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Social Projection to Ingroups and Outgroups: A Review and Meta-Analysis

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Social projection is the tendency to expect similarities between oneself and others. A review of the literature and a meta-analysis reveal that projection is stronger when people make judgments about ingroups than when they make judgments about outgroups. Analysis of moderator variables further reveals that ingroup projection is stronger for laboratory groups than for real social categories. The mode of analysis (i.e., nomothetic vs. idiographic) and the order of judgments (i.e., self or group judged first) have no discernable effects. Outgroup projection is positive, but small in size. Together, these findings support the view that projection can serve as an egocentric heuristic for inductive reasoning. The greater strength of ingroup projection can contribute to ingroup-favoritism, perceptions of ingroup homogeneity, and cooperation with ingroup members.

Social projection can be defined as a process, or a set of processes, by which people come to expect others to be similar to themselves. Associations between judgments about the self and judgments about the group capture this phenomenon statistically. The functional importance of social projection lies in the fact that it offers a readily available, though egocentric, window into the social world. Using their own dispositions or preferences as data, people can make quick predictions of what others are like or what they are likely to do. As a judgmental heuristic based on mental simulation or anchoring, social projection often yields correct predictions, although it is hardly error-free (see Krueger, 1998, 2000, for reviews).

An early meta-analytic review showed that social projection is a robust phenomenon with a medium to large effect size (Mullen et al., 1985). Variations in the effect size have been of interest because they provide clues to the underlying mental processes. For example, one cognitive-perceptual account suggests that projection increases as people gain familiarity with a stimulus. The easier it is to generate a response, the more readily a person will assume that others respond likewise (Kelley & Jacoby, 1996). Alternatively, one motivational account suggests that projection increases when a need for social connectedness is activated. When, for example, their own mortality is made salient, many people project more vigorously to others, presumably in an effort to stave off anxiety (Arndt, Greenberg, Solomon, Pyszczynski, & Schimel, 1999). Of the many mechanisms that have been proposed to explain projection, most have been shown to matter. None, however, has turned out to be necessary for social projection to occur (Krueger, 1998).

Participants in a typical study make judgments about groups to which they themselves belong. These could be broad social categories, such as the national population, or narrowly defined groups, such as fellow students in a classroom. Beginning in the 1960s, some reports suggested that people project comparatively little—if at all—when making judgments about groups to which they do not belong (Bramel, 1963; Ward, 1967). Thus, variations in the social context, rather than direct manipulations of psychological processes, emerged as a possible way of eliminating social projection. By now, a sufficient number of such studies is available for meta-analytic research. We performed such a meta-analysis with two main objectives in mind. First, we sought to establish credible estimates for the average strength of ingroup projection and the average strength of outgroup projection. Second, we explored the credibility of several process-related hypotheses, which have not been (or could not be) adequately tested in single empirical studies. To meet this objective, we examined systematic variations in certain design characteristics. Given the general expectation that ingroup projection is larger than outgroup projection, the exploration of potential moderator effects was focused on the variability of projection to ingroups. With regard to outgroups, our primary interest was to ascer-

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tain whether the effect sizes cluster around zero, or whether they tend to be positive or negative.

Moderator Variables

The designs of projection studies differ in three important ways. The first difference is whether social categorization refers to groups that exist in the social world or to groups that are created ad hoc in the laboratory. The second difference is whether the participants or the stimulus items are treated as the units of analysis. The third difference is whether participants make self-judgments before or after they make group-judgments. Each of these design features is associated with a distinct theoretical issue.

Context of Categorization

The effect of social categorization has been studied by both correlational and experimental means. In the correlational approach, pre-existing membership in social groups is determined through observation of fairly obvious characteristics (e.g., gender) or through participants' self-reports (e.g., political party affiliation). Membership in these categories tends to be stable over time, which typically entails considerable levels of familiarity with other group members (Linville, Fischer, & Salovey, 1989). In contrast, experimental studies impose a classification on each participant. Typically, participants complete a psychometric task and then receive feedback regarding their group membership. This feedback, be it bogus or bona fide, creates fleeting social groups that are novel and thus unfamiliar. When making group-judgments, participants can neither rely on pre-existing stereotypes nor on specific knowledge gathered from contact with individual group members.

How might the context of social categorization affect the strength of projection? A priori, it is clear that projection to real groups will either be stronger, weaker, or equal to projection to laboratory groups. The latter is a *null hypothesis*, which reflects the simple idea that people project to whatever ingroup is salient at the time. In contrast, the *selective-exposure hypothesis* suggests that projection is stronger in real groups (Marks & Miller, 1987). If people surround themselves selectively with others who share their responses and attributes, their available samples are egocentrically biased. If people then generalize the attributes they observe to the group at large, the correlations between self-judgments and group-judgments should be stronger in real than in laboratory groups, because in the latter, no opportunities for biased sampling exist (Fiedler, 2000). It is possible, however, that exposure to other group members reduces social projection. Hence, we refer to this idea as the *reduction hypothesis*. It suggests that projection diminishes inasmuch as people possess knowledge of other individuals, which only happens when group are real, but not when they are ad hoc (Krueger & Stanke, 2001).

Unit of Analysis

In the traditional "false-consensus" approach to social projection, the judgment items are treated as the units of analysis. This approach is *nomothetic* in that the correlations between self-judgments and group-judgments are computed across participants (e.g., Ross, Greene, & House, 1977; Wallen, 1943). Approaching social projection from a Brunswikian perspective, Hoch (1987) suggested that participants be used as the units of analysis, which requires *idiographic* analyses. Here, each participant's self-judgments are correlated with that person's group-judgments across a set of items (e.g., Dawes & Mulford, 1996; Krueger & Zeiger, 1993).

The *equivalence hypothesis* is that nomothetic and idiographic effect sizes are similar in size. Consider a study in which multiple participants make judgments about multiple items. The data can be organized into two matrices, one containing self-judgments and the other containing group-judgments. In each matrix, persons are coded in the rows and items are coded in columns. The extent of assumed similarity (i.e., projection) can be evaluated by correlating self-judgments with group-judgments across all cells of the matrix. The nomothetic approach is to compute the average correlation between sets of corresponding rows, and the idiographic approach is to compute the average correlation between sets of columns. Because the same information is used, the two averages may be expected to be similar (Kenny & Winquist, 2001; Krueger, 2002).

It is possible, however, that the average correlations differ depending on the method of aggregation. The *divergence hypothesis* is that idiographic correlations are larger than nomothetic correlations. Such an asymmetry may occur when judgments are more variable across items than across people. Suppose participants make judgments with regard to the personality-descriptive adjectives 'friendly,' 'intelligent,' 'selfish,' and 'weak.' Most endorse the first two items and reject the last two. If people correctly believe friendliness and intelligence to be the more popular items (Sherman, Chassin, Presson, & Agostinelli, 1984), their idiographic projection correlations will be larger than their nomothetic correlations.

The competing predictions of the equivalence and the divergence hypotheses have an important implication for the processes underlying projection. According to one view, people generate highly accurate group-judgments because they project from themselves while neglecting other valid information that is available about the group (Krueger, 2000). According to another view, people do not give egocentrically inflated weights to self-referent information (Engelmann & Strobel, 2000). Social knowledge eventually overwhelms self-referent knowledge because it can be aggregated in ever larger samples. In the extreme, people may come to a full understanding of the different rates at which group members endorse various personality traits as being self-descriptive. That is, they obtain enough valid social information to make accurate group-judgments without relying on projection. For example, the average idiographic correlation between group-judgments and actual trait base rates might be .9, falling short of the maximum value of 1.0 only because of random estimation error. At the same time, the average correlation between self-judgments and base rates may be around .5, reflecting the typical part-whole correlations between individual profiles of self-judgments and the group profile (i.e., the means of all self-judgments; Krueger, 1998). Without any added egocentric bias, the average idiographic correlation between self-judgments and group-judgments is then .45 (i.e., 9 · .5). If the same judgments are analyzed nomothetically, the correlations between self-judgments and group-judgments will be near zero because all valid knowledge of differences in the endorsement base rates has been stripped away, leaving the random estimation errors as the only source of variability. If, however, a projective bias is necessary for the attainment of accuracy, its effects should be seen equally in nomothetic and idiographic correlations.

Order of Judgments

Throughout the history of projection research, most investigators did not ascribe much theoretical relevance to the order in which self-judgments and group-judgments are made. This *null hypothesis* follows from the idea that correlations between the two types of judgment reflect a causal flow from self-perception to social perception. In several studies, it has been shown that people make self-judgments faster than they make group-judgments, and we know of no evidence to the contrary (Krueger, Acevedo, & Robbins, in press). When a stimulus item is presented for judgment, a person's own response may readily come to mind even when the experimental design invites group-judgments first. In other words, according to this null hypothesis, the order in which self-judgments and group-judgments are made explicitly is irrelevant.

Mullen et al. (1985) discovered that projection was stronger when participants made group-judgments first (M = .53) rather than last (M = .27). These authors speculated that projection [a.k.a. "false consensus"] is largely unintended, for if it "were the result of an intentional self-presentation strategy, one would expect the false consensus effect to be more extreme when the subject makes their [*sic*] behavioral choice before generating an estimate of consensus" (pp. 279–280). Karniol (2003) recently offered a different interpretation, suggesting that projection is not engaged when group-judgments are made first. Instead, correlations between the two types of judgment are assumed to reflect processes by which the self-image is assimilated to a group prototype (but see Krueger, 2003). For the present purposes, we refer to this view as the *group-anchoring hypothesis*. In contrast, Cadinu and Rothbart (1996) suggested (and found) that correlations between self-judgments and group-judgments are largest when self-judgments are made first. According to their model, self-judgments serve as anchors for group-judgments. Hence, we refer to this view as the *self-anchoring hypothesis*.

Projection to Outgroups

We had two general and interrelated expectations with regard to projection to outgroups. The first expectation was that compared with ingroup projection, outgroup projection would be small; the second expectation was that none of the three potential moderator variables would play any noticeable role. The latter expectation may simply reflect a lack of specific theoretical claims as to what these moderator effects might be. Nonetheless, we took advantage of the exploratory powers of meta-analysis to test the same models for both ingroup and outgroup projection.

Aside from the possibility of moderation, the literature on the perception of outgroups can be partitioned into three hypotheses. According to the *null hypothesis*, people do not project to outgroups. The rationale of this hypothesis is that the egocentric mechanisms underlying projection are engaged only when group membership has been established. Without categorization of the self nothing really happens (Krueger, 2000; Otten & Wentura, 1999).

According to the *induction hypothesis*, however, people do project to outgroups, albeit not as much as they project to ingroups. The rationale for this hypothesis derives from the hierarchical nature of social categories. Whichever distinction is made to separate people into ingroups and outgroups, it can always be undone by subsuming both groups under a superordinate social category. In the limiting case, humanity is such a category (Oakes, Haslam, & Turner, 1994). The multiplicity of possible social groupings means that any particular person may be seen as an ingroup member or an outgroup member depending on which categorical distinction is made at the time. Colleagues at a faculty party, for example, may belong to the same gender category but to different academic departments or vice versa. Despite these alternate options for categorization, the personal preferences or habits of these colleagues cannot be both similar and different with respect to the perceiver. As long as there is any basis for shared categorization, the similarities will outweigh the differences (see Migdal, Hewstone, & Mullen, 1998, for a review of research on cross-categorization; and Gaertner, Mann, Murrell, & Dovidio, 2001, for a review of research on re-categorization).

The induction hypothesis has some normative, prescriptive force; it demands that people extend their projective inferences to outgroups. This should be so especially when the stimulus items are personality traits. For such items, differences among individuals are more easily envisioned than differences between groups (Gidron, Koehler, & Tversky, 1993; Goldberg, 1982). To illustrate, consider again the traits of friendliness, intelligence, selfishness, and weakness. It seems safe to assume that members of any group more readily judge the first two than the latter two to be self-descriptive. Inasmuch as social perceivers realize this, they should project to both ingroups and outgroups, and thus come to acknowledge their actual similarities.

In contrast to the induction hypothesis, the *differentiation hypothesis* suggests that people project negatively or inversely to outgroups. A cognitive variant of this hypothesis is that people employ a heuristic of "oppositeness," leading them to assume that whatever feature is common in one group, is uncommon in the other (Cadinu & Rothbart, 1996). A motivational variant is that both regular ingroup projection and inverse outgroup projection satisfy psychological needs. Whereas the perception of an ingroup as being similar to the self satisfies the need to belong and to be "normal," the perception of an outgroup as being different from the self satisfies the need to be unique and separate from others. Together, the need to be similar and the need to be different serve the superordinate need to have a clearly demarcated social identity (Brewer & Roccas, 2001).

Table 1 presents the 12 hypotheses at one glance. Note that this list reflects the heterogeneity of hypotheses typically encountered in social psychology. After reading the empirical literature, but before doing the quantitative meta-analysis, we already regarded Hypothesis 1 (*Differential Projection*) as being strongly supported. The objective of the meta-analysis was to provide a stable estimate of the average effect size. Three hypotheses are null hypotheses, which, strictly speaking cannot be true (e.g., Krueger, 2001; Nickerson, 2000), whereas the remaining hypotheses only refer to the sign of the mean effect sizes. Because the hypotheses listed beneath each particular question of interest are mutually exclusive, the meta-analysis offers an opportunity for hypothesis reduction (Rosenthal & DiMatteo, 2001). By the end of this exercise, some hypotheses will have gained credibility, whereas others will be rendered implausible.

Methods

Because meta-analyses require a number of strategic decisions (Wood & Christensen, 2003), we begin by reviewing our choice of criteria for the inclusion of studies, our efforts to identify relevant empirical reports and code the a priori moderator variables, and our selection of statistical tools.

Selection Criteria

Studies were included if they met the following criteria. First, studies had to be published in English before June 2002 or be available as unpublished data at that time. Second, participants had to make self- and group-judgments for the same stimulus items. Indices of association between self- and group-judgments had to be either reported or it had to be possible to recover them from test statistics and sample size information. Third, social categorization had to be a design variable such that participants made judgments regarding clearly defined ingroups or outgroups. We did not include studies in which all judgments referred only to

Table 1. *Hypotheses About Projection to Ingroups and Outgroups*

1. General
* Differential Projection: People project more to ingroups than to outgroups.
2. Ingroup Projection
  2.1. Context of social categorization
    2.1.1. Null hypothesis: People project to real groups and laboratory groups at the same rate.
    2.1.2. Selective-exposure: People project more to real than to laboratory groups.
*   2.1.3. Reduction: People project less to real than to laboratory groups.
  2.2. Unit of Analysis
*   2.2.1. Equivalence: Idiographically and nomothetically assessed projection effects are of the same size.
    2.2.2. Divergence: Idiographic projection effects are larger than nomothetic effect.
  2.3. Order of Judgments
*   2.3.1. Null hypothesis: The size of the projection effect does not depend on the order in which self judgments and group judgments are made.
    2.3.2. Group-anchoring: Projection effects are larger when group judgments are made first.
    2.3.3. Self-anchoring: Projection effects are larger when self judgments are made first.
3. Outgroup Projection
  3.1. Null hypothesis: The size of the outgroup projection effect hovers around $r = 0$.
* 3.2. Induction: People project (perhaps weakly, but significantly) to social outgroups (i.e., $r > 0$).
  3.3. Differentiation: People contrast outgroups away from the self so that $r < 0$.

ingroups or only to outgroups. Likewise, we did not include studies examining social projection to known individual group members. Finally, if participants provided self-judgments and group-judgments more than once, we included only the first set of judgments.

Search Strategy

Initial searches were conducted with online databases (e.g. PsychInfo, PsychArticles, PubMed, and Social Science Citations Index). Keywords included "assumed similarity," "categorization," "consensus estimation," "false consensus," "ingroup-favoritism," "outgroup homogeneity," "projection," "self-anchoring," "self-stereotyping," and "social identity." Based on the initial yield, the search was extended using the "ancestry" and "descendency" approaches. Using the "invisible college" approach (Mullen, 1989), we posted a request for suitable data on the listserv of the *Society for Personality and Social Psychology.* A total of 19 reports involving 48 hypothesis tests and 5,053 participants met the inclusion criteria. Table 2 lists the studies, their design properties, and the reported effect sizes.

Selecting Moderator Variables and Coding Effect Sizes

The first author entered the relevant study information into a database. Double entry yielded only minor inconsistencies, which were reconciled in consultation with the second author. Pearson's r was computed for each hypothesis test as the effect size of social projection. Most of the studies reported this measure; for the remainder, it was recovered from other published information. In five cases (Holtz, 1997, 2003; Holtz & Miller, 2001; Monin & Norton, 2003; Otten, unpublished), the authors kindly shared their raw data or the necessary information to be included in the analysis.

Inspection of the empirical studies revealed differences in design reflecting the three a priori moderator variables. About half (52%) of the hypothesis tests were conducted with real target groups (e.g., academic affiliation, Holtz, 2003; nationality, Li & Hong, 2001; or ethnicity, Granberg, Jefferson, Brent, & King, 1981), about half (50%) used nomothetic analyses, and in about half (52%), self-judgments preceded group-judgments. When both methods of analysis were used in the same study (e.g., Clement & Krueger, 2002), the idiographic effects were entered.

Preliminary analyses revealed that the predictor variables were correlated with one another. Compared with studies using real target groups, studies with laboratory groups were more likely to elicit group-judgments first, $r = .60$, and to employ idiographic analyses, $r = .53$. There was also a correlation between order and analysis, such that idiographic analyses were more common in studies in which self-judgments were made first, $r = .65$.

Analysis

Pearson r coefficients were converted to Fisher Z scores and mean weighted effect sizes were computed as $\Sigma((n_j - 3)Z)/\ \Sigma n_j - 3))$, where $n_j - 3$ is the inverse of the standard error of the correlation coefficient. The weighted means and the bounds of their 95% confidence intervals were then converted back to correlation coefficients. The total variability of the effect sizes was partitioned into the systematic components due to moderator variables and to random sampling error. This was achieved with a weighted multiple regression (WMR) approach, which was computed in SPSS, version 11.0 (SPSS, Inc., 2001) with the modified syntax suggested by Lipsey and Wilson (2001, pp. 216–220). The significance of the variance due to systematic effects was indexed by the Q_R statistic, which follows a χ^2 distribution and whose degrees of freedom are the number of predictor variables in the model. The significance of the variance due to individual moderator variables was evaluated by z-tests for unstandardized regression weights, and the standardized regression weights were reported. The significance of the residual variance was indexed by the Q_E statistic with the number of effect sizes minus the number of predictor variables minus 1 as the degrees of freedom. When Q_E is not significant, it may be concluded that no systematic predictor variables were overlooked. After the initial models were estimated, the models were refit using only the significant predictors (Cooper & Hedges, 1994; Hedges & Olkin, 1985).

Ideally, a meta-analysis is performed on independent effect sizes, but this requirement is often difficult to meet. We therefore used three levels of integration. At the first level, the effects of the individual hypothesis tests were entered. At the second level, hypothesis tests were aggregated within studies before analysis. At the third level, only those studies were included in which ingroup judgments and outgroup judgments were collected from different participants. This last analysis provided the most unbiased test of the differential-projection hypothesis (Matt & Cook, 1994).

Results

Hypothesis Tests

The findings of one study (Spears & Manstead, 1990) were excluded from the first two rounds of analysis (individual hypothesis tests and study reports as units of analysis) because no information on the order of judgments was available.

Table 2. *Study Characteristics*

	Ingroup			Outgroup			Study			
Experiment	Test	N	Zr	Test	N	Zr	Context	Order	Analysis	Design
Bramel (1963)	t(46) = 3.16	48	0.45	t(47) = 0.42	49	0.06	Laboratory	S-O	Nomo	Btwn
Cadinu and Rothbart	Beta = 0.24	39	0.25	beta = –0.24	40	–0.25	Laboratory	O-S	Idio	Btwn
(1996)	Beta = 0.67	40	0.81	beta = 0.11	37	0.11	Laboratory	S-O	Idio	Btwn
	Beta = 0.55	41	0.62	beta = 0.04	42	0.04	Laboratory	O-S	Idio	Btwn
	Beta= 0.64	41	0.76	beta = 0.01	42	0.01	Laboratory	S-O	Idio	Btwn
Clement (1995)	r(8) = 0.62	22	0.73	r(8) = 0.14	22	0.14	Laboratory	O-S	Idio	Within
	r(8) = 0.67	22	0.81	r(8) = –0.26	22	–0.27	Laboratory	O-S	Idio	Within
	r(8) = 0.73	21	0.93	r(8) = 0.07	21	0.07	Laboratory	O-S	Idio	Within
Clement and Krueger	r(8) = 0.57	40	0.65	r(8) = 0.04	40	0.04	Laboratory	O-S	Idio	Within
(2002)	r(8) = 0.55	40	0.62	r(8) = 0.05	40	0.05	Laboratory	O-S	Idio	Within
	r(8) = 0.65	21	0.78	r(8) = –0.01	21	–0.01	Laboratory	O-S	Idio	Btwn
	r(8) = 0.69	21	0.85	r(8) = –0.22	21	–0.22	Laboratory	O-S	Idio	Btwn
	r(8) = 0.56	36	0.63	r(8) = 0.03	36	0.03	Laboratory	O-S	Idio	Btwn
	r(8) = 0.54	36	0.60	r(8) = –0.13	36	–0.13	Laboratory	O-S	Idio	Btwn
Clement and Krueger	r(8) = 0.42	24	0.45	r(8) = 0.00	24	0.00	Laboratory	O-S	Idio	Within
(unpublished)	r(8) = 0.28	32	0.29	r(8) = –0.01	32	–0.01	Laboratory	O-S	Idio	Within
de La Haye (2001)	r(48) = 0.30	50	0.31	r(48) = 0.02	50	0.02	Real	S-O	Idio	Within
	r(86) = 0.34	88	0.35	r(86) = 0.09	88	0.09	Real	S-O	Idio	Within
Granberg, Jefferson,	r(1571) = 0.48	1573	0.52	r(1571) = 0.22	1573	0.22	Real	S-O	Nomo	Within
Brent, and King	r(1382) = 0.42	1384	0.45	r(1382) = 0.10	1384	0.10	Real	S-O	Nomo	Within
(1981)	r(231) = 0.44	233	0.47	r(214) = 0.15	216	0.15	Real	S-O	Nomo	Within
	r(477) = 0.31	479	0.32	r(476) = 0.31	478	0.32	Real	S-O	Nomo	Within
Holtz (1997)	r(14) = 0.54	16	0.60	r(14) = 0.37	16	0.39	Real	S-O	Nomo	Btwn
	r(18) = 0.62	20	0.73	r(18) = 0.52	18	0.58	Real	S-O	Nomo	Btwn
Holtz (2003)	r(27) = 0.55	29	0.63	r(32) = 0.18	34	0.18	Real	S-O	Nomo	Btwn
	r(36) = 0.20	38	0.20	r(35) = 0.21	37	0.21	Real	S-O	Nomo	Btwn
	r(34) = 0.36	36	0.38	r(33) = 0.07	35	0.07	Real	S-O	Nomo	Btwn
Holtz and Miller	r(39) = 0.72	41	0.90	r(40) = 0.31	42	0.32	Laboratory	S-O	Nomo	Btwn
(2001)	r(40) = 0.72	42	0.91	r(38) = 0.10	40	0.10	Laboratory	S-O	Nomo	Btwn
Hort and Rothbart	r(52) = 0.31	54	0.32	r(52) = 0.23	54	0.23	Real	S-O	Nomo	Btwn
(unpublished)	r(48) = 0.42	50	0.45	r(48) = 0.16	50	0.16	Real	S-O	Nomo	Btwn
Krueger and Clement	r(8) = 0.63	25	0.74	r(8) = 0.40	27	0.42	Laboratory	O-S	Idio	Within
(1996)	r(8) = 0.56	24	0.63	r(8) = 0.31	28	0.32	Laboratory	O-S	Idio	Within
	r(8) = 0.63	22	0.74	r(8) = 0.21	21	0.21	Laboratory	O-S	Idio	Within
Krueger and Zeiger	r(16) = 0.47	21	0.51	r(16) = –0.09	25	–0.09	Real	O-S	Idio	Btwn
(1993)	r(16) = 0.45	27	0.49	r(16) = 0.02	27	0.02	Real	O-S	Idio	Btwn
Li and Hong (2001)	r(18) = 0.42	106	0.45	r(18) = 0.26	106	0.27	Real	S-O	Idio	Within
	r(18) = 0.45	64	0.49	r(18) = 0.41	64	0.44	Real	S-O	Idio	Within
Monin and Norton	r(118) = 0.50	120	0.54	r(114) = 0.31	116	0.32	Real	S-O	Nomo	Within
(2003)	r(41) = 0.47	43	0.51	r(39) = 0.04	41	0.04	Real	S-O	Nomo	Within
	r(66) = 0.41	68	0.43	r(65) = 0.15	67	0.15	Real	S-O	Nomo	Within
	r(95) = 0.27	97	0.28	r(93) = 0.02	95	0.02	Real	S-O	Nomo	Within
Mullen, Dovidio,	r(79) = 0.40	81	0.43	r(79) = 0.10	81	0.10	Real	O-S/S-O	Nomo	Within
Johnson, and	r(93) = 0.36	95	0.38	r(93) = –0.09	95	–0.09	Real	O-S/S-O	Nomo	Within
Copper(1992)	r(82) = 0.31	84	0.32	r(82) = –0.18	84	–0.18	Real	O-S/S-O	Nomo	Within
Otten (unpublished)	r(19) = 0.37	21	0.39	r(19) = 0.23	21	0.24	Laboratory	S-O	Nomo	Within
	r(21) = 0.57	23	0.65	r(21) = 0.45	23	0.48	Laboratory	O-S	Nomo	Within
Spears and Manstead (1990)	F(1,91) = 4.69	93	0.47	F(1,91) = 3.32	93	0.34	Real		Nomo	Within

Figure 1 shows that people project more strongly to ingroups than to outgroups. By Cohen's (1988) benchmarks, the average effect size for ingroup projection was large (M = .46, CI: .42 to .50), whereas the average effect size for outgroup projection was small (M = .13, CI: .08 to .18). The size of the differential projection effect is expressed by Cohen's q, which is the difference between the two Z-scored correlations (here, q = .36).

Turning to the role of the moderator variables, we found the WMR model for *ingroup projection* to be significant, $Q_R(3) = 29.71$, $p < .001$. It accounted for more than one-third of the variance in projection, R^2 = .42. Of the moderator variables, only the context of categorization was significant, $\beta = .73$. Projection to laboratory groups was greater (M = .57, CI: .52 to .62) than projection to real groups (M = .40, CI: .37 to .43), q = .22. The unit of analysis made no detectable difference,

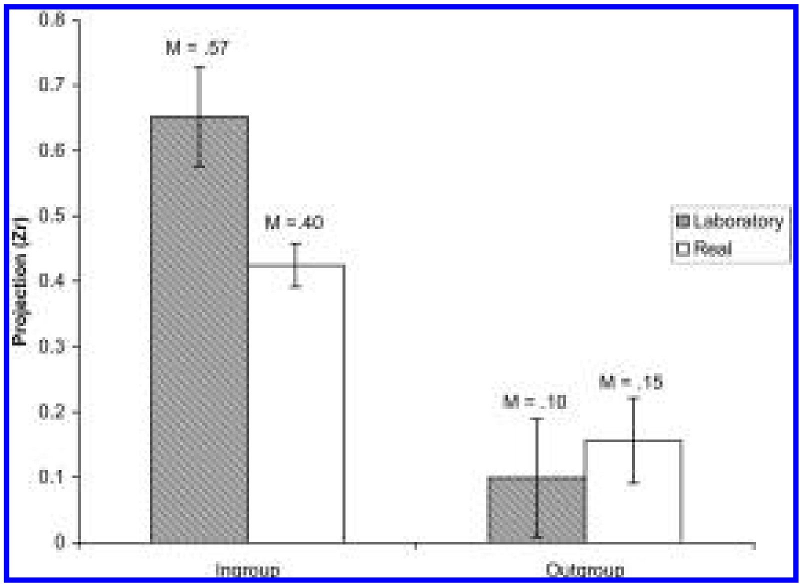

Figure 1. **Projection to ingroups and outgroups depending on the context of social categorization.**

β = –.01, and the small effect of the order of judgment, β = .17, suggested that there was trend for projection to be stronger when self-judgments were made last. The residual variance was unsystematic, $Q_E(43) = 41.79$. The final estimate of the model therefore only included the context variable as a moderator. This model was significant, $Q_R(1) = 28.26$, $p < .001$, $R^2 = .40$, and the context of group categorization remained a strong moderator, β = .63. The residual variation was unsystematic, $Q_E(45) = 43.24$. The model for *outgroup projection* was not significant, $Q_R(3) = 1.73$, $R^2 = .04$, nor was the residual variance, $Q_E(43) = 45.24$, suggesting that outgroup projection was not moderated by the design variables under consideration.

Study Reports

We created 20 independent effect sizes by aggregating across hypothesis tests within each study. For two studies (Cadinu & Rothbart, 1996; Otten, unpublished), separate effect sizes were computed for each order of judgments. Because order remained a potential moderator variable, this information had to be preserved. Figure 2 displays the weighted means in a scatterplot. The ingroup-outgroup difference in projection is readily apparent, as is the effect of context of categorization on ingroup projection. The absence of outliers can be noted. For real groups, the effect sizes for ingroup and outgroup projection were correlated, $r(8) = .67$, $p = .047$. For laboratory groups, this correlation was weak, $r(8) = .34$, $p = .33$.

As expected, the results of the initial models closely replicated the results obtained when integrating individual hypothesis tests. The model for *ingroup projection* was significant, $Q_R(3) = 17.85$, $p < .001$, $R^2 = .49$, and only the context of categorization affected the strength of projection, β = .77. The unit of analysis, β = –.01, had no effect, and the effect of judgment order was again small but nonsignificant, β = .17. As before, the residual variance could be considered random, $Q_E(15) = 18.31$. The final model, with context being the only included moderator was significant, $Q_R(1) = 18.71$, $p < .001$, $R^2 = .48$, with a strong effect of context, β = .69. The residual variance remained random, $Q_E(17) = 20.07$. The model for *outgroup projection* was not significant, $Q_R(3) = .88$, $R^2 = .05$, as was the residual variance, $Q_E(15) = 16.82$.

Test of the Ingroup-Outgroup Difference in Projection

As a final test, we examined only the 10 studies in which separate samples of participants rated ingroups and outgroups. The model, which included only target group as a moderator variable, was significant, $Q_R(1) = 22.54$, $p < .001$, $R^2 = .59$, β = .77, and the residual variance was not, $Q_E(16) = 15.88$. Because the means for ingroup projection ($M = .50$, CI: .41 to .58) and outgroup projection ($M = .14$, CI: .02 to .26) were similar to the means obtained in the preceding analyses, we considered the threat of non-independence seen in tests at the hypothesis level to be minor.

Summary and Follow-Up

By integrating the findings from a domain of research, meta-analyses can enhance the “accuracy, simplicity, and clarity” of the conclusions drawn from the empirical base (Rosenthal & DiMatteo, 2001, p. 68). With regard to the goal of accuracy, we were encouraged by the fact that different ways of setting up

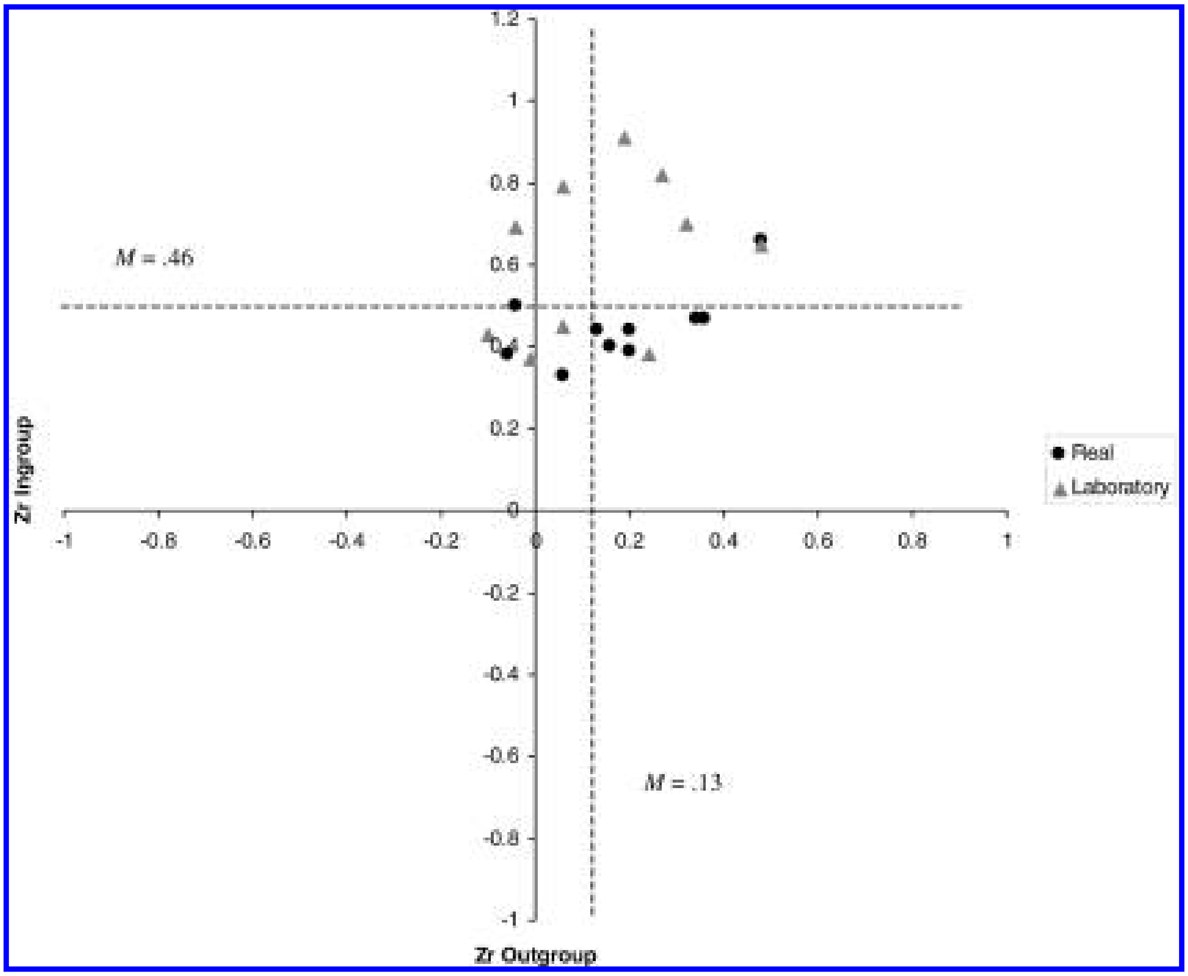

Figure 2. Scatterplot of differential projection for analyses at the study level.

the analyses yielded convergent results. With regard to simplicity, our main result was a large effect of differential projection. The point-biserial correlation between the type of group (ingroup vs. outgroup) and the size of the projection effect was $r = .74$, which is impressive considering that the typical effect size in social psychology is .21 (Richard, Bond, & Stokes-Zoota, 2003). A useful way of representing this finding is the binomial effect size display (Rosenthal & Rubin, 1982). This transformation shows that 87% of the ingroup projection effects and 13% of the outgroup effects were larger than the overall median effect size of $r = .35$. An equivalent observation is that ingroup projection was 41 times more likely than outgroup projection to be above the median. A further testimony to the robustness of the differential effect is that alternative types of measures yield convergent results (see Allen & Wilder, 1979; Schubert & Otten, 2002; Wilder, 1984, for examples). With regard to clarity, we obtained the expected benefit of "hypothesis reduction." Four of the initial 11 specific hypotheses were supported; doubt was cast on the others. The supported hypotheses are marked with an asterisk in Table 1.

Analyses of moderator variables showed that ingroup projection varied with the context of categorization. For real groups, the average effect size was nearly the same as the one reported in a meta-analysis 20 years ago (Mullen et al., 1985). For laboratory groups, the average effect was larger. This discrepancy supported the *reduction hypothesis*, which suggested that people project less to the group when they have knowledge of individual others. Neither the null hypothesis nor the selective-exposure hypothesis could account for this finding. According to the null hypothesis, conceiving oneself as a member of a real group should not alter one's perceived similarity with the group, and according to the selective-exposure hypothesis, perceptions of similarity should increase instead of decrease.

The unit of analysis did not yield a significant moderator effect, which supported the *equivalence hypothesis*. The divergence hypothesis had suggested that nomothetic projection correlations (i.e., computed across people and within items), but not idiographic correlations (i.e., across items and within people) would become smaller if people used other-referent knowledge when making group-judgments. Obviously, this is more likely to happen in real groups than in laboratory groups. Therefore, we conducted a follow-up analysis, in which we regressed the effect sizes on the group context (real vs. laboratory), the type of analysis (idiographic vs. nomothetic), and the interaction between the two. Irrespective of the level at which the analysis was conducted (i.e., at the level of hypothesis tests or at the level of studies), the interaction term was not significant for either ingroup or outgroup projection (β ranging from –.21 to –.03, all $p > .05$). Compared with laboratory groups, idiographic and nomothetic projection effects in real groups were reduced at the same rate.

Finding support for both the reduction and the equivalence hypothesis is somewhat puzzling. We propose the following resolution. Recall that if people had—and used—valid samples of information about real ingroup characteristics, nomothetic projection effects should be reduced at a higher rate than idiographic effects. In the limiting case, nomothetic projection effects would be eliminated in real groups, however idiographic effects would still reflect the differences among the base rates with which judgment items are endorsed. The finding that nomothetic effects were as large as idiographic effects when information from other group members was available (i.e., in real groups) suggests that people still failed to weight other-referent information optimally (Alicke & Largo, 1995; Clement & Krueger, 2000; Krueger & Clement, 1994). This relative insensitivity to social information has been strikingly demonstrated in research on the acceptance of advice (e.g., Lim & O'Connor, 1995; Sorkin, Hayes, & West, 2001). Yaniv (2004), who found that even people motivated to make the most accurate judgments discounted the estimates of others, suggested that this "self/other" effect reveals a "fundamental asymmetry," such that "decision-makers can assess what they know and the strength of their own opinions, but they are far less able to assess what [others] know and the reasons underlying [their] opinions" (p. 9). In short, we suspect that when people make judgments about a real group, they only conclude that they should project less, but they fail to see that they should weight other information more.

The effect of judgment order found in an earlier meta-analysis (Mullen et al., 1985) emerged only as a trend in the present analysis. Ingroup projection was somewhat larger when participants made group-judgments first (M = .50) rather than last (M = .44, $\beta_{\text{univariate}} = -.20$, $p = .16$ (at the level of hypothesis tests). However, the regression analyses were not sensitive to the possibility that the order of judgment has opposite effects for real and laboratory groups. Because participants in laboratory groups have little to rely on besides their own responses, they may be particularly inclined to project when they make self-ratings first (Cadinu & Rothbart, 1996). When there is ample social knowledge, as is often the case in real groups, there may be less need for self-anchoring. People might even model their own responses, in part, after these perceived social norms, and especially so when they make group judgments first (Karniol, 2003). To examine this possibility, we regressed the ingroup effects on the context variable (real vs. laboratory), the order of the judgments (self first vs. last), and the interaction between the two. No significant interactions were found either at the level of hypothesis tests or at the level of studies (b ranging from –.20 to .09, all $p > .05$). In other words, social projection to laboratory groups was stronger than projection to real groups regardless of the order in which self and group judgments were made.[1]

Insufficient Projection to Outgroups?

With regard to outgroups, no moderator effects were expected or found. Here, the main question was concerned with the size of the effect and how to best account for it. Being small and positive, the aggregate effect size was inconsistent with cognitive or motivational hypotheses assuming negative projection. There was no evidence that people view outgroups as opposites of themselves or that they contrast these groups away from the self to satisfy personal or social needs. This is not to say, however, that particular circumstances might be identified in which projection is negative. If so, such effects might await detection in future integrations of the literature.

It is critical to examine whether the small positive effect for outgroup projection is consistent with the induction hypothesis. Whereas the null hypothesis is precise, the induction hypothesis merely suggests a positive effect lying somewhere between zero and the level of ingroup projection. In trying to make sense of the obtained effect size of $r = .13$, it is useful to remember that a small positive effect can emerge without projection. To illustrate, consider first the possibility that this effect does signify outgroup projection. If so, the correlation between ingroup judgments and outgroup judgments can be obtained as the product of ingroup projection and outgroup projection (i.e., $.46 \cdot .13 = .06$). That is, the perceived attributes of ingroups and outgroups would be nearly independent if the two projection effects were assumed to be the only mediating factors. The question can then be rephrased as follows: How strong would perceptions of similarity between ingroups and outgroups have to be so that the correlation between self-judgments and outgroup judgments is at its meta-analytically observed value? The answer

[1]Our consistent finding that the residual variability of the effect sizes remained indistinguishable from sampling variability suggests that no hidden moderator variables were overlooked. However, this conclusion must remain tentative until the literature is large enough to permit simultaneous analysis of more than three moderators with adequate power (Hedges & Pigott, 2001).

is obtained by dividing the outgroup projection effect by the ingroup projection effect (i.e., .13/.46 = .28). If people only project to ingroups and if they also have a sense of intergroup similarity independent of any projective tendency, a small but spurious outgroup projection effect will result.[2]

Our view is that induction, done properly, would lead people to project more strongly to outgroups. Particularly in the laboratory, participants could be expected to realize that members of different groups are similar to one another in most respects. Most people share at least some of the perceiver's ingroups, and thus should be seen as similar. Instead, we found particularly large differences between ingroup and outgroup projection for laboratory groups. As long as the ingroup-outgroup differential in projection persists, the comparative lack of outgroup projection violates the rationale of inductive reasoning and the mandate of coherence (Dawes, 1998). Thus, perceptions of outgroups may be less accurate than perceptions of ingroups (e.g., Judd, Ryan, & Park, 1991; Ryan & Bogart, 2001) in part because people make insufficient use of self-knowledge.

Given the size of the ingroup projection effect, the ease with which it is produced, and the difficulty of eliminating it, one needs to ask how people manage to partially disable projection to outgroups. Two hypotheses are worth testing. One is that when a person realizes that he or she is not a member of a certain group, the process of projection is not engaged in the first place (Otten & Wentura, 1999). Another possibility is that projection is engaged, but that effortful processes of inhibition are partially successful in suppressing it (Keysar, Barr, Balin, & Brauner, 2000). According to both of these scenarios, it is the small effect of outgroup projection that is more theoretically provocative than the large ingroup projection effect.

Implications for Intergroup Perception and Behavior

The principal concern of projection research has been the relationship between social projection and predictive accuracy, and little attention has been paid to other consequences. We now consider the implications of differential projection for two biases in intergroup perception and one behavioral puzzle. The two biases are ingroup-favoritism and perceptions of group homogeneity; the behavioral puzzle is the commonly observed high rate of cooperation with fellow ingroup members in social dilemma situations.

[2]The strong effect size obtained for ingroup projection is less ambiguous because the product of outgroup projection and perceived intergroup similarity cannot account for it. For an outgroup projection effect of $r = .13$, the intergroup similarity effect would have to be far larger than 1 to explain an ingroup effect of .46.

Cognitive Consequences

Ingroup-favoritism. The prevalent interpretation of ingroup-favoritism comes from social-identity theory (Tajfel & Turner, 1979) and its offshoot, self-categorization theory (Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). These theories assume that people are motivated to maintain a positive self-concept, and that one way they can do this is by seeing themselves as members of positively valued groups. A favorable view of the self can then evolve from a favorable view of a salient ingroup, at least on dimensions that are important in a given intergroup context (Hewstone, Rubin, & Willis, 2002). The hypothesis of differential projection offers an alternative and broader view. We distinguish between a direct path and an indirect path from differential projection to ingroup-favoritism. The direct path only requires a perception of similarity between the self and the group. When people perceive such a similarity, they express greater liking for the group regardless of the social desirability of the particular group attributes (Clement & Krueger, 1998).

The indirect path takes the desirability of the attributes into account. It starts with the observation that most people have positive self-images (Sedikides & Strube, 1997). From this, it follows that the projection of self-ascribed attributes to ingroups yields positive descriptions of these groups. The expected correlation between the group judgments and judgments of attribute desirability is the product of the positivity of the self-image and social projection (i.e., $r_{\text{INGROUP,DESIRABILITY}} = r_{\text{SELF,DESIRABILITY}} \cdot r_{\text{SELF,INGROUP}}$). By comparison, descriptions of outgroups are less favorable, in part, because these descriptions are less projective (see Otten, 2002, for a review).

Using the meta-analytical effect sizes for ingroup projection and outgroup projection, Figure 3 displays the favorability of ingroup descriptions (solid line) and the favorability of outgroup descriptions (dashed line) as a function of the positivity of the self-image. Ingroup-favoritism is the space between the lines, which widens as the self-image becomes more positive. The model is consistent with the common finding that ingroup-favoritism is mostly a matter of attraction to the ingroup rather than repulsion from the outgroup (Brewer, 1999).

Inasmuch as self-images are more positive when expressed as judgments of personality traits instead of judgments of attitudes, the model also predicts that ingroup-favoritism is greater in judgments of traits (Otten & Wentura, 2001) than in judgments of attitudes (Krueger et al., in press). For the domain of attitudes, Chen and Kenrick (2002) recently reported support for an important implication of this model. Namely, participants were more attracted to members of the ingroup than to members of the outgroup inasmuch as they were more likely to assume that the former rather than the latter held attitudes similar to their own.

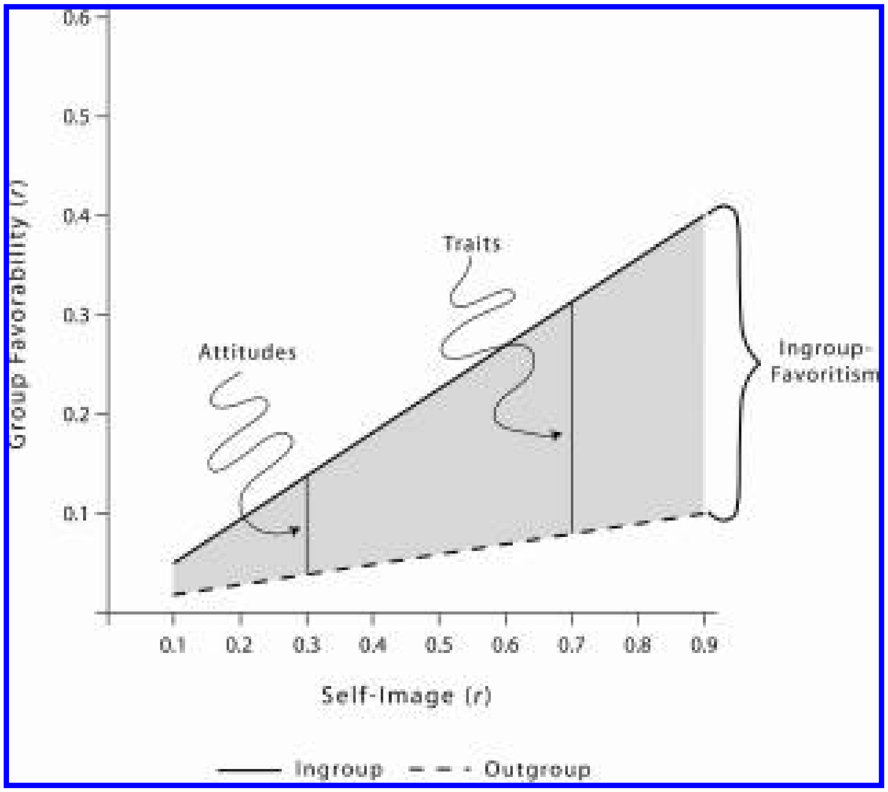

Figure 3. Favorability of ingroup and outgroup descriptions as a function of the favorability of the self-image and social projection.

When, however, the attitudes of other individuals were assured (rather than merely assumed) to be similar to the participants' own, attraction was strong regardless of group membership.

Social categorization is typically more complex than the simple ingroup-outgroup arrangement preferred by investigators. Often, categorization by multiple criteria yields individuals who can be construed both as ingroup members and as outgroup members. In these mixed cases, levels of projection tend to be intermediate (Krueger & Clement, 1996), and, as a result, so are levels of perceived favorability (Migdal et al., 1998). Moreover, social categorization is often mutable. Outgroup members can be re-categorized as ingroup members, and ingroup members can be re-categorized as outgroup members. When such changes occur, new ingroup members gain the benefit of projected favorability (Gaertner et al., 2001), and new outgroup members lose it (Clement & Krueger, 2002).[3] Although this latter effect does not directly cause derogation of excluded subgroups, it may enable it. Many historic examples of genocide began with the gradual re-definition of ingroup members as outgroup members (Friedländer, 1997). Once the benefits of being the target of favorable ingroup projection are withdrawn, active discrimination is easier to justify. It is thus the lack of outgroup projection that may set the stage for the depersonalization of others. In short, differential projection provides a cognitive basis for ingroup-favoritism, and it can explain how patterns of ingroup-favoritism are modified by changes in social categorization. The bias is egocentric rather than ethnocentric in nature, and thus—as the meta-analyses suggest—it should be more pronounced in laboratory than in real groups.

Social projection entails assumptions of similarity, which in turn facilitate perceptions of cohesion, attraction to other group members, and attachment to the group at large. People are more likely to perceive these attributes in ingroups than in outgroups (see Dion, 2000, or Jackson & Smith, 1999, for reviews), and social projection may be a mediating factor. There is, however, a difference: Ingroup-favoritism tends to be stronger in real groups than in laboratory groups (Mullen, Brown, & Smith, 1992), whereas the reverse is true of social projection. Therefore, social projection can only offer a base for ingroup-favoritism in real groups. It is a challenge for future research to understand the factors that increase in in-group favoritism beyond this base.

Outgroup homogeneity. A majority of studies shows that real-world outgroups are perceived as more

[3]According to self-categorization theory (Oakes et al. 1994), the depersonalization of individuals is a function perceived group homogeneity on an important dimension. The egocentrism reflected in social projection suggests the alternative that the self is conceived as the paradigm of "personhood" (Sears, 1983). Others, who are seen as being similar to the self, are thus personalized instead of depersonalized.

homogeneous than ingroups. This phenomenon is typically attributed to cognitive factors such as differences in familiarity (Linville et al., 1989) or differences in the preferred level of categorization (Park & Rothbart, 1982). Self-categorization theory offers an alternative view, according to which ingroups are sometimes seen as more homogeneous than outgroups (Simon & Brown, 1987). People seek comfort in the idea that they are similar to other ingroup members on important dimensions, and thus form a tightly circumscribed unit.

Differential projection to ingroups and outgroups has implications for the baseline of perceived group homogeneity because differences in group-judgments, when expressed as consensus estimates, imply differences in perceived variability. When a trait or a behavioral response is attributed to 0% or to 100% of the group members, variability is nil. When the estimate is 50%, perceived variability (and thus uncertainty with regard to any particular individual) is at its maximum. This simple relationship is captured by the probability that any two randomly selected group members will be the same (i.e., $p^2 = [1 - p^2]$, see Attneave, 1959; or Locke, 2003, for an information-theoretic account).

Projection is a mechanism that increases the extremity of percentage estimates, and thereby reduces perceptions of within-group variability. Percentage estimates can be modeled as $\alpha \cdot$ True + Bias + Error, where "True" is the true percentage being estimated, and *a* is a scaling constant that describes the rate at which estimates increase as true percentages increase (Stevens, 1957). The "Bias" term reflects a perceiver's tendency to over- or underestimate true percentages, and the "Error" term reflects a random variable that is uncorrelated with true scores or biases. Following the theorem of additive variances (McNemar, 1969, p. 142), the variance of the empirical estimates is given by

$$s^2_{EST} = (\alpha \cdot s_{TRUE})^2 + s^2_{BIAS} + 2 \cdot \alpha \cdot s_{TRUE} \cdot s_{BIAS} \cdot r_{TRUE,BIAS} + s^2_{ERROR}.$$

Thus, s_{EST} increases with $r_{TRUE,BIAS}$. In turn, $r_{TRUE,BIAS} = r_{TRUE,SELF} \cdot r_{SELF,BIAS}$ that is, the correlation between the true percentage and a perceiver's judgment bias is large inasmuch as the perceiver's own responses are typical of the group being judged, and inasmuch he or she projects these responses to the group. If the perceiver's own responses are correlated with the aggregated responses of the group, an increase in social projection will increase the variability and thus the extremity of the percentage estimates. As a result, the group will appear to be more homogeneous (see Dawes, McTavish, & Shacklee, 1977; Krueger et al., in press, for empirical demonstrations). It then follows that the processes responsible for perceptions of outgroup homogeneity must be particularly strong to override the countervailing effect of ingroup projection (de la Haye, 2001; Ryan & Judd, 1992). If ingroup projection is stronger in the laboratory than in the outside world, it is not surprising that perceptions of outgroup homogeneity are harder to demonstrate in the former (Judd & Park, 1988).

So far, this review has led us to propose that differential projection facilitates ingroup-favoritism, while inhibiting perceptions of outgroup homogeneity. To complete the survey of the interrelations among these variables, we need to consider the relationship between perceived group homogeneity and ingroup-favoritism. Although Park and Rothbart (1982) originally conceived of ingroup-favoritism and outgroup homogeneity as orthogonal constructs, later research showed a positive association (Maurer, Park, & Rothbart, 1995). Taken together, these findings suggest that there is a direct facilitative path from differential projection to ingroup-favoritism, and an indirect inhibitory path, which is mediated by perceptions of ingroup homogeneity.[4]

Behavioral Consequences

The behavioral expression of ingroup-favoritism is intergroup discrimination. People not only ascribe more favorable attributes to ingroup members than to outgroup members, they also treat them preferentially (Tajfel, Billig, Bundy, & Flament, 1971). Just as perceptual ingroup-favoritism could not be chalked up entirely to the perceivers' irrational biases, behavioral discrimination may, in part, be adaptive. Consider the case of cooperative behavior. Inasmuch as interpersonal behavior is sensitive to the balance of exchanges, cooperative acts depend on the degree to which cooperation was reciprocated in the past and the degree to which such reciprocation can be projected into the future.

In many experimental studies, participants are paired arbitrarily so that specific memories of the past are irrelevant. Participants can, however, anchor expectations of reciprocation on the group status of the interactant. They end up cooperating more with ingroup members than with outgroup members (Gaertner & Insko, 2000) because they—projectively—expect the former more than the latter to make decisions as they themselves do (Yamagishi & Kiyonari, 2000). Expecting reciprocal behavior from ingroup members—and acting on this expectation—is adaptive in that it increases collective welfare. Low projection to

[4]Fiedler, Kemmelmeier, and Freytag (1999) presented a Brunswikian model to show how ingroup-favoritism and perceptions of ingroup homogeneity (in the sense of more extreme percentage estimates on unipolar scales) can arise when available samples of observations are larger for ingroup than for outgroups. Being overall consistent with the Brunswik model, this model only adds the assumption that self-referent data are weighted more heavily than other-referent data.

the outgroup does not seem to be discriminatory as much as it seems to be a missed opportunity.

In large-scale social dilemmas, projection to a collective ingroup yields similar benefits. In a public-goods dilemma, for example, potential contributors need to figure out whether their own efforts or sacrifices matter. Projection suggests that they do. Supporters of public television may donate money, in part, because they believe that their own contributions signal the existence of sufficient support in the population. At the same time, projection raises psychological barriers against the temptation to take a free ride. When projecting, potential free-riders must fear that many others intend to free-ride too, thus putting the availability of the public good at risk.

Voting is a cooperative act that benefits the group if many engage in it. The individual, however, cannot hope that his or her own vote has a discernable impact on the outcome of the election. By taking time away from more rewarding activities, voting only seems to create opportunity costs. Nonetheless, Quattrone and Tversky (1984) found that people express intentions to vote inasmuch as they think their own decisions to vote (vs. abstain) are diagnostic of the decisions of many like-minded others. At the time, choosing to vote on the basis of the diagnostic relationship between one's own decision and the decisions of others appeared to be a "voter's illusion." Recently, Acevedo and Krueger (2004) suggested that projection-based voting is a case of "voter's induction." Whereas it would be irrational to assume that one's own, independently performed, behavior can affect the behavior of the collective, it would also be questionable to ignore one's own perceptions of probable outcomes when deciding what to do (Nozick, 1993).

Projection can motivate individual voters only if it is directed more strongly to "like-minded others" (i.e., ingroup members) than to everyone else in the electorate. In a two-party system, this means that people more strongly project their intentions to supporters of their own party than to supporters of the competing party. If voters were to project to the general electorate, their own intentions to vote could only forecast an overall increase in turnout, but not victory for any one party. Hence, a comparatively low level of projection to the outgroup is critical for the attainment of a collective good involving a conflict of interest between groups. Inasmuch as "thinking is first and last and always for the sake of [. . .] doing" (James, 1890, p. 333), a little irrationality (i.e., violating the strict demands of induction) may be a risk worth taking.

Conclusion

Differential projection to ingroups and outgroups is a robust phenomenon of social perception. The size of the effect is consistently large regardless of variations in the measurement strategy. The context of social categorization is the only reliable moderator variable. Real ingroups elicit somewhat weaker projection than laboratory ingroups, with the opposite being true for outgroups. This pattern is consistent with the idea that projection can be modeled as an egocentric inductive inference in which the self operates at two levels. The first level is that self-referent information enjoys greater cue weight in social prediction than does information obtained from other individuals. The second level is that the self serves as a focal point for social categorization. Whichever categories exclude the self are largely fenced off from the spread of projection.

The simplicity of this model and the robustness of its effects make it a credible baseline against which other prominent phenomena of intergroup perception and behavior may be judged. Our analysis suggests that differential projection is sufficient to produce attribute-based ingroup-favoritism and perceptions of group homogeneity. Cooperative behavior with other individuals within one's own group can be understood as a behavioral extension projection-based ingroup-favoritism. In short, research on social projection can move beyond the phenomenal analysis of what it is and how it is caused to questions of how social projection facilitates (or impedes) effective functioning in a complex world.

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