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Social Categorization and the Truly False Consensus Effect

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The false consensus effect involves adequate inductive reasoning and egocentric biases. To detect truly false consensus effects (TFCEs), we correlated item endorsements with the differences between estimated and actual consensus within Ss. In Experiment 1, Ss overgeneralized from themselves to gender in-groups and to the overall population, but not to gender out-groups. Experiments 2 and 3 demonstrated intuitive understanding of consensus bias. Another person's choices were inferred from that person's population estimates or estimates about the gender in-group. In Experiment 4, Ss inferred that consensus estimates for a behavior were higher among people who were willing to engage in that behavior than among those who were not. Implications of these findings for general induction, social categorization, and the psychological processes underlying TFCEs are discussed.

> When, for example, Freud . . . got hold of a simple but significant fact he would feel, and *know* [italics added], that it was an example of something general or universal . . . that is the way the mind of a genius works. (Jones, 1953, p. 66)

> [Projection is] not essentially different from the tendency to assume naively that others feel or react in the same manner as we ourselves do. (Horney, 1939, p. 26)

Generalizing from oneself to others may be the hallmark of genius or just common sense. Psychoanalysts as well as social psychologists, however, have pointed out that projection is often irrational or defensive (reviewed by Holmes, 1968, 1978). Ross, Greene, and House (1977) concluded that "laymen tend to perceive 'false consensus'—to see their own behavioral choices and judgments as relatively common and appropriate to existing circumstances while viewing alternative responses as uncommon, deviant, or inappropriate" (p. 280). Considerable research efforts have been dedicated to the discovery of the mechanisms underlying the perception of false consensus. Reviewers have concluded that both cognitive (e.g., selective attention and memory) and motivational (e.g., protection and enhancement of the ego) processes contribute to this phenomenon (Marks & Miller, 1987; Mullen et al., 1985).

The standard measurement of false consensus involves within-items, between-subjects comparisons of consensus estimates. Estimates are considered biased when people who endorse an attitude item or make a certain behavioral choice provide higher consensus estimates than people who do not endorse the item or choose an alternative behavior. On purely statistical grounds, however, some projection is justified and in fact required (Dawes, 1989; Hoch, 1987). In order to determine whether estimates of social consensus are indeed egocentrically biased, a comparison between the magnitude of observed and appropriate projection is needed. We introduce a measure that discriminates between statistically appropriate consensus effects and true bias. *Truly false consensus effects* (TFCEs) can be detected within subjects by correlating item endorsements with the differences between estimated and actual consensus. Conceptually, this within-subjects measure is more conservative than the conventional between-subjects approach. We tested the hypothesis that most people indeed show TFCEs.

In addition to the question of the measurement of TFCEs, we examined the idea that social categorization constrains the spread of projection. People tend to perceive more consensus for their attributes in groups they belong to than in groups they do not belong to (Mullen, Dovidio, Johnson, & Cooper, 1992; Spears & Manstead, 1990). We examined this idea using the within-subjects measure of TFCEs and by using gender as a categorical variable. To our knowledge, no study has investigated social projection for in-groups and out-groups in conjunction with projection to the overall population consisting of these same two groups. We developed and tested the hypothesis that there are greater TFCEs in judgments about in-groups than in judgments about out-groups. Additionally, there are as many TFCEs in judgments about the overall population as in judgments about in-groups.

The second main objective of this research was to examine the role of consensus bias in person perception. We suggest that people intuitively understand TFCEs and therefore infer other people's characteristics from their consensus estimates. Such intuitive understanding of bias is predicted (a) on the grounds of generic principles of induction and (b) because other judgmental biases are also recognized (e.g., Dawes, Singer, & Lemons, 1972). Perhaps the most compelling reason for testing this hypothesis is its plausibility. Consider the following thought experiment: Suppose one encounters two men, one who believes that 70% of all people favor capital punishment and one who believes that only 30% do so. Which one of the two favors the death penalty?

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Measuring TFCEs Within Subjects

Separating Adequate From False Projection

When estimating social consensus in an uncertain environment, people should project from themselves to others. By definition, people are more likely to endorse items that have high rather than low actual consensus (Hoch, 1987). Therefore, the average person's choices are correlated with actual consensus across items, r(act, end) > 0. To minimize bias, consensus estimates should be correlated with endorsements across items as well, r(est, end) > 0. In a theoretical article, Dawes (1989) argued that consensus estimates are a special case of induction. The social perceiver's own position on an item is an inescapable piece of information that should not be ignored. On the basis of normative statistical principles alone, endorsers of an item should give higher consensus estimates than nonendorsers. Formally speaking, when generalizations are made from instances, Bayes's theorem requires that prior probabilities be revised by diagnostic observations no matter how small the sample.[1] To detect TFCEs, one needs to ask whether the amount of projection exceeds the appropriate measure. We wanted to know whether *relative to actual consensus* people would estimate consensus to be higher for endorsed items than for nonendorsed items. The within-subjects correlations between the difference score (estimated minus actual consensus) and the rater's own endorsements address this question.

Individual Differences

The within-subjects measure permits discrimination between those who project too much, too little, or the right amount. Suppose raters endorse the first two of four items whose actual consensus is 70%, 70%, 30%, and 30%. Raters will experience uncertainty if the items refer to events that they have little knowledge about other than their own positions. If they decide to project from their own positions, they may capture actual consensus. In this case, r(diff, end) = 0 because the difference scores (estimated minus actual consensus) are zero. Alternatively, TFCEs would be indicated if estimates were insufficiently regressive (e.g., 80%, 80%, 20%, and 20%), r(diff, end) = 1.0. Other raters may be wholly inaccurate, but their errors may be unrelated to their own positions. Finally, suppose raters who endorse majority positions (Items 1 and 2) ignore their own endorsements. Estimating 50% consensus on each item is a perfectly regressive strategy. In that case, Items 1 and 2 are underestimated by 20% and Items 3 and 4 are overestimated by 20%. These difference scores are negatively correlated with endorsements, r(diff, end) = −1.0, thus showing a false uniqueness effect (FUE).

Within-Subjects Versus Between-Subjects Measurement

The within-subjects correlational measure is more conservative in detecting bias than are between-groups comparisons. Table 1 shows two simple patterns containing actual and estimated consensus, the difference scores, and hypothetical endorsements (1 = agree and −1 = disagree). In a meta-analysis of 134 between-groups tests of the false consensus hypothesis,

Table 1
Hypothetical Data Patterns Showing Possible Relationships Between Estimated Consensus, Actual Consensus, and Endorsements

Item	Actual consensus	Estimated consensus	Difference	Endorsement
		r(diff, end) = −1.0		
1	80	75	−05	1
2	60	55	−05	1
3	40	45	05	−1
4	20	25	05	−1
		r(diff, end) = 1.0		
1	80	70	−10	−1
2	60	50	−10	−1
3	40	55	15	1
4	20	15	15	1

Note. Diff, end = one's own endorsements vs. difference scores.

Mullen and Hu (1988) identified these two patterns as being the most typical. In this example, endorsers' consensus estimates on a particular item are consistently higher than the estimates made by nonendorsers. This between-subjects false consensus effect does not necessarily translate into a within-subjects TFCE. Raters following Pattern 1 show an FUE, r(diff, end) = −1.0; raters following Pattern 2 show a TFCE, r(diff, end) = 1.0.

The differences between estimated and actual consensus can be used to assess inaccuracies at the group level. To do this, estimates are first averaged across subjects and actual consensus is then subtracted. The typical finding is that the averages of the estimates provided by those in the majority underestimate their own consensus (Pattern 1) and that the averages of the estimates provided by those in the minority overestimate their own consensus to an even greater degree (Pattern 2; Mullen & Hu, 1988). To the extent that the group means accurately reflect the average pattern of inaccuracy within individuals, one should expect that the majority of subjects displays FUE, r(diff, end) < 0.0.

It is possible, however, that between-groups and within-subjects analyses yield different results. When between-groups analyses are performed, individual differences within a group constitute the error variance. Data are first aggregated across subjects and within groups to obtain the power to test between-groups differences in the mean. By contrast, within-subjects correlations present a measure of effect size for the individual.

[1] To illustrate, consider Bayesian induction from the color of individual chips to the prevalence of this color in urns. Suppose there are two urns, A and B, of equal size. Their contents have been assembled randomly and independently of each other. In both urns, blue and red chips have uniform priors. Then, p(blue) = .5 for each urn. When a single chip is drawn from urn A and it is blue, the posterior is $(k + 1)/(n + 2)$, where k is the number of successes (blue) and n is the sample size. Thus, p(blue/A) = .67; p(blue/B) remains .5. Consequently, if the target population is defined as the chips in all urns, p(blue) = $1/2*(p[\text{blue/A}] + p[\text{blue/B}]) = .58$.

This measure is unrelated to sample size. The average of the correlations is the average effect size, which can be tested against zero for significance.

In sum, false consensus effects have been attributed to psychological processes operating in the individual. The within-subjects correlation between the difference score (estimated minus actual consensus) presents a cautious measure of whether the average individual (rather than the average of the individuals) is truly biased.

Social Categorization Constrains Projection

In most tests of the false consensus hypothesis, subjects estimate consensus in groups to which they belong. Differences in generality of the target population have little effect on consensus effects (Mullen et al., 1985). However, social categorization may constrain the spread of projection depending on whether the target group includes the rater (Ryan & Judd, 1993). Estimates about consensus among in-group members typically show strong false consensus effects. Such effects are either weak, absent, or reversed in estimates about out-group members. This discrepancy has been interpreted in terms of in-group favoritism (Granberg, 1984; Sherman, Chassin, Presson, & Agostinelli, 1984) or intergroup differentiation (Krueger, 1992).

Spears and Manstead (1990), using gender and status as a student as a means of social categorization, found slightly stronger false consensus effects for in-groups than for out-groups on judgments of preference (watching the Wimbledon tennis tournament on TV). They suggested that people achieve a positive social identity through discriminations between in-groups and out-groups. When Mullen et al. (1992) varied the salience of the in-group–out-group distinction across experiments, false consensus effects were consistently obtained for in-group judgments. Moreover, with increasing salience of social categorization, consensus estimates for out-groups became biased toward false uniqueness. Mullen et al. (1992) concluded that one way of achieving intergroup differentiation is to perceive in-group members as being similar to the self (assimilation) and out-group members as different (contrast).

Another approach to in-group–out-group differences in consensus bias is given by Dawes's (1989) Bayesian analysis of social induction. According to this view, people project inasmuch as they show a correlation between "endorsements of a behavior or attitude item and their estimates of the endorsement frequency in *a specified group of which they are a member*" (Dawes, 1989, p. 1 [italics added]). By definition, no one is a member of an out-group. Hence, there should be no projection to groups whose boundaries exclude the self. In statistical terms, estimates about out-groups should be prior probabilities, not posterior probabilities. Therefore, the first hypothesis of Experiment 1 was that subjects show greater simple projection (i.e., $r[est, end] > 0$) for in-group than for out-group judgments. That is, subjects were expected to follow the rules of induction adequately. The second hypothesis was that subjects show additional TFCEs in in-group estimates but not in out-group estimates. That is, they were expected to assimilate perceptions of in-group members to the self to a greater degree than would be warranted by simple induction.

To our knowledge, the question of how consensus estimates for in-groups and out-groups are related to estimates for the population consisting of the total of the two groups has not been addressed in a single study. Across studies, consensus effects are fairly insensitive to the generality of the target population (Mullen et al., 1985). The question is, Should they be? On the one hand, proper induction demands that a single piece of information affects estimates of base-rate probabilities, no matter how large the population (Dawes, 1989). On the other hand, consider a population consisting of two equally large groups (e.g., men and women). If people perceive TFCEs in gender in-groups but not in gender out-groups, the magnitude of TFCEs for adults in general should be intermediate. Our third hypothesis was that people project on the basis of group membership alone. That is, TFCEs in consensus estimates for adults in general will be as large as for gender in-groups.

Experiment 1

Eighteen items and their endorsement frequencies (actual consensus) were selected from the Minnesota Multiphasic Personality Inventory (MMPI; Butcher, Dahlstrom, Graham, Tellegen, & Kaemmer, 1989).[2] Six items were sex typed as masculine, 6 as feminine, and 6 were not sex typed. Subjects estimated consensus in the adult population as well as for one of the sexes. They then indicated their own item endorsements. Ratings were made in this order to minimize the salience of one's own position at the time when population estimates were made. It was expected that between-subjects and within-subjects consensus effects would appear only when the sex of the target population was unspecified or when estimates referred to people of the same sex. TFCEs, as measured by r(diff, end), was expected to be significant but smaller than simple projection, r(est, end). The sex typedness of the items was varied to explore the possibility that the strength of intergroup differences in consensus bias depends on the diagnosticity of the item for men and women.

Method

Subjects and Procedure

Forty-six male and 54 female undergraduate subjects were recruited on the Brown University campus. Subjects were approached individually at various locations (e.g., cafeterias, snack bars, dormitories) and asked to complete a questionnaire. They were not paid for participation. Participants were assured that the questionnaire was part of an experiment in social perception and did not constitute a test of personality. Subjects took approximately 15 min to fill out the questionnaire and were then thanked and debriefed. They received a sheet with the actual consensus on each item as feedback on their accuracy.

[2] Minnesota Multiphasic Personality Inventory population norms are based on representative samples of 1,462 women and 1,138 men. We thank Robyn Dawes for suggesting this procedure of securing data on actual consensus.

Materials and Design

Items are listed in Table 2. Care was taken to select statements that did not suggest personality pathology. One third of the items were sex typed as masculine, with actual consensus for men being at least 10 points higher than actual consensus for women (see Table 2). Another third was sex typed as feminine, and the remaining 6 were neutral, with sex differences in actual consensus of less than 10 points. The *relative* differences in actual consensus identified items as diagnostic of gender independent of the absolute level of consensus.

This selection of items ensured some variability in actual consensus, and statements with extreme actual consensus (higher than 80% or lower than 20%) were not chosen in order to not preclude over- or underestimation. Half of the subjects estimated consensus for adults in the United States first, followed by estimates for either men or women. The other half made these ratings in reverse order. Sex of subject and sex of target group were between-subjects variables, creating two types of in-group ratings (i.e., men estimating the responses of men and women estimating the responses of women) and two types of out-group ratings (i.e., men estimating the responses of women and women estimating the responses of men). The level of social categorization (i.e., estimates about adults in general vs. estimates about gender groups) and the sex typedness of the items (masculine, feminine, and neutral) were the within-subjects variables.

Results

In this and the following experiments, data were analyzed in two ways. Within-subjects correlational analyses are reported first. Correlations involving subjects' item endorsements (yes or no) were point-biserial. All correlations were transformed to Z scores (McNemar, 1962) and submitted to analyses of variance (ANOVAs). The reported means are the correlation coefficients corresponding to the means of the Z scores. Unless noted otherwise, we rejected the null hypotheses only when alpha was smaller than .01. Between-subjects analyses involved conventional single-item as well as multiple-item comparisons of average estimates.

Estimates About the General Population

Within-subjects analyses. For each subject, three correlations were computed and transformed to Z scores. First, the significant positive correlation between the difference scores (estimated minus actual consensus) and endorsements indicated the predicted TFCE, mean r(diff, end) = .26. That is, relative to actual consensus, subjects gave higher estimates when they agreed with a statement than when they disagreed. Second, the mean correlation between consensus estimates and endorsements showed the size of simple projection, r(est, end) = .34. This correlation was significantly higher than r(diff, end), $t(99)$ = 4.34. Third, there was no correlation between estimated and actual consensus, r(est, act) = −.07.

The robustness of the TFCE was supported by the finding that r(diff, end) was positive for 86% of the subjects, which was unlikely to be a chance event ($p < .000001$ by sign test). The average variability of estimates (mean SD = 21.50) was twice as large as the variability in actual consensus (mean SD = 10.60).

Table 2
Experiment 1: MMPI Statements, Consensus Estimates Made by Endorsers and Nonendorsers, and Actual Consensus

MMPI statement	Non-endorsers	Endorsers	Actual consensus
1. I enjoy a race or game more when I bet on it.	52.35	>45.48	30
2. When I get bored, I like to stir up excitement.	58.05	>43.06	43
3. At movies, restaurants, or sporting events, I hate to have to stand in line.	79.12	76.43	55
4. I enjoy reading love stories.	48.19	>40.02	47
5. I have difficulty starting to do things.	59.96	54.64	34
6. I like poetry.	39.44	35.40	62
7. I am neither gaining nor losing weight.	36.78	33.35	65
8. I like to read about science.	32.02	29.20	68
9. I usually feel better after a good cry.	48.61	>33.94	49
10. My feelings are not easily hurt.	45.85	41.18	43
11. I like to talk about sex.	64.88	>50.74	48
12. I would like to wear expensive clothes.	69.70	>60.13	69
13. I like to flirt.	64.83	>51.61	57
14. My eyesight is as good as it has been.	50.97	>37.75	57
15. I used to keep a diary.	48.00	38.02	52
16. I love to go to dances.	56.00	>43.35	49
17. I have no fear of spiders.	51.80	>38.89	53
18. I think nearly everyone would tell a lie to keep out of trouble.	68.49	>56.12	52
M	54.17	44.96	

Note. Significant differences between estimates of endorsers and nonendorsers are indicated by ">" for $p < .01$. MMPI = Minnesota Multiphasic Personality Inventory.

Thus, the TFCE resulted from positive correlations between endorsements and estimates in combination with an overestimation of variability between statements.

Between-subjects analyses. Following the traditional mode of data analysis, we computed the averages of estimates for each item separately for endorsers and nonendorsers. Table 2 shows the results and the actual consensus. For all statements, the mean estimates given by endorsers were higher than the mean estimates given by nonendorsers. Separate 2 (sex of subject) × 2 (yes or no endorsement), between-subjects ANOVAs were performed for each item. In 11 of these analyses, the effect of endorsement was significant. The sex of the subject did not interact with the consensus bias in any of these tests.

Next, we used this data set to examine Mullen and Hu's (1988) conclusion that the average estimates of people holding a majority position fall below their actual consensus and that the average estimates of people holding a minority position exceed their actual consensus to an even greater degree. To illustrate, consider Item 1. Endorsers held a minority position (actual consensus was 30%). They overestimated consensus: 52.35 − 30 = 22.35. Nonendorsers, who held the majority position, underestimated their consensus (i.e., nonendorsement): (100 − 45.48) − (100 − 30) = −15.48. Ten of the 18 items showed this pattern (stems 1, 2, 5, 6, 7, 8, 11, 13, 15, and 17) for endorsers and nonendorsers, and another five showed it for one group (Items 4, 10, 12, 13, and 16). It is important to note that although these between-subjects results are largely consistent with previous research, they are not highly informative about the average within-subjects results. Indeed, these data show that group averages on majority positions may involve underestimation of consensus while at the same time the majority of individuals (86%) show true consensus bias.

Estimates About In-Groups and Out-Groups

Within-subjects analyses. As predicted, the TFCE was significant for in-group estimates but not for out-group estimates. For 90% percent of the male ($p < .001$) and 85% percent of the female ($p < .001$) subjects, the in-group-related correlations between the difference score and one's own endorsements were positive. The proportion of positive out-group-related correlations for male (68%) or female (44%) subjects did not differ from chance. Table 3 shows that the average size of r(diff, end) was larger for same-sex groups than for different-sex groups. In an ANOVA of the Z scores, the relevant interaction between sex of subject and sex of target group was significant, $F(1, 96) = 27.35$. The means of the out-group-related coefficients were not different from zero ($ps > .05$). Correlations between estimates and one's own endorsements, r(est, end), showed the same interaction, $F(1, 96) = 31.85$. Again, it should be recalled that these correlations indicate simple projection and do not constitute a true consensus bias.

Next, within-subjects correlations were computed between estimates and the actual sex-specific consensus. In contrast to the case of population estimates, which were found to be unrelated to actual consensus, the average group-specific correlation was significant ($r = .38$) and did not vary as a function of the sex of the subjects or the in-group–out-group status of the

Table 3
In-Group and Out-Group Data in Experiment 1: Mean Personal Correlations Between One's Own Endorsements and Difference Scores (Diff, End; Estimated Minus Actual Consensus) and Between One's Own Endorsements and Estimated Consensus (Est, End)

	r(diff, end), truly false consensus		r(est, end), simple projection	
Target group	Men	Women	Men	Women
Sex of subject				
Male	.36	.12	.47	−.09
Female	.02	.33	.02	.45

rated sex (all $Fs < 1$). The finding that TFCEs emerged despite the partial sensitivity to actual consensus underscores their importance. Moreover, because awareness of actual consensus was the same for in-groups and out-groups, differences in the strength of the consensus bias could not be reduced to differences in the knowledge of actual consensus.

The variability of estimates for men and women (mean SD = 26.49) exceeded the variability in actual consensus (mean SD = 15.99) and did not vary as a function of the in-group or out-group status of the judgment (all $ps > .09$). In other words, the differences in the consensus effects between in-groups and out-groups were a consequence of differences in the correlations between estimates and endorsements. Differences in the over- or underestimation of true variability, which theoretically might have contributed to in-group–out-group differences, did not play a role.

Between-subjects analyses. No item-by-item analyses were conducted on the group-related estimates because the breakdown into two types of in-group judgments and two types of out-group judgments yielded several small numbers of endorsers or nonendorsers. When averaged across items, however, the differences between the unweighted means of the estimates showed that there were strong between-subjects consensus effects for in-group judgments (men, $M = 12.43$; women, $M = 10.96$) and somewhat weaker effects for out-group judgments (men rating women, $M = 6.75$; women rating men, $M = 5.29$).

In order to perform tests of mean differences, data were aggregated as follows: For each subject, estimates were averaged separately for endorsed items and nonendorsed items. This was done separately for the masculine, the feminine, and the gender-neutral items, resulting in a 2 (sex of subject) × 2 (sex of target group) × 2 (yes or no endorsement) × 3 (masculine, feminine, or neutral item type) design with within-subjects measures on the last two variables. The significant three-way interaction between sex of subject, sex of target group, and endorsement indicated the predicted in-group–out-group difference in consensus, $F(1, 70) = 5.16$, $p < .03$. This effect was not qualified by the sex type of the items. When out-group-related estimates were analyzed alone, men showed consensus bias in their judgments about women, $t(23) = 2.74$, $p < .02$; however, women showed no such bias in judging men, $t(26) = 0.72$.

Population Versus Group Estimates

The within-subjects analyses had shown large TFCEs for estimates about in-groups ($r = .36$) and no TFCEs for estimates about out-groups ($r = .07$). On the basis of this difference in group-related judgments, one should expect the effect size for estimates about the entire population to be intermediate. The data just presented, however, indicated that the population-related TFCE ($r = .26$) was similar to the effects related to in-groups. Subjects seemed to rely on their estimates about in-groups and ignored estimates about out-groups when estimating consensus in the overall population.

To test the hypothesis that people overprojected on the basis of group membership alone, the Z-scored correlations indicating the TFCE in population estimates and in-group estimates were entered into a joint multivariate ANOVA. Sex of subject and sex of target group were the between-subjects variable, and generality of target group was the within-subjects variable. The expected three-way interaction was significant, $F(1, 96) = 13.65$. Follow-up paired t tests showed that when subjects made estimates about in-groups, the size of the TFCE did not vary with the generality of the target group. Men overprojected to the population ($r = .30$) as much as they overprojected to men ($r = .37$), $t(20) = 1.19$, $p > .20$; women overprojected to the population ($r = .32$) as much as they overprojected to other women ($r = .36$), $t(26) = 0.40$. By contrast, the TFCE varied with the generality of the target group among subjects who made estimates for out-groups. Men overprojected more to the population ($r = .24$) than to women ($r = .12$), $t(24) = -1.93$, $p < .07$, and women overprojected to the population ($r = .23$) but not to men ($r = .03$), $t(26) = -5.11$.

Mean-Level Accuracy

Correlations involving actual consensus addressed correlational accuracy. To complement the preceding analyses, average absolute differences between estimated and actual consensus were computed for each subject. Average mean-level differences were the same when population estimates were involved ($M = 20.78$) and when group estimates were involved ($M = 21.00$). A 2 (sex of subject) $\times$ 2 (sex of target group) between-subjects ANOVA was performed on the differences involving group estimates. Only the effect of sex of target group was significant, $F(1, 1795) = 7.31$. Estimates about women came closer to true actual consensus ($M = 20.10$) than did the estimates about men ($M = 21.95$). Interestingly, there were no in-group–out-group differences in mean-level accuracy, $F(1, 1795) = 1.27$. This result illustrates the independence of mean-level and correlational measures of accuracy.

Discussion

There was support for the three hypotheses. First, truly false within-subjects consensus effects showed that relative to actual consensus, estimates were biased toward the raters' own responses. In addition, traditional between-groups comparisons showed that consistent with previous research, endorsers gave higher estimates than nonendorsers. Also, consistent with Mullen and Hu's (1988) review, the average estimate of subjects holding a majority position underestimated the actual consensus with their position, and the average estimate of subjects holding a minority position overestimated the actual consensus with their position. Most important, it was shown that the average of the within-subjects measures was not predictable from group-based analyses.

Second, both simple projection and TFCEs were strong for judgments about in-groups and the general population and absent for judgments about out-groups. That is, by not projecting to groups they did not belong to, people recognized that social categorization constrains induction. Similarly, the effects of simple projection suggest that people are sensitive to the rules of induction. However, the significant TFCE for in-group estimates, however, indicates that this projection is insufficiently regressive.

Third, estimates of consensus in groups varying in inclusiveness were made on the basis of membership alone. Estimates targeted at the overall population showed as much consensus bias as in-group estimates. Because the overall population was made up in equal parts of the in-group and the out-group, the magnitude of the consensus effects should have fallen between the in-group and the out-group effect. Possibly, subjects inferred social consensus in a two-stage process, involving both egocentrism and ethnocentrism. In the first stage, egocentrism, they estimated their in-group's characteristics but not their out-group's characteristics from their own personal characteristics. In the second stage, ethnocentrism (or, rather, gender centrism), they estimated the characteristics of people in general exclusively from what they believed to be true about their in-group. This process, however, violates the rules of induction.

In sum, the results of Experiment 1 showed that social projection involves appropriate as well as erroneous reasoning. When comprehensive information about group characteristics is missing, it is appropriate to use one's own characteristics as cues for the estimation of consensus in groups to which one belongs. Nonregressive estimates, however, lead to true bias. Previous researchers on the mechanisms underlying the false consensus effect have not investigated whether people understand or think they understand their own estimation process. Possibly, the reason for this was that all consensus effects were considered false. As a consequence, it seemed unnecessary to assess people's awareness of bias. If they were aware of it, why would they continue to project? Thus, regardless of the type of mechanism that was postulated to explain consensus bias, a shared implicit assumption of previous work may have been that people do not know that their estimates are swayed by their own position.

By contrast, the view that some degree of projection is justified permits the hypothesis that people know what they are doing. It is conceivable that people understand the universality of the inductive process. They may rightly assume that others project from themselves when estimating consensus in a group. Suppose a person estimates the popularity of TV programs he or she likes to be higher than the popularity of programs he or she dislikes. Knowing that simple projection is adequate in this situation, this person may assume that everyone's estimates are influenced by his or her own positions. Hence, someone else's estimates of program popularity reveal what that person likes and dislikes.

There are examples of the intuitive understanding of judgmental biases in the literature. Dawes et al. (1972) found contrast effects in attitudinal judgment. Self-proclaimed hawks and doves attempted to write statements about the United States' involvement in Vietnam that they thought were acceptable to the typical hawk or dove. However, hawks rejected hawkish statements written by doves as being too hawkish, and doves rejected statements written by hawks as being too dovish. In a subsequent experiment, hawks and doves were presented with a sample of the statements generated in the first study. Surprisingly, both hawks and doves recognized that the more extreme statements had been written by members of the opposing group. Similarly, in-group favoritism, or evaluative ethnocentrism, is the well-documented and pervasive tendency to describe in-groups in more favorable terms than out-groups. Vivian and Berkowitz (1992) showed that subjects not only discriminated against out-groups but that they also realized that out-groups would discriminate against them. In this case, the recognition of bias in others was limited by subjects' expectation that impartial observers would side with their group rather than with the out-group.

The second experiment was designed to test the idea that people intuitively recognize the role of consensus effects in the judgments made by others. We hypothesized that another person's position on an item would be inferred from that person's consensus estimates. High estimates are understood to indicate agreement; low estimates are understood to indicate disagreement. In other words, because people grasp the fundamentals of inductive reasoning, they understand that within-persons correlations between item endorsements and consensus estimates tend to be positive, $r(\text{est, end} > 0)$. Because in Experiment 1 there was a TFCE and simple projection, however, the question arises as to whether people are also sensitive to the extent that consensus estimates made by others deviate from actual consensus. Suppose Mr. Kool estimates the percentage of smokers to be 40% and the actual consensus is known to be 30%. On the basis of a simple rule of induction, it would be inferred that Mr. Kool does not smoke (his estimate was below 50%). If, however, people are sensitive to the error in estimates relative to actual consensus, Mr. Kool should be identified as a smoker.

Experiment 2

Experiment 2 was conducted to test two hypotheses. The first hypothesis was that people infer a person's item endorsements from that person's population estimates. According to a simple-heuristic version of this argument, people infer others' endorsements from high estimates and nonendorsements from low estimates. It is possible, however, that people are sensitive to differences between estimates and actual consensus. If they are, they will infer agreement with an item when consensus estimates exceed actual consensus, and they will infer disagreement when estimates fall below actual consensus. The second hypothesis was that people are sensitive to the mediating role of social categorization. If they are, they will infer agreement with an item if the target person had made estimates about the gender in-group, but not when he or she had made estimates about the gender out-group.

Method

Subjects and Procedure

Recruitment procedures were similar to those used in Experiment 1. Thirty-six male and 56 female undergraduate subjects volunteered without receiving monetary compensation. Subjects took about 15 min to complete the questionnaire and were then thanked and debriefed. The questionnaire consisted of four sets of inference tasks, followed by their own endorsements.

Materials and Design

The questionnaire listed the same 18 MMPI statements that were used in Experiment 1. Subjects were told that data of a previous experiment were used for this judgment task (true) and that each list of responses had been taken from one individual subject and retyped on a new sheet to ensure anonymity (not true). For each of the four sets of inferences and each item, the estimates of another (fictitious) subject were provided. Subjects entered a 1 when they thought the anonymous estimator had agreed with the statement and a 0 when they thought the estimator had disagreed. Two of the lists contained in-group estimates (i.e., a man estimating agreement among men or a woman estimating agreement among women). The other two contained out-group estimates (i.e., a man estimating agreement among women or a woman estimating agreement among men). The order of presentation of these lists was randomized across subjects. Half of the subjects received estimates only, and half received the actual and estimated consensus for the target group.

The provided estimates were not taken from real individual subjects in Experiment 1 and they were not made up. Instead, for each item in each of the four lists, we checked whether the majority of subjects in Experiment 1 had agreed or disagreed. The average estimate of that majority was entered as the fictitious subject's estimate. Thus, it was ensured that the provided responses represented the outcome of Experiment 1 fairly. Hence, unknown to these subjects, they did not infer the endorsements of a specific person but the actual consensus of the sample of students in Experiment 1.

Results and Discussion

Within-Subjects Analyses

Point-biserial correlations between the difference scores (estimated minus actual consensus) and inferred endorsements were computed and transformed to Z scores. Table 4 shows a summary of the results. Z scores corresponding to the correlations given in the table were submitted to an ANOVA, with sex of subject, condition (actual consensus given or withheld), sex of estimator, and sex of target group as fully crossed variables. The last two variables were within subjects. Again, unless noted otherwise, we rejected the null hypotheses only when alpha was smaller than .01.

As predicted, inferences were significantly related to the differences between estimated and actual consensus when judgments about in-groups had been made. This was not true when the target persons had made estimates about out-groups. The relevant interaction between sex of estimator and sex of target

Table 4
In-Group and Out-Group Data in Experiment 2: Mean Within-Subjects Correlations Between Inferred Endorsements and Difference Scores (Target Person's Consensus Estimates Minus Actual Consensus)

	Actual consensus			
	Not provided		Provided	
Target group	Men	Women	Men	Women
Sex of estimator				
Male	.48	.15	.58	.24
Female	−.02	.45	.17	.53

group was significant, $F(1, 88) = 98.09$. This interaction was also significant for correlations between inferences and estimates alone or inferences and actual consensus, $Fs(1, 88) = 185.82$ and 97.29, respectively. Sensitivity to actual consensus increased when it was made explicit, $F(1, 88) = 8.27$. The presence of actual consensus elevated the correlations between the difference scores and inferred agreement, irrespective of the in-group–out-group status of the provided predictions. Thus, when actual consensus was given, there was a tendency to infer consensus bias even when out-group judgments had been made. Interestingly, subjects' own item endorsements did not contribute to inferences about others and were unrelated to social categorization. Surprisingly, there was a tendency to infer others' endorsements to be contrary to one's own ($r = -.20$).

Group-Based Analyses

For each statement we calculated the percentage of subjects who inferred that the fictitious subject had agreed. These percentages were correlated across items with the difference between the provided estimate and actual consensus. The percentage of "agree" inferences correlated highly with estimates about in-groups ($r = .94$). This correlation was not affected by sex of the target group or by the sex of the subjects. Estimates about out-groups were negatively related to inferences. This was true when a man had made estimates about women (male subjects, −.32; female subjects, −.37) or when a woman had made estimates about men (male subjects, −.35; female subjects, −.08). Indeed, subjects seemed to react against the notion of perceived consensus, assuming that a person who had given a high estimates about an out-group was less likely to endorse the item for himself or herself.

The results of this experiment present a mirror image of the findings of Experiment 1. Not only did people commit a truly false consensus bias, they also detected such bias in others. They used another person's population estimates to infer that person's responses to questionnaire items and, in doing that, they were sensitive to the difference between the target person's estimate and the actual consensus. As in Experiment 1, estimates about out-groups had little effect on consensus effects. Taken together, sensitivity to actual consensus and to social categorization suggest that a simple-heuristic view of the inference process in insufficient. People's intuitive understanding of the TFCE goes beyond merely equating high estimates with agreement and low estimates with disagreement. Yet, the correlation between estimates and endorsements, r(est, end) is a critical component of the inference process. In Experiment 2, this correlation was computed across statements. The question remains whether variations in the estimate on a single item entail variations in subjects' willingness to infer that the estimator had agreed with the item. Experiment 3 was designed to answer this question.

Experiment 3

In this experiment, different percentage estimates were provided for each item. This way, we could test the idea that variations in the estimates are sufficient to elicit different inferences more rigorously. We hypothesized that the closer a provided consensus estimate would be to the endpoints of the percentage scale, the more subjects would infer that the target person disagreed or agreed (respectively) with the item.

Method

Twenty-three male and 24 female Brown University undergraduates participated as volunteers. Procedures were similar to those in Experiment 2. Differences in the materials involved reducing the list to 12 statements and to pair each statement with one of two estimates. In contrast to Experiment 2, the provided numbers reflected estimates about the entire adult population rather than sex groups. Six of the statements (Items 2, 3, 5, 7, 8, and 12 in Table 5) were paired with either the estimates derived from Experiment 1 or with their inverse (1 − estimate). The other statements were paired with extreme estimates (above 80% or below 20%) in one condition and with their inverse in the other. If estimates guide inferences for each item, differences in inferences should be larger for extreme estimates than for estimates close to 50%. Similarly, confidence in inferences should increase with greater extremity of estimates. To test the latter hypothesis, confidence ratings were collected on a 5-point scale for each inference. Confirmatory results in this experiment would indicate that subjects recognized and used consensus bias in population predictions the same way they used in-group-related predictions (Experiment 2).

Results and Discussion

The hypothesis that variations in estimates would cause similar variations in inferred agreement was confirmed for all but Item 5. With item contents held constant, high estimates led significantly more subjects to conclude that the estimator had agreed with the item than did low estimates. Table 5 shows the results (based on chi-square tests).

Across items, the percentage of subjects inferring agreement with the statements was correlated almost perfectly with the provided estimates, regardless of whether half of these estimates consisted of actual data drawn from Experiment 1 ($r = .93$) or whether they were the inverse of these data ($r = .96$). The more extreme the estimates were, the more subjects concurred in their corresponding inferences. Contrary to expectations, however, the extremity of estimates and the likelihood of a certain inference were unrelated to the confidence in the inference (all $rs < .10$).

Table 5
Experiment 3: Percentage of Subjects Inferring Agreement With Item for Another Person

Item	Provided estimate	% inferred agreement	% inferred in reversed condition	p
1. Like poetry	86	87	25	.0001
2. Gaining or losing weight	33	13	67	.001
3. Read about science	32	35	75	.02
4. A good cry	10	26	96	.00001
5. Talk about sex	65	78	50	.09
6. Stir up excitement	95	83	13	.00001
7. Like to flirt	65	83	38	.01
8. Eyesight good	38	29	79	.002
9. Enjoy love stories	13	17	76	.0001
10. Keep a diary	82	78	25	.001
11. Go to dances	09	09	79	.00001
12. Tell a lie	68	78	42	.03

With the exception of confidence ratings, Experiment 3 yielded support for our hypotheses. For individual items, inferred endorsements depended on the direction and the extremity of provided estimates. Subjects used population estimates, as they had used in-group estimates in Experiment 2, to make a judgment about the estimator's choice. Together, these findings resemble the outcome of Experiment 1, wherein consensus effects emerged for population and in-group estimates but not for out-group estimates.

Subjects' remarkable intuitions about consensus effects led to the final hypothesis that the estimates of others are inferred from their choices. To only hypothesize that choices are inferred from estimates, but not vice versa, one would have to argue the unlikely point that people believe that high estimates cause affirmative choices but not vice versa. Bidirectional inferences are consistent with the notion that people are good enough Bayesians rather than pure egocentrics. Self-generated and other-generated data are equally valuable in statistical induction. It has been shown, for example, that given the response of a randomly chosen other, people generalize as much as they do from a self-generated response (Sherman, Presson, & Chassin, 1984).

Experiment 4

Subjects read about a fictitious person's choice concerning the compliance with a request and were asked to guess that person's population estimate. The first hypothesis was that people infer high estimates from affirmative choices and low predictions from negative choices. Second, we expected that the availability of actual consensus would attenuate but not eliminate attributed consensus bias. Third, estimates attributed to others may not only depend on the actors' choices but on the observers' own hypothetical choices as well. If people believe that others think and act as they do, they should expect the population estimates of others to be similar to their own. Consequently, estimates attributed to actors should be an additive function of the actor's and the observer's choice.

Method

Forty-seven male and 38 female undergraduates were recruited on the Brown University campus and were asked to fill out a brief questionnaire. The questionnaire described the Ross et al. (1977, pp. 290–291) instructions for their Experiment 4. In that experiment, subjects were first asked to decide whether they were willing to walk on the campus of Stanford University carrying a sandwich board with the words *Eat at Joe's*. The Stanford subjects then estimated the percentage of people who would comply with this request. After reading this cover story, our subjects read about "John," a fictitious subject in the Stanford experiment. Half learned that he had chosen to wear the sign; the other half were informed that he had declined. Within each condition, one third of the subjects did not receive the actual consensus on compliance. A second third received the actual consensus (70%), and the remaining subjects received its inverse (30%). The main dependent variable was the subject's estimate of John's population estimate (in percentages). After the estimate had been made, subjects were asked the following: "If you had been a participant in the Stanford study, would you have agreed to walk around with the sandwich board?" The design was a 2 (actor's choice: compliance vs. noncompliance) × 2 (own hypothetical choice: comply vs. not comply) × 3 (actual consensus: no information vs. low vs. high actual consensus) factorial, analyzed by ANOVA. As in Experiments 1 and 2, effects were considered significant only when $p < .01$.

Results and Discussion

Table 6 shows that the results support the hypotheses. Subjects attributed higher estimates to the compliant John (M =

Table 6
Experiment 4: Inferred Estimates of Compliance as a Function of Actor's Choice and Actual Consensus

		Actual consensus	
Actor's choice	Not provided	30%	70%
Compliance	47.92	40.20	51.25
Noncompliance	32.24	14.79	42.73

46.57) than to the noncompliant John ($M = 29.17$), $F(1, 61) = 14.65$. In the condition in which the actual consensus was withheld, the between-groups difference (15.68 percentage points) was virtually identical to the difference obtained by Ross et al. (1977) for self-related estimates (17.5 percentage points). Subjects were also responsive to the provided actual consensus, $F(2, 61) = 5.65$. Paired comparisons (by Tukey test) revealed that low actual consensus elicited lower inferred estimates than did high actual consensus ($p < .01$) but that neither condition differed significantly from the condition without actual consensus. Finally, subjects who were inclined to comply themselves attributed higher estimates to the actor ($M = 45.88$) than did subjects who were disinclined to do so ($M = 32.61$), $F(1, 61) = 17.04$. Observed and one's own hypothetical choice were independent (chi square = .10). The finding that the effects of one's own and other's choice on estimates were additive suggests that subjects were sensitive to sample size. They treated their own preferences and those of others as independent, additive cues toward actual consensus.

The findings support the idea that social estimation is a special case of induction. In statistical induction, self-generated or other-generated data are equivalent. The source of the data does not affect their power to reduce uncertainty. To the extent that people realize this, they should project and they should expect others to project as well.

However, the results of Experiment 4 also highlight an important difference between social estimation and generic induction. In social estimation, a piece of self-related information is always available. Although the weight in its use may vary, it can never be excluded. Inferring someone else's consensus estimate, given that person's choice, will automatically invoke a personal preference, however hypothetical (Zajonc, 1980). In generic induction, on the other hand, population percentages can be estimated on the basis of a single case outside of oneself. The imaginary male explorer on a Pacific island would be able to estimate the percentage of blue "shreebles" on the basis of a small number of observed shreeble birds without asking whether he himself is blue (Nisbett, Krantz, Jepson, & Kunda, 1983).

General Discussion

Our research started with the idea that TFCEs need to be separated from simple projection. It was shown theoretically and empirically that the within-subjects correlation between the difference score (estimated minus actual consensus) and endorsements could accomplish this. This correlation, when squared, represented the person's effect size of the TFCE. In Experiment 1, consensus estimates showed simple projection and true bias when the target group included the rater. That is, the rater's position on the items predicted the rater's estimates and the inaccuracies relative to actual consensus. Because this effect was correlational, it was equally correct to say that the rater's estimates and the inaccuracies relative to actual consensus predicted the rater's positions. In Experiments 2 and 3, we made the sobering discovery that subjects knew this. Observers used a target person's estimates about in-groups and about the general population to infer that person's item endorsements. The results of Experiment 4 show that observers attributed higher consensus estimates to actors who had made a certain behavioral choice than to those who had not made that choice.

To a degree, the reported phenomena can be understood by assuming that people use Bayesian rules of induction reasonably well. Simple projection to in-groups but not to out-groups, inferring the choices of others from their consensus estimates, and inferring that others will project from their own positions to their in-groups are all compatible with the Bayesian requirement that individual pieces of data be used in estimations of actual consensus. However, Bayesian thinking neither predicts the commission nor the recognition of TFCEs in others. Therefore, our results reinforce the conclusions of other investigators that consensus effects indeed involve genuine cognitive–motivational distortion. People project too much, but at the same time they recognize and attribute TFCEs to others. Given this knowledge, the question arises as to why estimates do not approach actual consensus. This is a paradox. If everyone's judgments did approach actual consensus, TFCEs would disappear and could not be attributed to others either. Social perceivers, with their biased judgments about their social environment, their comprehension of these biases, and their attribution of these biases to others, are caught in a hall of mirrors.

The need to postulate cognitive–motivational processes that contaminate judgment poses a final question. Are the processes involved in the commission of TFCEs the same as those involved in the recognition and attribution of TFCEs? The coexistence of TFCEs and attributed TFCEs would be truly paradoxical if they sprang from the same underlying mechanisms. If the two phenomena can be traced to different mechanisms, then the paradox of their coexistence may be more apparent than real. Indeed, some of the traditional explanations of consensus bias may apply only to the commission but not the recognition of TFCEs. Ross et al. (1977), for example, suggested that one's own behavior seems common because one has disproportionate exposure to and memory for one's own actions and those of similar others. It has also been argued that people are motivated to judge their own actions as common in order to guard against the threat of deviance (Agostinelli, Sherman, Presson, & Chassin, 1992). Although these self-related factors plausibly explain TFCEs, it is difficult to see how they might account for subjects' willingness to attribute higher estimates to other compliant than to noncompliant target persons (Experiment 4). Subjects had no biased exposure or memory for cases consistent with the observed person's behavior, and there was no reason to be motivated to see the other person's action as relatively common.

It is possible, however, that people have a rudimentary understanding of psychological processes and that they believe that these processes do not only operate in themselves but in others as well. Ironically, this would be a consensus effect by itself, although on a higher level. Thus, people may assume that others are motivated to see themselves in the majority; they may assume that others have been selectively exposed to people who are similar to them; and they may assume that to others their own positions are particularly salient and available. Notice that committing the TFCE may come in a comparatively automatic fashion (Bargh, 1989), whereas attributing the TFCE to others may require a more sophisticated lay psychology. We suspect that if attributive inferences are made, they probably demand some effortful reflection. In other words, there may be a difference in the ease with which self- and other-related judgments are made. A possible test of this hypothesis would require subjects to make consensus estimates for themselves and for others under time pressure. If subjects lack the time or the cognitive resources to reflect about the perspectives of others, the paradox may vanish. Perhaps under such conditions people exhibit the ultimate false consensus effect. That is, Jerry may assume that George shares his consensus estimates even if George disagrees with Jerry with respect to a target item. To the extent that many real-life inferences are made fast and with a minimum of conscious analysis, consensus bias begets incredulity or frustration when others disagree with one's preferences.[3]

In this research we assumed that people do not recognize TFCEs in their own judgments. We left this assumption untested because we considered the coexistence of the commission and recognition of TFCEs *within the same person* patently illogical. In cases in which observers have the mental resources to detect TFCEs in others but fail to recognize their own, their truly egocentric conclusion is "I know that I am projecting, but you are projecting too much."

[3] A scene from the popular TV show *Seinfeld* illustrates this point. Mrs. Seinfeld's distraught exclamation to her son Jerry—"How can anyone not like you?!"—reveals that she failed to allow opinions about her son other than her own.

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