehlern erster Art beim Testen von Moderatorhypothesen mit Indexvariablen zu bestimmen. Hierbei zeigte sich eine deutlich erhöhte Fehlerwahrscheinlichkeit, was die Plausibilität einer Interpretation von bisher gefundenen Moderatoreffekten als Artefakte erhöht.

Die analytischen und empirischen Ergebnisse unterstreichen die Notwendigkeit und Nützlichkeit eines multivariaten Ansatzes zur Überprüfung von Hypothesen über multivariate Konstrukte.

Danksagung

Ich danke Uli Wagner für das Vertrauen, den Realitätssinn und die gleichzeitige Geduld mit meinem Möglichkeitssinn, und die vielfältige inhaltliche Anregung und Unterstützung, die ich als Student und Doktorand genossen habe. Ebenfalls danke ich Rolf van Dick für sein Vertrauen und die lehrreiche und freundschaftliche Zusammenarbeit außerhalb meiner Promotion.

Lernen und Forschen ist aber ein Gruppenprozess, und ich bin dankbar für die wunderbare Zeit in der Arbeitsgruppe Sozialpsychologie Marburg und im Graduiertenkolleg „Gruppenbezogene Menschenfeindlichkeit", danke an euch alle. Schließlich waren die Diskussionen mit Björn Böttcher, Karin Schermelleh-Engel und Sven Waldzus bei der Fertigstellung der beiden Aufsätze unschätzbar wertvoll – danke!

Erklärung

Ich versichere, dass ich die vorliegende Dissertation „Statistical treatment of multivariate constructs in social psychology“ selbständig und ohne unerlaubte Hilfe angefertigt habe. Dabei habe ich mich keiner anderen als der von mir ausdrücklich bezeichneten Quellen und Hilfen bedient.

Die Dissertation wurde weder in der jetzigen noch in einer ähnlichen Form bei einer anderen Hochschule eingereicht und hat noch keinen sonstigen Prüfungszwecken gedient.

Johannes Ullrich

Ort und Datum