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Conditional Projection: How Own Evaluations Impact Beliefs about Others Whose Choices Are Known

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ABSTRACT

We study how a person's evaluation of choice options influences her estimates of other people's evaluations when their choices are known. People rely on the relation between their own evaluations and their final decision to make sense of others, projecting their evaluations of the corresponding options. A person's liking of the option she chose between two alternatives influences her estimates of others' liking of the option they chose, regardless of whether it matches her own choice. Likewise, her evaluation of the option she rejected affects her estimate of others' evaluations of the option they rejected. Across four studies, we provide evidence of conditional projection in political and consumer decisions, using across-people differences in ratings of choice options, within-person changes in ratings, as well as manipulated differences in participants' ratings. We demonstrate that existing accounts of projection would not directly predict our findings, and rule out other alternative explanations.

Our beliefs about others' preferences are an important element in decision making. We rely on these beliefs for making decisions in a variety of situations, from gift giving to cooperation, from product adoption to market entry, from bidding to bargaining. Others' choices are often visible, but the underlying evaluations that led to these choices are often the "missing piece" that people fill in when trying to make sense of others. Little is known about how people infer others' evaluations of the options when only the final choice is observed, or what role one's own evaluations might play, if any.

For example, consider an Obama supporter during the 2008 Presidential election interacting with a colleague whom she knows is a McCain supporter. What does she think about his views about Obama? Clearly she knows her colleague's evaluation of McCain is higher than his evaluation of Obama, but she could still conclude her colleague's evaluation of Obama is quite negative, neutral, or even somewhat positive. How do her evaluations of the candidates influence these beliefs, if at all?

We explore how one's own evaluations might affect inferences about others' *evaluations* (i.e., the subjective liking) of choice options, conditional on knowing others' choices. Prior research on egocentrism (Epley et al. 2004) argues that relevant aspects of one's self serve as natural starting points for reasoning about others. Research on social projection (see Marks and Miller 1987, and Clement and Krueger 2002, for reviews) demonstrates that estimates about an aspect of others, specifically binary choices and endorsements, relate to that same aspect in one's self, when nothing else is known about the person. By extension, we may expect a person's estimates of others' evaluations of an option to be directly influenced by her own evaluation of that same option.

However, in inferring the evaluations of others whose choices are known, people are privy not only to their evaluations of each option, but also to the relationship between own their evaluations and their final choices. Thus, people can draw from a rich set of aspects to form a mental model of the other person. Studying beliefs about evaluations introduces this novel ambiguity and enables us to investigate how knowledge about others' choices shapes projection.

We propose that when a person knows which option others have chosen among two alternatives and estimates others' evaluations of one of the options, she will specifically project her own evaluations of the *corresponding option*[1] rather than her evaluations of the same option. This "conditional projection" account yields testable predictions for observers' beliefs about the evaluations of others who have chosen differently between the same two alternatives ("opposite-choosers"). First, consider an observer's estimate of the evaluation that an opposite-chooser places on the option the latter rejected. We predict that the observer's evaluation of her rejected option, rather than of the option being estimated, will influence her estimate. For example, Obama voters with more positive evaluations of McCain will believe McCain voters have more positive evaluations of Obama. Second, we predict that the observer's evaluation of her chosen option, rather than of the option being estimated, will influence her beliefs about the opposite-chooser's evaluation of *his* chosen option. Thus, in our example, Obama voters with more positive evaluations of Obama will believe McCain voters correspondingly have more positive evaluations of McCain.

This view is distinct from three alternative predictions (illustrated in Figure 1) that are

[1] We define the *corresponding option* for another person's chosen option as one's own chosen option, and the *corresponding option* for another person's rejected option as one's own rejected option, regardless of whether one's own choice matches the other person's choice.

based on the assumption that the observer's mental model of the opposite-chooser is determined by attempting to directly relate her own evaluation of an option to the opposite-choosers' evaluation of that same option.

I. People might directly and negatively project their own evaluations of an option. A person with a high evaluation of her chosen option would believe opposite-choosers have a lower evaluation of it than would a person with lukewarm evaluations. For example, Obama voters with high evaluations of Obama would believe McCain voters value him less, on average, than do Obama voters with lower evaluations.

II. People might directly and positively project their own evaluation of an option. A person with a higher evaluation of her chosen option would then estimate other-choosers' evaluations as being higher (albeit lower than their evaluations of their chosen option). For example, Obama voters with a high evaluation of Obama would estimate McCain voters' evaluations of Obama as being, on average, higher than would those with lower evaluations.

III. People might not project if they find opposite-choosers too different to rely on any aspect of themselves for making sense of them. In this case, everyone would, on average, have the same beliefs about opposite-choosers' evaluations.

These three predictions differ from those of our conditional projection hypothesis (depicted as IV in Figure 1) in the type of introspective information people rely on when forming opinions of others. Across four studies, we test whether and how people rely on their own evaluations to estimate others' evaluations. We find uniform support for conditional projection: people project their evaluations of the corresponding options onto others, regardless of others' choices. We argue these findings provide evidence that people use the relation between their

evaluations and their final decisions as an immediately accessible point of reference to make sense of others' evaluations given their decisions.

In contrast, recent literature has concluded that people do not project onto others with different choices (O'Brien and Ellsworth, 2012; Ames, 2004b) because they do not treat aspects of themselves as relevant for making sense of dissimilar others (Ames, 2004a; Krueger 2000). We agree that people do not rely on their own evaluations of an option to guess how others who chose differently evaluate that same option. Instead, the conditional-projection account proposes that people may make estimates about others' evaluations given their choices based on how their own evaluations shaped their own choices, even when other's choices do no match their own. Indeed, our results show that people rely on their evaluations of the corresponding option to guess the evaluations of others with different choices. Conditional projection suggests people have a sense of shared similarity to others, even when their decisions are incongruent. These results highlight the pervasive influence of our own perspectives on how we make sense of others.

In the next section, we review the existing literature on social projection and relate prior accounts of projection to the different predictions discussed above. Then we present evidence for conditional projection across four studies. Study 1 provides evidence for our account using across-people differences in ratings of choice options and within-person changes in ratings over time in a real-world political decision context. Study 2 expands the analysis to a product-choice domain, investigates the role of beliefs in the correlation between option evaluations and examines participant's awareness of projection. Studies 3 and 4 provide direct causal evidence for our account by manipulating preferences, across participants and within participants over time, respectively. The different choice contexts in these studies establish the robustness and

generalizability of our findings. In the last section, we review and rule out possible alternative hypotheses and discuss the implications of our findings for related fields of research.

LITERATURE REVIEW AND THEORETICAL DEVELOPMENT

Research in social psychology has shown that aspects of a person's self often serve as a starting point for making inferences about others (Allport 1924, Epley et al. 2004). The literature on social projection (or "false consensus," Ross, Greene and House 1977) is the most directly relevant body of research to our paper. False consensus has been defined as the phenomenon that "people who engage in a given behavior will estimate that behavior to be more common than it is estimated to be by people who engage in the alternative behavior" (Mullen et al. 1985). Consistent with this widely adopted definition, false consensus has generally been demonstrated as a positive correlation between one's own choice (i.e., choosing option A over option B) and the estimated likelihood of that choice among others (see Figure 2).[2] We will instead use the term "choice projection" to refer specifically to this robust finding for choices and to avoid the non-normativity claims implied by "false consensus."

Projection of Choices to Different groups: Moderation of "False Consensus."

Three primary competing process accounts of social projection are based on the evidence of choice projection: anchoring (Clement and Krueger, 2002), differentiation (Mullen et al. 1992; Cadinu and Rothbart 1996), and induction (Dawes 1989; Hoch 1987). Clement and Krueger (2002, p. 220) differentiate these accounts by noting "all three theories predict projection to the

[2] Few exceptions to the binary-decisions context exist. Nisbett and Kunda (1985) provide evidence that one's own evaluations influence estimates of others' evaluations when others' choices are not known; see also Ames 2004a).

in-group, [but] one theory (differentiation) predicts reverse (i.e., negative) projection to the out-group, another theory (induction) predicts reduced projection to the out-group and a third theory (anchoring) predicts no projection to the out-group."

Figure 3 visually summarizes the differing expected relationships between one's own choice and one's expectations about an out-group's choices for each theory. In the differentiation account, people are motivated to contrast away from dissimilar others, yielding an inverse relationship between own choice and beliefs about the out-group's choices. According to the induction account, people rely on the self as a valid source of information, yielding a positive (but potentially weak) relationship between own choice and believed choices in the out-group. The prevailing theory (Robbins and Krueger 2005) is that people anchor on the self, but only when the self is directly relevant to the group being estimated, with no projection of choices to out-groups or dissimilar others (Ames 2004b).

Projection of Evaluations beyond Choice Projection

Although the existing theories do not make specific predictions when others' choices are known, Clement and Krueger (2002) have described them as broadly extending to reasoning about others. Accordingly, we outline how these theories could be extended to predict the three alternatives for projection to opposite-choosers we discussed in the introduction. To do so, we assume that opposite-choosers are seen as dissimilar out-group members. This assumption is generally held in the social projection literature and explicitly employed in recent work by O'Brien and Ellsworth (2012) and Ames (2004a).

The differentiation theory may imply a negative relationship between a person's own evaluation and her estimates of opposite-choosers' evaluations for a given option, based on the

argument that people contrast their evaluations with that estimated for the out-group. In contrast, arguments made for the induction theory may imply a positive relationship based on the observation that one's own evaluation may be informative about others' evaluations of the same option. Lastly, the selective anchoring theory may imply a lack of any relationship between own evaluation of a choice option and estimates of the opposite-choosers' evaluations of that option. Researchers have relied on demonstrating lack of projection to out-groups as evidence of selective anchoring. Krueger (2000, p. 334) is particularly emphatic on this point, arguing, "The surest way to eliminate projection is to ask people to estimate social consensus for a group to which they do not belong. . . . It is as if people treat members of out-groups as members of different species."

These three possibilities are depicted in Figure 4. The figure also reflects several expected null relations due to the assumption that others' evaluations of the two options are orthogonal to each other. If people think others' evaluations of the two options are correlated, the projection of own estimates for one option might influence estimates of the other option. For clarity of exposition, we focus on the case of uncorrelated evaluations here and revisit this issue in the empirical data.

The conditional projection hypothesis proposes that people may perceive a similarity in the way evaluations shape decisions, even when those decisions are known to be different. Thus it predicts a different pattern (see Figure 4). When estimating opposite-choosers' ratings of one choice option, for example, Option B, a person is expected to rely primarily on her own evaluation of Option A, which has a corresponding status to the opposite-choosers' evaluation of Option B. Specifically, if she has chosen Option A, Option B is the chosen option among

opposite-choosers, and if she has rejected Option A, then Option B is the rejected option among opposite-choosers. In contrast, her evaluation of Option B may not have any impact on her estimate of opposite-choosers' evaluations of Option B precisely because of its different choice status.

The literature on similarity judgments suggests an explanation for why people may still perceive a similarity to others whose choices do not match their own and thus relate their evaluations to theirs. This literature distinguishes between two types of assessments: surface similarity, which is based on matches in the same information; and analogical similarity, which is based on parallels in corresponding information (structural alignment, Gentner and Markman 1997). This distinction has not been incorporated in existing research on social projection, which has so far relied only on surface similarity to define out-groups. Our projection account is consistent with opposite-choosers being perceived as analogically similar, based on a structural alignment between one's own and others' evaluations of the chosen and rejected alternatives, respectively.

Analogical inference is prompted directly by the goals and process of comparison, often occurring spontaneously when the object of analogy (in this case, the self) is accessible in mind (Holyoak and Koh 1987; Gentner and Medina 1998). Consistent with research on egocentrism (Epley et al. 2004), we expect people's own decision process to generally be salient. The act of making estimates about others will prompt people to map their own decision process onto others, facilitating spontaneous analogical inferences. Accordingly, we predict people will primarily base their estimates of another person's evaluation of an option on their own evaluation of the analogous option.

Next, we present the results of four studies that investigate people's beliefs about the

evaluations of same-choosers and opposite-choosers. The pattern of projection onto opposite-choosers will provide the key test for our account of conditional projection as distinct from direct extensions of the existing choice-projection theories to evaluations.

STUDY 1: VOTERS' PROJECTIONS OF CANDIDATE EVALUATIONS

In Study 1, we investigate the relation between a person's evaluations of political candidates and the person's beliefs about others' evaluations of these candidates. We utilize political decision-making as a particularly appropriate and important context to study the formation of people's beliefs about others' views. These beliefs are influential in deciding for whom to vote (Bartels 1988), whether to donate to or volunteer for a political campaign (Fehr and Fischbacher 2003), and even where to live (Bishop and Cushing 2008). Beliefs about other voters differ considerably, although detailed factual information about others' views is widely available and consumed. For example, in the 2008 election, the media reported over 500 polls encompassing over 800,000 voters (Blumenthal 2008).

We conducted three studies (Studies 1a, 1b, and 1c) before the 2008 U.S. Presidential general election. We use both cross-sectional and within-person differences in candidate evaluations to examine whether and how differences in one's own evaluations relate to differences in beliefs.

Methods.

We conducted Study 1a among undergraduate Obama supporters at a major Midwestern university one week before the 2008 election, using an incentive-compatible belief elicitation method. Participants rated each of the candidates, from very unfavorable (1) to very favorable

(9), and chose the candidate for which they would vote. They then predicted choices among all other participants. Next, they estimated the distribution of ratings of each candidate on the same favorability scale using an interactive bar chart (Figure A1, in the Web Appendix), first only among McCain voters, then only among Obama voters, and finally among all voters.

As the student population strongly favored Obama, differences in the beliefs of these voters were the focus of the study, and we collected the views of a small group of McCain supporters to implement an incentive-compatible elicitation of beliefs. We told participants they could earn up to an additional $8 for the accuracy of their estimates, by minimizing the absolute errors in their estimation. After the study, one question was selected at random, and we re-contacted participants to collect their additional accuracy-incentive compensation. In all, we collected 72 surveys from Obama supporters, all of which were complete and internally consistent.

Studies 1b and 1c used a single sample of registered voters, drawn from a national consumer survey panel. We conducted Study 1b September 25–26, 2008, right before the first presidential debate. We re-contacted the same respondents for Study 1c during October 1–2, 2008. In the pre-debate survey (1b), respondents chose the candidate they planned to vote for and rated each candidate on a scale from 0 (“very unfavorable”) to 10 (“very favorable”). Respondents then estimated the percentage of voters that would vote for each candidate. Following these questions, we elicited respondents’ beliefs about the average favorability rating of others, first among all other participants in the survey, then among only same-choosers (fellow supporters of their preferred candidate), and finally, among only opposite-choosers (supporters of the other candidate). The post-debate study (1c) repeated these questions, counter-balancing

the same-chooser and opposite-chooser estimation tasks. The pre-debate survey yielded 351 internally consistent and complete surveys, of which 153 also had a complete post-debate survey (Study 1c)[3]. Respondents who completed the re-contact survey did not significantly differ in observable characteristics or political attitudes (Table A1)[4] from those who did not complete it.

Social Projection in Beliefs about Evaluations and Choices of Other Voters in General.

Before we turn to beliefs about others whose choices are known, we briefly consider beliefs about others whose choices are unknown. First, we replicate choice projection. As Granberg and Brent (1983) and Brown (1982) document for the presidential elections from 1952 to 1980, people who chose one candidate believed that choice was more prevalent in the population, compared to the beliefs of those who instead chose the other candidate (see Table A2).

Second, we provide novel evidence for projection of candidate ratings over and above choice projection. Previously, Nisbett and Kunda (1985) showed a correlation between own ratings and guesses of others' mean ratings. Of course, this relation might reflect the impact of one's own choice on beliefs alone, as those who choose an item also evaluate it as higher. However, we show that voters' relative candidate ratings impact their beliefs about the election outcome ($\beta = 1.03$, $p < .01$), even controlling for the effect of their own choices ($\beta = .42$, $p > .1$; Table A3). Thus the influence of a person's own evaluations on her beliefs about others' choices is not attributable to her evaluations being a proxy for her choice. Even two voters who make the same choice systematically differ in their predictions for the election outcome if they differ in

[3] Of 483 completed surveys, we excluded respondents whose ratings conflicted with their choices (1%); who estimated a lower rating for the chosen vs. rejected option in a subgroup (15%); or whose summed option share estimates exceeded 100 (10%).

[4] Tables denoted with A can be found in the Web Appendix.

their evaluations of the candidates. This finding motivates our investigation of how one's own evaluations shape beliefs about others' evaluations, *conditional* on knowing others' choices. Next, we briefly describe our findings for beliefs about same-choosers and then turn to beliefs about opposite-choosers, the group that provides the key test of our hypotheses.

Social Projection in Estimates of Others Whose Choices Are Known.

We explore how one's own evaluations impact one's beliefs about others' evaluations when their choices are known. A total of four dependent variables capture beliefs about same-choosers' and opposite-choosers' evaluations of two candidates: mean estimated rating of same-choosers' chosen option, same-choosers' rejected option, opposite-chooser's chosen option, and opposite-choosers' rejected option. We conduct a multiple regression for each of the dependent variables, with participants' own ratings of the chosen and rejected options as the predictors. The dependent and independent variables are recoded from each participant's beliefs about each sub-group and evaluations of each candidate, based on the participant's choice.

Projection onto same-choosers. In a series of multiple regressions (Table 1, top panel), beliefs about same-choosers' mean ratings of their chosen candidate are predicted by ratings of one's own chosen candidate (Study 1a: $\beta = .17$, $p < .05$, 1b: $\beta = .50$, $p < .01$; 1c: $\beta = .54$, $p < .01$), and not by ratings of own rejected candidate (1a: $\beta = -.02$, $p > .1$; 1b: $\beta = .05$, $p > .1$; 1c: $\beta = .05$, $p > .1$). Likewise, beliefs about same-choosers' ratings of their rejected candidate are predicted by ratings of own rejected candidate (1a: $\beta = .14$, $p < .05$; 1b: $\beta = .57$, $p < .01$; 1c: $\beta = .68$, $p < .01$; Table 1, lower panel) and not by ratings of own chosen candidate (1a: $\beta = -.02$, $p > .1$; 1b: $\beta = .07$, $p > .1$; 1c: $\beta = -.04$, $p > .1$). Both the extensions of the current theories of false consensus and our own conditional-projection account predict this positive relation.

Projection onto opposite-choosers. Projection onto others whose choices are known to be different from one's own provides support for a novel view of social projection. Estimates of opposite-choosers' ratings of their chosen candidate are predicted by ratings of the participants' own chosen candidate (Study 1a: $\beta = .17$, $p < .10$; 1b: $\beta = .16$, $p < .01$; 1c: $\beta = .41$, $p < .01$; Table 2, top panel) and (except for Study 1b) not by the ratings of the participants' rejected candidate, which is the same as the opposite-choosers' chosen candidate (1a: $\beta = .08$, $p > .1$; 1b: $\beta = .17$, $p < .01$; 1c: $\beta = .07$, $p > .1$). In addition, we find estimates of opposite-choosers' ratings of their rejected candidate are predicted by ratings of the participants' own rejected candidate (Study 1a: $\beta = .24$, $p < .01$; 1b: $\beta = .39$, $p < .01$; 1c: $\beta = .40$, $p < .01$; Table 2, lower panel) and not by ratings of the participants' own chosen candidate (1a: $\beta = -.14$, $p > .1$; 1b: $\beta = -.06$, $p > .1$; 1c: $\beta = .08$, $p > .1$). In sum, a person's rating of her chosen candidate predicted her estimate of how the supporters of the opposing candidate would rate *their* chosen candidate, and a person's rating of her rejected candidate predicted her estimate of how the supporters of the opposing candidate would rate *their* rejected candidate. These results are robust to the order of belief elicitation.

The unique prediction of our account, projection of the corresponding evaluations to opposite-choosers (as depicted in the far right of Figure 4), is consistently supported by our data. In contrast, our results are inconsistent with three other possible predictions also shown in Figure 4. These results support a novel view of projection, in which people find their own evaluations relevant for making sense of others and draw a parallel between themselves and others in how the evaluations shaped final decisions. These findings are contrary to the argument that people generally lack the ability to project onto dissimilar others, such as those with opposing political allegiances (O'Brien and Ellsworth 2012).

These analyses relate static, across-people differences in evaluations to static, across-people differences in beliefs about others' evaluations and resulting choices. Next, we investigate how changes in a given person's evaluations over time impact changes in that same person's beliefs. If, as we argue, a person's beliefs are shaped by her corresponding evaluations, as her evaluations change over time, we should observe a change in her corresponding beliefs.

Changes in Beliefs about Others Whose Choices Are Known, from Changes in Own Evaluations

We test whether changes in a person's candidate evaluations relate to changes in her estimates about others' corresponding evaluations. The time between Study 1b and 1c included not only the first presidential debate, but also the public's reaction to McCain's campaign suspension and the candidates' responses to the rapidly deepening financial crisis and the proposed bailout. Although none of the participants' choices reversed over the several days between the two surveys, we observed substantial variation in people's evaluations of candidates. Differences between the pre- and post-debate evaluations ranged between -5 to +3 for the chosen candidate, and between -6 to +5 (on a 10-point scale) for the rejected candidate. The mean of the absolute differences was .96 for evaluations and 1.33 for beliefs.

The results based on changes in people's beliefs about how each candidate's supporters evaluated the two candidates (Table 3) reflect the same projection pattern we uncovered using static ratings and beliefs. In particular, voters who became more positive toward their chosen candidate also increased their estimates of how positively both same-choosers (β =.26, p < .01) and opposite-choosers (β =.26, p < .05) each rated their own candidate. However, changes in the evaluation of one's rejected candidate did not influence these beliefs (both p's > .1). Similarly,

changes in the ratings of the rejected candidate predicted changes in beliefs about same-choosers' rejected candidate (β =.32, p < .01), whereas changes in the evaluations of one's chosen candidate did not play a role (p > .1). The relation between changes in own evaluation of the rejected candidate and changes in beliefs about the opposite-choosers' evaluations of their rejected candidate was directionally consistent with our expected finding, but not significant.

The relation between shifts in evaluations and shifts in beliefs (even when choices have not changed) suggests that as we change the evaluations that form our choices, our beliefs about others change correspondingly.

Discussion of Study 1 Results.

Study 1 presents the first evidence that people's evaluations affect their beliefs about the evaluations of others whose choices are known, even when the other person's choice differs from one's own. These findings not only provide support for the fact that people project onto opposite-choosers, but more importantly, distinguish the specific way in which people rely on their own evaluations to make sense of others'.

The replication of our static findings in this within-person analysis rules out the possibility that the relationship between differences in own evaluations and beliefs across people is attributable to omitted individual factors, such as scale-usage bias or stable beliefs about the extremity of others' ratings. However, the context of choices between political candidates is likely to be more polarized (i.e., characterized by a negative correlation between evaluations of the two candidates) than a typical decision. To ensure the results are not limited to settings in which participants believe others' evaluations are negatively correlated, we not only replicate our findings in non-polarized contexts in Studies 2-4, but we also elicit participants' beliefs

regarding the correlation in evaluations, and find no moderation of our projection results.

STUDY 2: PROJECTION OF POSTER EVALUATIONS

Study 2 extends the analysis in Study 1 to a consumer product choice domain, elicits beliefs about the correlation between others' evaluations of the two choice options as well as their thought process in making estimates about others' evaluations.

Method.

In an online survey (N=127), participants were shown two black and white posters (one of the New York City skyline and one of a shoreline with round boulders), chose between them, and then rated both on a seven-point scale. They then estimated the ratings of both posters for a typical opposite-chooser, estimated opposite-choosers' evaluations, and answered an open-ended question about how they had made their estimates. Participants then answered the same questions for a typical same-chooser. We used this order to ensure that answering questions about same-choosers did not result in spurious projection onto opposite-choosers. We also replicate these findings in a between-subjects study using the same stimuli, reported in the Web Appendix.

In Study 1, we excluded a substantial number of participants who erroneously estimated a higher rating for a rejected option than a chosen option among a subgroup. In this study, the software was programmed to check for this problem during the survey, to inform participants who made any such error (29 out of the 127 completed surveys, or 22%), and ask them to restate their estimates for the task in which they had made a mistake. As a result, we obtain usable data

for 123 participants.[5] Excluding respondents who initially made a mistake but then corrected their answers (N=29) did not have any effect on the results, which supports the validity of such exclusions in Study 1. Participants also indicated their beliefs about the correlation between peoples' ratings and answered demographic questions.

Projection of Evaluations to Others Whose Choices are Known.

We replicate the findings of Study 1 regarding projection onto same-choosers (see Table 1). Estimates of same-choosers' ratings of the chosen poster are predicted by ratings of own chosen poster (β = .38, p < .01) and not by ratings of own rejected poster (β = -.01, p > .1). Likewise, estimates of same-choosers' ratings of the rejected poster are predicted by ratings of own rejected poster (β = .67 p < .01) and not by ratings of own chosen poster (β = -.13, p > .1). More importantly, we replicate our findings regarding projection onto opposite-choosers (Table 2). Estimates of opposite-choosers' ratings of their chosen poster are predicted by ratings of the participants' own chosen poster (β = .46, p < .01) and not by ratings of the participants' rejected poster (β = .05, p >.1). The estimates of opposite-choosers' ratings of their rejected poster are predicted by ratings of participants' own rejected poster (β = .52, p < .01) and not by ratings of the participants' own chosen poster (β = .08, p > .1). These results support our prediction that people's beliefs about opposite-choosers' evaluations are influenced by their own corresponding evaluations.

Awareness of Conditional Projection.

We have proposed that people draw a parallel between themselves and others based on

[5] One participant gave the rejected poster a higher rating than the chosen poster, and three participants (less than 3%) repeated the error when asked to correct their estimate and were therefore excluded.

the known choice, and conditionally project their own evaluations of the corresponding option. This process could be a deliberate and conscious inferential one in which people identify which of their own evaluations are most relevant for estimating the evaluations of specific others. However, prior research has suggested egocentrism in general (Epley et al. 2004), and choice projection specifically (Krueger and Clement 1994) are not conscious and may occur due to an automatic activation of relevant self-information. Likewise, analogical inference has been shown to occur spontaneously, without conscious awareness (Day and Gentner 2007). Thus the influence of own evaluations when estimating the corresponding evaluations of others might occur in the absence of a deliberate inferential strategy to do so. In this study, we collected some initial data on the basis of participants' reasoning.

First, we analyzed people's descriptions of how they had made their estimates.[6] A research assistant who was unaware of the hypotheses was given a list of the reasons, in random order, with no indication of which reasons corresponded to same-chooser or opposite-chooser estimates. The assistant coded whether each response discussed the posters as either polarizing or similar (or neither), and whether the response either compared or contrasted the participant with the group being estimated. The majority of participants did not relate their own preferences or decision processes to either estimate (79% same-choosers, 85% opposite-choosers, difference $\chi^2(1) = 1.3$, $p > .1$). They instead talked about aspects of the specific posters and their beliefs about others' likes and dislikes. Participants were more likely to indicate similarity rather than dissimilarity to others (20% vs. 2% for same-choosers, $\chi^2(1) = 17.0$, $p < .01$; 11% vs. 4% for opposite-choosers, $\chi^2(1) = 2.7$, $p < .10$). Thus, although the majority of people did not explicitly relate their own evaluations to the estimates for others, those who did were much more likely to

[6] We thank an anonymous reviewer for suggesting the thought-listing procedure.

mention a perceived similarity than dissimilarity. These results are consistent with a process of analogical inference being prompted by the estimation task, and therefore occurring spontaneously for most participants.

We also tested whether participants would agree with characterizing others' evaluations in terms of analogical relations, when explicitly presented with multiple possibilities. We asked participants to choose one of four statements that best described how they thought about the decision processes of opposite-choosers, and then of same-choosers. A strong majority in both cases (71% for same-choosers vs. 63% for opposite-choosers, difference $\chi^2(1) = 2.5$, $p > .1$) endorsed a description of opposite choosers' as analogous to themselves ("*Their preferences are probably analogous to mine: they feel about the poster they chose the way I feel about the one I chose*"), as opposed to others' preferences reflecting either (1) poor decisions, (2) decisions unrelated to one's own process, or (3) a process of direct contrast with one's own evaluations.

Taken together, these data suggest that although most participants' evaluations did not involve conscious analogical inference, most endorsed the implied congruent structural alignment between themselves and others. We provide these results as suggestive evidence for a spontaneous analogical reasoning account that can reconcile why people may view their evaluations as relevant for estimating the evaluations of others with dissimilar choices.

The Role of Believed Correlation between Evaluations.

Although political choices, as in Study 1, are often characterized as polarizing, consumers often own multiple posters and therefore appreciating one does not necessarily imply a more negative view of another. An exploration of whether our findings are sensitive to the correlation between evaluations of the two options, or to participants' beliefs about this correlation, is of value. A person who believes evaluations are negatively or positively correlated

might reconcile their estimates to reflect this belief. This reconciliation may have implications for the degree of projection of both option ratings. For example, as we mentioned earlier, the projection of own estimates for one option might also influence estimates of the other option.

In this study, participants' ratings of the two posters were slightly negatively correlated (r = -.15, p < .1), suggesting minimal polarization. However, their beliefs about correlations for others' ratings were mixed: 33% indicated a belief in negative correlations, whereas the majority indicated either no correlation (34%) or a positive correlation (33%). In the open-ended questions, participants' descriptions reflected similar mixed opinions about whether others' evaluations of the two posters were polarized or similar (41% polarized vs. 36% similar for same-choosers, χ^2 = .38, p > .1; 38% polarized vs. 42% similar for opposite-choosers, χ^2 = .25, p > .1). Participants' agreement with the statements characterizing the correlation between others' evaluations was significantly related to both the degree of correlation in their estimates and their open-ended description of their decision process as assuming others' views of the posters were polarized or similar (see discussion in the Web Appendix).

In a series of regression analyses (see the discussion in the Web Appendix and Tables A9 and A10), we find no evidence that participants' endorsements about the correlation between others' evaluations of the two options or their open-ended characterization of this correlation moderated any of the projection findings. We conclude that lay beliefs about correlations between others' evaluations of the two options do not explain or contribute to our understanding of the results in this study.

Discussion of Study 2 Results

In both Study 1 and this study, we used correlational analyses to investigate the presumed

projection of a person's own evaluations when estimating others'. One limitation of this approach is that it allows for the possibility that people might form their preferences based on their beliefs about others'. Although people are unlikely to base their evaluations on the believed evaluations of others who chose a different poster, we conducted a pre-test to offer evidence that the poster evaluations were not impacted by beliefs about opposite-choosers' evaluations (reported in the Web Appendix). To provide further causal evidence for our results, we also manipulate evaluations independently of choices in Study 3 and analyze systematic within-person changes in evaluations in Study 4.

STUDY 3: THE PROJECTION OF MANIPULATED EVALUATIONS

In this study, we manipulate evaluations to further establish the causality of conditional projection in beliefs about opposite-choosers. To directly test the causality of our claims, we need an experimental manipulation that yields differences in average evaluations of the chosen option versus the rejected option. This approach enables us to test whether the manipulated differences in evaluations, in turn, yield the corresponding differences in beliefs. Such a manipulation must satisfy several key criteria. First, it cannot provide differential objective information across conditions that might directly affect beliefs. Second, the manipulation must shift evaluations conditional on choice, because a manipulation that only shifted choices (but not evaluations among choosers of each option) would not allow us to test our claims. Third, to identify the effect of evaluations of the chosen option on beliefs separately from the effect of evaluations of the rejected option, the manipulation needs to change evaluations asymmetrically. If evaluations of both the chosen and the rejected options change in the same direction, the

manipulation does not provide a test that can rule out different patterns of projection. We find that manipulating whether people expect to provide reasons for their choice yields an appropriate experimental test of our claims.

Method.

A total of 89 students at a large Midwestern university completed a study about their preferences regarding two campus postcards. Postcards depicted a scenic spot on campus from two different vantage points, one a close-up of the Law School (Postcard A) and the other a wider-angle perspective including the Law quadrangle (Postcard B). We randomly assigned participants to either the *reasons* or the *no reasons* condition. In the *no reasons* condition, participants inspected both postcards, learned that a local artist painted the postcards and that both were of equal value, and were told they would be asked to pick the postcard they liked better and to rate both postcards. In the *reasons* condition, we gave the participants the same postcards and the same information, but also told them they would be asked to list two reasons explaining their choice, after they indicated their choice and ratings. Thus, in the *reasons* condition, participants knew they would be providing the reasons for their choice, but the actual information available to participants did not vary across conditions. After making their choice, participants rated their liking of both postcards from 1 ("Do not like at all") to 7 ("Like very much"). If they were in the *reasons* condition, they provided reasons for their choice on the next screen.

We expected the manipulation would not necessarily affect participants' choices, but would impact the strength of participants' preferences. Previous research shows that post-decisional accountability prompts defensive bolstering of initial attitudes (Lambert, Cronen, Chasteen, Lickel, 1996; Tetlock et al., 1989). Specifically, participants employ a confirmatory

thought process when they anticipate providing post-decisional justification, looking for reasons that bolster their initial choice (Lerner and Tetlock, 1999). Therefore, in our context, we hypothesized that expectations of justification would increase participants' focus on the differences between otherwise similar postcards. Accordingly, we expected an increase in preference strength, reflected in a decrease in the ratings of the rejected option or an increase in the ratings for the chosen option or both.

After participants made their choices, rated the postcards, and provided reasons (if applicable), we asked them to estimate the ratings of both postcards among others who had chosen either one or the other postcard. Participants who made mistakes (rating a postcard higher among a group defined by choosing the other postcard) were prompted to correct their mistakes. To ensure changes in beliefs were due to changes in own preferences and not to lay beliefs about the effect of giving reasons, we did not ask participants about other participants from the same study. Instead, participants in both conditions read about other students who had participated in an online survey in which they saw both postcards, chose one, and rated both, but were not asked any other questions (as in the *no reasons* condition). Finally, participants indicated their beliefs about the correlation between evaluations of the two postcards among others and filled out demographic questions.

Projection of Experimentally Manipulated Evaluations.

A total of 89 students in a major Midwestern university completed the study. The majority of participants in both conditions chose Postcard B (68% among no reasons, N=44; 80% among reasons, N=45, difference insignificant; $\chi^2 = 1.6$, $p > .1$). Furthermore, participants' ratings of the chosen option did not differ across conditions (M = 6.1 vs. 5.9, $t = 1.21$, $p > .1$). However, ratings for the rejected option were significantly higher in the *no reasons* condition

than in the *reasons* condition (M = 4.8 vs. 4.1, t =2.83, p < .01). A repeated measures ANOVA confirms the difference in ratings between conditions for the rejected option was significantly different from the difference in ratings between conditions for the chosen option (F(1,87) = 4.1, p < .05).

The manipulation produced the required asymmetric change in ratings, enabling us to distinguish between different potential patterns of projection based on this experimentally manipulated difference. Because ratings of the rejected option were higher in the *no reasons* condition, we expected the estimates of both same-choosers' and opposite-choosers' ratings of their respective rejected options to be higher in this condition. On the other hand, because ratings of the chosen option were not affected by the manipulation, we expected the estimates of both same-choosers' and opposite-choosers' ratings of their respective chosen options not to differ across conditions.

Indeed, participants' estimates of same-choosers' evaluations of their chosen postcard did not differ (M = 5.9 vs. 6.1, t =.96, p >.1), but estimates for the rejected postcard were higher in the *no reasons* (vs. *reasons*) condition (M = 4.2. vs. 3.7, t = 2.03, p < .05). This result provides evidence for a causal effect of own evaluations being directly projected in forming beliefs about same-choosers. Furthermore, estimated opposite-choosers' evaluations of their own chosen postcard did not differ (M = 5.8 vs. 5.9, t = .40, p >.1), but estimates of their own rejected postcard were higher in the *no reasons* (vs. *reasons*) condition (M = 4.3. vs. 3.8, t = 2.48, p < .05). Repeated measure ANOVA analyses confirm the differing effect of condition on estimates for the rejected versus the chosen option (F(1,87) = 7.63 same-choosers, F(1,87)=8.43 opposite-choosers; both p's < .01).

Discussion of Study 3 Results

The results of this study establish conditional projection as a causal mechanism. To clarify the findings of Study 3, consider the participants who chose Postcard B. When asked to provide reasons for their choice, they rated their rejected postcard (A) lower than if they were not asked to provide reasons. They also provided lower estimates for how A-choosers would rate postcard B, but gave nearly identical estimates of how A-choosers would rate postcard A.

In addition to providing causal evidence, Study 3 also fully replicates the conditional projection analyses employed in Study 2. We collapse the data from both conditions and thus combine manipulated and spontaneously occurring variation in evaluations and relate this variation to variation in beliefs about same- and opposite- choosers (see Tables A4 and A5). Furthermore, we find no consistent moderation of either the experimental effects or the correlational analysis by participants' beliefs about the correlation between others' evaluations of the options (Tables A11 and A12).

STUDY 4: THE IMPACT OF SPONTANEOUS CHANGE IN EVALUATIONS ON BELIEFS

In Studies 1b and1c, the change in people's evaluations occurred largely because of an external change in their information—the debate itself and the circumstances surrounding it. In Study 4, we investigate the effect of spontaneous changes in post-choice evaluations over time on beliefs, when no new information is made available.

Method.

In the first wave, the participants saw detailed profiles of Sony Cybershot and Panasonic Lumix, including pictures, the brand, identical prices ($120), and 19 technical attributes based on Amazon.com product descriptions including optical zoom, aperture, physical measurements, and

performance information (Figure A2 in the Web Appendix). Participants made their choices and rated each of the cameras from 1 ("Do not like at all") to 7 ("Like very much"). They estimated the ratings of both cameras for the typical person who chose the Sony camera, and then for the typical person who chose the Panasonic camera. Participants were prompted to correct their ratings if needed (i.e., estimating a higher Panasonic than Sony ratings for a Sony-chooser), and were told a second survey would be conducted in one week. In the second wave, participants were again shown the pictures, price, and brand name of both cameras, but did not see any of the other camera attributes they had seen the prior week. They rated both cameras on the same scale and their beliefs were collected, as in the first wave.

We did not expect participants' choices to change, because they were likely to remember their initial choice and no new information was presented. However, we speculated evaluations would systematically change over time. In the first wave, participants faced a relatively complicated choice, because each camera was superior on some of the attributes. In the second wave, however, participants had to rely on their initial choices and memory of attributes when evaluating the cameras. Therefore, we expect participants in Wave 2 to anchor on their initial choice and, with the conflicting attributes being less salient, to have stronger preferences. Any systematic change specifically in participants' chosen (or rejected) brand evaluations would present a unique way to test conditional projection, which would predict a corresponding change in beliefs for both same-choosers and opposite-choosers.

We collected 274 complete and usable surveys.[7] The chosen camera rating for a participant in a given wave was calculated as the higher of the two camera ratings in that wave for the participant; the rejected camera rating was calculated as the minimum of the participant's

[7] Of 277 participants who completed valid surveys in both waves, we excluded three because they estimated a group as rating their rejected camera as higher than their chosen camera even after being prompted to correct the mistake.

two ratings in that wave. In Wave 1, 30% of the participants preferred the Sony camera and 70% preferred the Panasonic. Fifteen people had a change in the higher-rated camera between the two waves, 14 from preferring Panasonic to preferring Sony and one from Sony to Panasonic. We coded a participant's estimates for same-choosers and opposite-choosers based on the camera the participant rated higher in that wave, with stated choice used as a tie-breaker. The results are robust to excluding these participants.

Relating Within-Person Changes in Evaluations to Within-Person Changes in Beliefs.

Participants' evaluations varied substantially between Wave 1 and Wave 2. The mean of the absolute differences between evaluations in wave 1 and wave 2 was .51 for the chosen and .4 for the rejected option (on a 7-point scale). The changes in the evaluations of the chosen camera ranged between -2 and 2, and changes in the evaluations of the rejected camera ranged between -4 and 2. More importantly, we relate these within-person changes in evaluations to within-person changes in beliefs, replicating the analysis presented in Study 1.

In Study 4, this analysis takes advantage of both the manipulated and the spontaneously occurring variation in evaluations over time. As depicted in Table 4, an increase in the evaluation of the chosen option led to an increase in the estimated evaluation of the chosen option of same-choosers (β =.19, p <.01), but increased evaluations of the rejected option did not (p > .1). Likewise, an increase in one's own evaluation of the chosen option led to an increase in the estimated evaluation of opposite-choosers' chosen option (β =.16, p <.05), but changes in own evaluations of the rejected option did not (p > .1). Similarly, an increase in the evaluation of the rejected option led to an increase in the estimated evaluations of the rejected option of same-choosers (β =.33, p <.01), but an increase in the evaluation of the chosen option did not (p >.1).

Finally, an increase in the evaluation of the rejected option led to an increase in the estimated evaluation of the rejected option of opposite-choosers (β =.26, p <.01), as well as a marginal increase in the estimate for the chosen option (β =.15, p <.1).

Impact of Systematic Changes in Evaluations over Time.

Our experiment yields not only overall variation in evaluations over time, but more importantly, the passage of time and absence of repeating product information in Wave 2 systematically shifts evaluations.[8] Participants' average rating for their rejected camera was lower in Wave 2 than Wave 1 (M = 4.39 vs. 4.60, t = 4.0, p < .01), whereas ratings of the chosen brand did not differ (M = 5.81 vs. 5.84, t = .76, p > .1). This difference represents an asymmetric shift in evaluations, with a significantly different change across the two waves for the rejected versus chosen camera (F(1,273)=12.32, p < .01), which provides a causal test for conditional projection onto opposite-choosers.

Consistent with conditional projection and the lack of change in participants' own ratings of their chosen option, average estimates of opposite-choosers' evaluations of their respective chosen option did not differ across the two waves (M = 5.98 vs. 6.04, t = 1.6, p > .1), and we find only marginal changes for same-choosers (M = 6.05 vs. 6.12, t = 1.9, p < .1). On the other hand, corresponding to participants' own decline in evaluations of the rejected camera, the estimated evaluations of the rejected option decreased among both same-choosers (M = 4.13 vs. 4.40, t = 5.0, p < .01) and opposite-choosers (M = 4.26 vs. 4.47, t = 3.80 p < .01). Both the differences for same-choosers (F(1,273)=13.01, p < .01) and opposite-choosers (F(1,273)=6.28, p < .05) were statistically significant. Only our conditional-projection account predicts this specific pattern of

[8] We asked choice before ratings in Wave 1, and coded evaluations as chosen vs. rejected separately in each wave, ignoring the choice question. Thus the effect of our manipulation on ratings does not imply either cognitive dissonance or revealed preference.

casual relationship.

Discussion of Study 4 Results

In this study, we replicate our finding that changes over time in one's evaluations leads to changes in one's beliefs (as in Study 1), even when no new information is provided during that time. Furthermore, we find that a within-subjects manipulation of evaluations leads to corresponding changes to participants' beliefs that support our causal account of projection. We fully replicate all the prior static findings when we analyze Waves 1 and 2 separately (see Tables A4 and A5). Again, we find no evidence that any of the findings are consistently moderated by people's beliefs about correlations between others' ratings of the two options (Tables A13 and A14).

GENERAL DISCUSSION

Across four studies, our findings support the notion that people estimating the evaluations of others draw a parallel between themselves and others based on the known choice, and conditionally project their own evaluations of the corresponding option. These findings not only provide support for the fact that people project onto opposite-choosers, but more importantly, distinguish the specific way in which people rely on their own evaluations to make sense of others. We provide a brief overview of why our findings are incompatible with several key

alternative explanations, and then discuss implications for the theory of social projection, as well as for consumer decision-making.

Discussion of Alternative Accounts.

Systematic differences in reasoning. If unobserved person-specific factors jointly impact both own ratings and beliefs about others' ratings, spurious correlations would result. Potential causes include rating differences in scale usage, polarization or partisanship, defensive pessimism, wishful thinking (e.g., Krizan and Windschitl 2007), and lay theories (Gershoff et al. 2007). If people vary in these factors, we might expect a correlation between own evaluations and estimates of others'. These accounts, however, would predict own evaluations should generally correlate with estimates of others' evaluations of any of the options, whereas we propose (and find) the corresponding evaluations are projected.

Furthermore, in Study 3, shifts in beliefs across conditions are attributable only to the experimental manipulation of evaluations and cannot be explained by these causes. Projection of changes in evaluations (Studies 1b/c and 4) also rules out potential alternative explanations based on omitted person-specific biases or beliefs. Calculating within-person difference scores in the analyses relating *changes* in evaluation to *changes* in estimates eliminates these factors.

Projection of polarization. A more nuanced version of the above argument is that our findings are explained by projection of polarization, such that when people become more polarized in their own evaluations, their estimates also become more polarized. Even though our data are consistent with this prediction, our proposed process provides a more specific prediction of which of the beliefs regarding same and opposite choosers will respond to differences in evaluations. The data support these specific predictions. Thus, although our account would

predict a projection of polarization effect as a byproduct, a general account of projecting polarization would not generate our specific predictions.

Reverse causality. The potential for reverse causality (own views being influenced by knowledge of or beliefs about others' views) is an understudied issue in the literature on social projection, which has predominantly relied on correlational analyses. In a pre-test for Study 2, we found that manipulating participants' beliefs about opposite-choosers' evaluations did not influence their own evaluations of the posters we used (Web Appendix). Moreover, we manipulated participants' own evaluations, both between subjects (Study 3) and within subjects over time (Study 4), and found the predicted corresponding change in estimates.

Implications for Theories of Social Projection.

This paper is the first to investigate how people estimate the choice-option evaluations of others whose choices are known. We introduce a novel account, conditional projection, which predicts evaluations of the chosen item will be projected onto others' evaluations of their chosen item, for both same- and opposite-choosers. Similarly, this account predicts the evaluations of the rejected item will be projected onto others' evaluations of their rejected item, regardless of the option chosen.

Our account of conditional projection shares the intuition of general theories of projection, which posit the self serves as a basis for forming judgments about others (Allport 1924). Studying beliefs about other's evaluations when their choices are known, however, enables us to identify important distinctions that cannot be addressed by studying projection of choices and binary endorsements. Prior research has concluded people generally do not project onto dissimilar others, because the self is judged to be irrelevant for making sense of dissimilar

out-groups (Krueger 2000). We introduce a critical distinction between two ways people may relate themselves to others whose choices are known: literal equivalence in evaluations of the same option or an analogical relationship between one's own evaluation of an option and others' evaluation of the corresponding option. Although we agree projection may rely on an assessment of whether one's own information is relevant, departing from the previous conclusions, we contend that surface dissimilarity to others does not necessarily make them seem irrelevant. Rather, people may project onto even dissimilar others if the context prompts an analogical correspondence between themselves and others. Supporting the conditional-projection account, we find highly robust projection onto opposite-choosers. The degree of projection onto opposite-choosers is generally lower than it is for the same-choosers, suggesting that perhaps the required analogical correspondence may be less readily accessible for others with dissimilar choices. These results suggest that understanding how people may relate to others is essential for broadening general accounts of social projection. We hope our findings will spur future research in the area of the implications of analogical inference for social projection.

We have refrained from relating our findings to the prominent and lively debate over normativity of projection. Hoch (1987) and Dawes (1989) have argued "false consensus" is not really "false;"— that normative models of inference can predict choice-projection findings. Krueger and Clement (1994), on the other hand, have presented evidence of falsity that they argue satisfies Dawes' criterion. Addressing normativity in the projection of evaluations requires eliciting expectations of joint and marginal distributions of others' evaluations, and a fundamentally different analysis that is beyond the scope of this paper. Therefore, we stress that our current results neither rule out nor support the contention that projection is a bias.

We have distinguished our findings from one particular extension of the induction account to

our context, based on the common assumption that surface similarity determines perceived relevance. Under this assumption, one's own evaluation of an option will (and should) have a positive, but weakened, effect for estimates of opposite-choosers' evaluations of that *same* option. Dawes (1989, p. 9) argues, in the context of choice projection, "If I believe that I am liberal but the group as a whole is conservative, then … the weight given my own response considered in isolation should decrease relative to the weight given it without considering such a variable [ideology]." In contrast, we find that a person's evaluation of an option consistently influences her estimate of opposite-choosers' evaluations of the *other* option, rather than her estimates of the evaluation of the same option.

That being said, Dawes's critique could be generalized to the intuition that people should use their own information to assess how it could be useful for the estimation task. Our account can then be seen as a way to distinguish between the different ways people might make such an assessment. Rather than merely relying on surface similarity, we argue that people incorporate what they know about others to draw parallels between themselves and others, and thereby conclude their own evaluations are relevant for estimating others' corresponding evaluations. The consideration of a more sophisticated reasoning, by which people map their own evaluations to the corresponding evaluations of others, thereby potentially facilitating a different kind of induction, is precisely the conceptual contribution of our paper that distinguishes it from the prior literature.

Implications for Consumer Decision-Making.

In this paper, we concentrate on the evaluations underlying choices between two options across a range of consumer decision contexts (posters, cameras, postcards, and video game consoles; see the Web Appendix), in order to test conditional projection for estimates of both

chosen and rejected options. The conditional projection account could also be generalized more broadly to choices among multiple options. We would expect estimates of others' evaluations of their chosen option to be influenced by the evaluations of one's chosen option, when making estimates for either same-choosers or opposite-choosers. However, estimates of others' rejected options may not be influenced by one's own evaluations, particularly when making estimates for opposite-choosers, when there are multiple rejected options. Moreover, in such settings, the distinction between knowing that someone has chosen a different option than one's own choice (which might inhibit conditional projection) and knowing which other option the person has chosen (which would enable drawing a correspondence between that person and oneself, facilitating conditional projection) may be important.

We believe our results have implications for many common decision situations in which peoples' decisions are influenced by how others will make a decision or react to one's own choices. For example, consider how a person's own evaluations might influence the choice of a gift for another person. If the recipient is someone who is known to share one's tastes, one's own evaluations will affect the choice. However, our results suggest own evaluations may play a role even when purchasing a gift for someone known to have different tastes (e.g., whose observed choices differ from one's own), as long as identifying a correspondence between one's own evaluations and those of the other person is possible. Similarly, people may use themselves as a basis for thinking about both similar and different others when making product purchase decisions for which network externalities are present or stock-outs are likely, when bidding in an auction, and when bargaining with a buyer or investing in a public good. Exploring how conditional projection can influence decisions in such contexts is an important direction for future research.

Not accounting for such effects can also lead to misinterpreting the causes of people's choices. Currently, the notion that one's own evaluations systematically impact beliefs about others contradicts an assumption widely invoked in formal models: mutually consistency of beliefs. Such models may overestimate the effect of own preferences and underestimate strategic effects, when not accounting for projection. We hope this work will also facilitate inclusion of these systematic influences on beliefs into how we structure and interpret models of strategic decision-making.

Our findings highlight the pervasive influence of our own perspectives on how we make sense of others. Even if others' choices are different from ours, we continue to see others as broadly similar to ourselves, based on how their views correspond to ours. Ultimately, our understanding of ourselves fundamentally shapes the way we perceive people around us.

Figure 1. Possible influence patterns of own evaluations of A and B on the estimates of opposite-choosers' evaluations, E(A) and E(B)

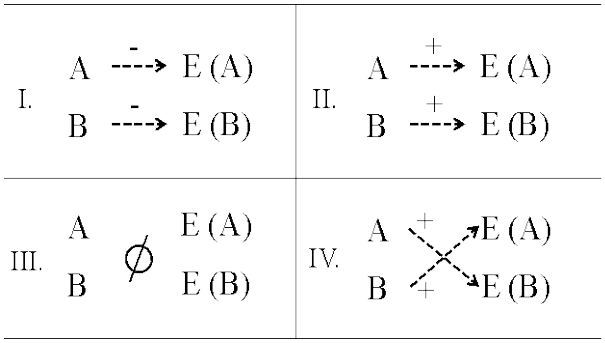

Figure 2. Pattern of Choice Projection

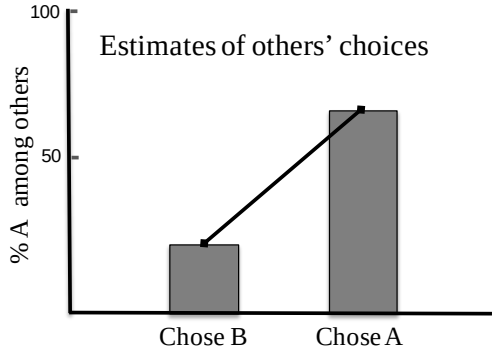

Figure 3. Projection onto out-group: Different predicted estimates for choice of A.

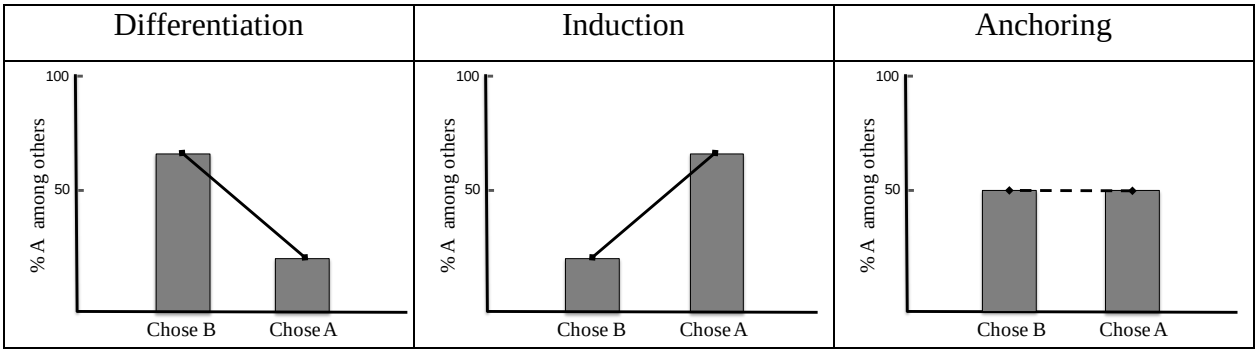

Figure 4. Projection of evaluations onto opposite-choosers by A-choosers.

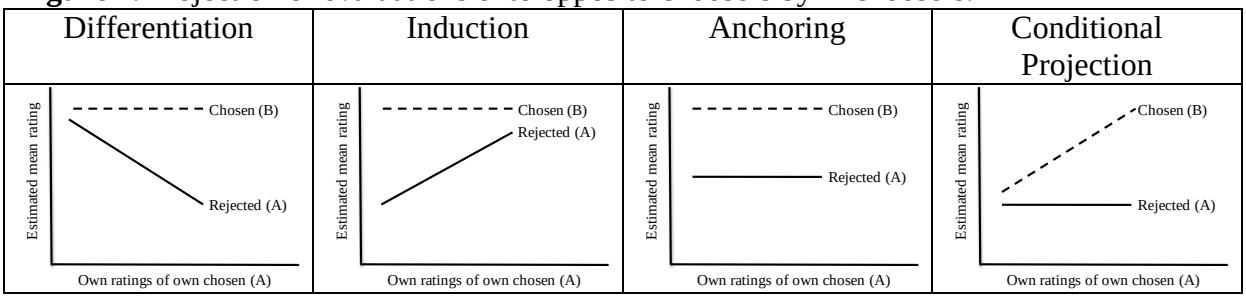

Table 1. Predicting estimated average evaluations for same-choosers.

Target group: Same-choosers.	Mean estimated rating for *their chosen* option			
Variables	Study1a Lab voting	Study 1b Pre-debate	Study 1c Post-debate	Study 2 Posters
Own rating of one's chosen option	.173** (.067)	.495*** (.031)	.538*** (.047)	.377*** (.078)
Own rating of one's rejected option	-.024 (.047)	.046 (.028)	.051 (.045)	-.011 (.054)
Constant	6.476*** (.513)	4.159*** (.247)	3.728*** (.384)	3.868*** (.388)
Target group: Same-choosers.	Mean estimated rating for *their rejected* option			
	Study1a	Study 1b	Study 1c	Study 2
Own rating of one's chosen option	-.022 (.084)	.074 (.055)	-.04 (.065)	-.128 (.124)
Own rating of one's rejected option	.137** (.06)	.572*** (.051)	.676*** (.061)	.666*** (.087)
Constant	2.382*** (.645)	1.138** (.448)	1.253** (.529)	1.864*** (.619)
Number of observations	72	351	153	123

*** significant at p < .01, ** significant at p < .05, * significant at p < .1. Standard errors are in parentheses.

Table 2. Predicting average evaluations estimated for opposite-choosers.

Target group: Opposite-choosers.	Mean estimated rating for *their chosen* option			
Variables	Study1a Lab voting	Study 1b Pre-debate	Study 1c Post-debate	Study 2 Posters
Own rating of one's chosen option	.170* (.091)	.161*** (.049)	.408*** (.06)	.458*** (.079)
Own rating of one's rejected option	.084 (.065)	.174*** (.046)	.065 (.057)	.045 (.056)
Constant	5.424*** (.700)	5.837*** (.400)	4.208*** (.488)	3.163*** (.397)
Target group: Opposite-choosers.	Mean estimated rating for *their rejected* option			
Variables	Study1a	Study 1b	Study 1c	Study 2
Own rating of one's chosen option	-.14 (.113)	-.06 (.053)	.082 (.076)	.077 (.137)
Own rating of one's rejected option	.240*** (.080)	.393*** (.049)	.396*** (.071)	.515*** (.096)
Constant	3.416*** (.865)	2.884*** (.430)	1.597** (.616)	1.297*** (.682)
Number of observations	72	351	153	123

*** significant at p < .01, ** significant at p < .05, * significant at p < .1. Standard errors are in parentheses.

Table 3. Study 1b and 1c. Predicting change in estimated candidate evaluations of same- and opposite-choosers.

	Change in the estimated mean rating for their chosen candidate		Change in the estimated mean rating for their rejected candidate	
Variables	Same-choosers	Opposite-choosers	Same-choosers	Opposite-choosers
Change in own rating of	.256***	.259**	.164	.16
one's candidate of choice	(.095)	(.112)	(.148)	(.178)
Change in own rating of	-.065	-.091	.321***	.130
one's rejected candidate	(.068)	(.08)	(.106)	(.127)
Constant	.098	.282**	.227	.068
	(.118)	(.139)	(.183)	(.220)
Number of observations	153	153	153	153

*** significant at p < .01, ** significant at p < .05. Standard errors are in parentheses.

Table 4. Study 4. Predicting change in estimated camera evaluations of same- and opposite-choosers.

	Change in the estimated mean rating for their chosen camera		Change in the estimated mean rating for their rejected camera	
Variables	Same-choosers	Opposite-choosers	Same-choosers	Opposite-choosers
Change in own rating of	.186***	.158**	-.023	.146*
one's preferred camera	(.055)	(.062)	(.079)	(.083)
Change in own rating of	.002	-.024	.331***	.264***
one's rejected camera	(.045)	(.050)	(.065)	(.068)
Constant	-.063*	-.062	-.197***	-.151***
	(.036)	(.041)	(.052)	(.055)
Number of observations	274	274	274	274

*** significant at p < .01, ** significant at p < .05, * significant at p < .1. Standard errors are in parentheses.

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"Conditional Projection: How Own Evaluations Impact Beliefs About Others Whose Choices Are Known"

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WEB APPENDIX

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SUPPLEMENTAL STUDIES

Study A1: Consumers' Projection of Videogame Console Evaluations

In this study, we investigate the views of a national online sample of videogame players about videogame consoles, specifically, the Nintendo Wii and the Sony Playstation 3 (PS3). Unlike the political context, evaluations for choice options are not negatively correlated in this domain. Data from an industry survey conducted several months before our study shows favorability ratings of the Wii and PS3 were weakly positively correlated, $r = .14$, $p < .01$, N=1646).[1] In addition, to elicit people's beliefs about correlation in evaluations, we conducted a pre-test with 69 videogame players. We find no difference in their estimates of how Nintendo-likers (rating the Nintendo Wii a 7 on a 9-point scale) or Nintendo-haters (rating it a 3 on a 9-point scale) evaluated the Sony PS3 (M = 3.9 vs. 3.8, respectively; paired-$t = .64$, $p > .1$), suggesting participants in the pre-test believed evaluations of the two consoles were uncorrelated. Testing for our predictions in this domain provides further generalization of our findings to contexts in which subjects do not believe evaluations of two options are negatively correlated.

In the main study, we analyze the beliefs and evaluations of a national online sample of 159 videogame players, between 18 and 65 years old.[2] To measure beliefs, we asked participants to estimate the proportion of survey respondents who would pick either the Nintendo Wii or the Sony PS3 console, when choosing between the two. They estimated the average ratings for each console among the respondents, on a 9-point favorability scale. Next, participants chose between the two consoles and rated each on the scale. In line with the beliefs elicited in the pre-test, the participants' ratings of the two consoles were not correlated ($r = -.04$, $p > .1$). We next asked participants to think about respondents who had chosen the Wii and then estimate the average ratings of each console among this group. They repeated the exercise for estimates of ratings among PS3 supporters.

Finally, participants filled out informational questions about video gaming and some demographic items. In particular, we asked people to rate their knowledge about videogames and the degree to which videogames were important to them personally. In addition, to objectively assess participants' knowledge of the category, we asked them five factual questions, in which they identified which of the two consoles a console-specific attribute applied to (three product features and two game releases). We computed each participant's knowledge score based on the total number of factual questions he or she answered correctly.

We replicate choice projection, even when beliefs about others' choices were elicited before the participants made their own evaluations of the consoles. As Table A2 shows, PS3-choosers estimate a higher percentage of people choosing PS3 ($\beta = 15.9$, $p < .01$) than do others who prefer the Wii. The replication of false consensus results, despite eliciting beliefs first, addresses a potential alternative account based on response-scale anchoring and adjustment (Biernat et al.

[1] An anonymous market research firm that consults for the videogame industry provided the data.
[2] Of 198 completed surveys, we excluded respondents whose ratings were inconsistent with their choice (1%), or who demonstrated a misunderstanding of instructions, that is, estimates that were inconsistent with the definition of subgroups (19%).

1997). In contrast to their predictions regarding choice projection, we observe assimilation to the self regardless of elicitation order.[3] As in Study 1b, we also provide evidence for projection of candidate ratings over and above choice projection. As Table A3 shows, participants' own differences in ratings of the videogame consoles influence their beliefs about others' choices (β = 1.88, $p < .05$), controlling for the effect of their own choices (β = 8.64, $p < .05$).

Returning to our primary research question, we examine beliefs about others whose console choices are known, either the same-choosers (who favor the same console) or the opposite-choosers (who favor the other console). We conduct multiple regressions with the participants' estimates of the average ratings among same-choosers and opposite-choosers for both videogame consoles as the dependent variables, predicted by the participants' own ratings of the consoles. As in the studies reported in the paper, we find a direct relation between one's own evaluations of each console and the estimated average ratings of same-choosers (Table A7). Estimates of same-choosers' ratings of the chosen console are predicted by ratings of own chosen console (β = .38, $p < .01$) and not by ratings of own rejected console (β = -.07, $p > .1$). Similarly, estimates of same-choosers' ratings of the rejected console are predicted by ratings of own rejected console (β = .47, $p < .01$) and not by ratings of own chosen console (β = -.02, $p > .1$).

Likewise, consumers' own evaluations underlying their decisions impact their beliefs about the corresponding ratings of opposite-choosers (Table A8). Estimates of opposite-choosers' ratings of their chosen console are predicted by ratings of the participants' own chosen console (β = .15, $p < .01$), as well as, in this case, the ratings of the participants' rejected console, which is the same as the opposite-choosers' chosen console (β = .14, $p < .01$). Furthermore, estimates of opposite-choosers' ratings of their rejected console are predicted by ratings of participants' own rejected console (β = .32, $p < .01$) and not by ratings of the participants' own chosen console (β = .03, $p > .1$).

We collected additional measures to help us address a potential concern that less involved participants might use the console rating scales more conservatively, both when expressing their own evaluations and when estimating their beliefs about others'. If this were the case, differences in overall category involvement could contribute to our findings. However, we find no evidence for this possibility. Relative preference for one's chosen console (i.e., difference in evaluations between chosen and rejected options) does not correlate with videogame importance, category knowledge, or quiz score (all r's < .05, $p > .1$). Additionally, all of our results replicate when we control for importance, self-reported knowledge, and quiz score. We also check whether uninformed participants projected their evaluations more onto others. We find self-reported knowledge or quiz score do not moderate any of the effects reported above.

Study A2: Projection of Poster Evaluations

This study employs the same stimuli as Study 2 in the paper. First, we report a pre-test, conducted to test the potential for reverse causality, in which participants base their own evaluations on their beliefs about others' evaluations.

Pre-test for direction of causality. Online participants (N=88) were shown the same two posters

[3] We thank an anonymous reviewer for pointing out this comparison.

as in Study 2 and chose the one they preferred. Then each participant saw the ratings of both posters among a subset of participants from a previous study who had chosen the other poster. In a between-subjects conjoint design, we told participants the average rating on a 7-point scale of the opposite-choosers' preferred poster was high (7) versus moderate (5), and that the rating of the poster opposite-choosers' rejected poster was either one point or three points lower. Thus participants learned the ratings were 7 and 6, 7 and 4, 5 and 4, or 5 and 2 among opposite-choosers for their preferred and rejected poster, respectively. After seeing this information, participants rated both posters and then estimated the rating of a typical opposite-chooser. Participants gave significantly higher estimates for opposite-choosers' ratings of each poster when they were shown higher ratings for that poster in a sample of previous opposite-chooser respondents. This result confirms that the participants read, understood, and believed the manipulated information.

The information manipulation did not, however, affect the participants' own ratings. To test for an effect, we ran two multiple regressions on the full sample, predicting participants' own ratings of each of the posters based on the information we gave them about opposite-choosers' ratings. Participants' ratings of their chosen poster are not significantly affected by the provided opposite-choosers' ratings of either poster (both p's > .1). Likewise, participants' ratings of their rejected poster are not significantly affected by the provided opposite-choosers' ratings of either poster (both p's > .1). This result suggests that in the context of choices for these posters, participants' own evaluations are unlikely to be influenced by their beliefs about how opposite-choosers rated the same posters.

Study design. To further ensure order effects do not contribute to our findings, the study replicated Study 2 in the paper using a between-subjects design in which we randomly assigned participants to two groups; one group estimated only the same-choosers' evaluations, the other group estimated only the opposite-choosers' evaluations. Participants chose between the two posters used in the pre-test and next rated both on a 7-point scale. Then they estimated the ratings of both posters for a typical respondent in their target group (same-chooser or opposite-chooser, by random assignment). We prompted participants who made mistakes (rating a poster higher among a group defined by choosing the other poster) to correct them (12 out of 155 completed surveys, or 8%). As a result, we obtained usable data for 151 participants (79 in the same-choosers condition and 72 in the opposite-choosers condition).[4] Participants also completed two measures of beliefs about correlation of peoples' ratings. We find no consistent moderation of our results by beliefs about correlations, as discussed in 2.2 below.

Results. We find the same pattern for projection as in our other studies for participants who estimated same-choosers' evaluations (Table A7). Estimates of same-choosers' ratings of the chosen poster are predicted by ratings of own chosen poster ($\beta = .40$, $p < .01$) and not by ratings of own rejected poster ($\beta = .08$, $p > .1$). Likewise, estimates of same-choosers' ratings of the rejected poster are predicted by ratings of own rejected poster ($\beta = .24$, $p < .05$) and not by ratings of own chosen poster ($\beta = .23$, $p > .1$).

[4] Four participants (less than 3% of the total) repeated their errors when asked to correct their estimates, and were therefore excluded. We also checked whether excluding the respondents who initially made a mistake but then corrected their answers changed our results, and did not find any such effect, which speaks to the validity of such exclusions in Studies 1 and A1.

More importantly, among participants who were randomly assigned to estimate opposite-choosers' evaluations, we likewise find the same pattern of influence of own ratings (Table A8). Estimates of opposite-choosers' ratings of their chosen poster are predicted by ratings of the participants' own chosen poster (β = .32, p < .01), as well as, in this case, marginally by ratings of the participants' rejected poster (β = .11, p =.10). Likewise, estimates of opposite-choosers' ratings of their rejected poster are predicted by ratings of participants' own rejected poster (β = .25, p = .05) and not by ratings of the participants' own chosen poster (β = .11, p > .1).

Study A3: The Projection of Manipulated Evaluations

Study Design. In this study, we aimed to manipulate evaluations of the chosen and rejected options. A total of 65 adult consumers successfully completed an online survey about purchasing digital cameras. We randomly assigned participants to either the *missing information unaware* or the *missing information aware* condition, and were presented with the corresponding information grid for two cameras, the Sony Cybershot and the Panasonic Lumix. In the *unaware* condition, participants saw a picture and the brand name of each camera, priced identically ($120 for each). In the *aware* condition, participants saw the same information, but the grid also included 19 blank rows, labeled with other product attributes (e.g., Optical Sensor Resolution, Lens Type, Weight), and they were told, "For now, only some of the product information is provided below. You will see the additional information about the cameras at the end of the survey." Thus, in the *aware* condition, the fact that they had limited information with which to make their choices was made salient to the participants, although the actual information available to them did not vary across conditions. Our expectation was that manipulating awareness of the missing information would reduce the participants' evaluations of their chosen camera, without necessarily affecting which one they chose.

After choosing, participants rated on a scale of 1 ("Do not like at all") to 7 ("Like very much") their liking of both cameras. Then they estimated the ratings of the cameras among both Sony- and Panasonic-choosers, and were prompted to correct their mistakes (e.g., when they estimated higher ratings for the non-chosen camera than the chosen one for a group).[5] They completed measures of similarity to both same- and opposite-choosers, indicated their belief about the correlation of option evaluations among others, stated how they thought opposite-choosers formed their evaluations, and rated their own knowledge of digital cameras.

Manipulation Results. The majority of participants (84%) chose the Sony camera, and the choices did not differ significantly by condition (89% among unaware, N=35; 79% among aware, N=29, difference insignificant; χ^2 = 1.0, p > .1). Furthermore, participants' ratings of their chosen option were significantly higher in the unaware condition, compared to the aware condition, (M = 6.0 vs. 5.3, t = 2.99, p < .01). However, ratings for the rejected option did not differ across the two conditions (M = 4.2 vs. 3.9, t =1.0, p = .32).

According to our account, because ratings of the chosen option are higher in the unaware condition, we expect the estimates of both same- and opposite-choosers' ratings of their respective chosen options to be higher in this condition. On the other hand, because ratings of the

[5] One participant did not correct an error after being prompted, and was excluded from further analyses.

rejected option are not affected by the manipulation, we expect the estimates of both same- and opposite-choosers' ratings of their respective rejected options to not differ across conditions. Indeed, participants' estimates of same-choosers' evaluations of their chosen camera are marginally higher in the unaware (vs. aware) condition (M = 6.2. vs. 5.9, $t = 1.74$, $p < .10$), but no differences are present in the estimates of their evaluations of the rejected camera (M = 3.8 vs. 3.9, $t = .39$, $p > .1$). Furthermore, estimated opposite-choosers' evaluations of their own chosen camera are higher in the unaware (vs. aware) condition (M = 6.1. vs. 5.6, $t = 2.32$, $p < .05$), but no differences are present in the estimates of their evaluations of the rejected camera (M = 4.0 vs. 4.1, $t = .23$, $p > .1$).

To clarify this finding, consider the Sony-choosers. When the missing information was made salient to them (in the *aware* condition), they rated their chosen camera (Sony) lower than in the *unaware* condition. They also provided lower estimates for how Panasonic-choosers would rate the Panasonic camera, but gave nearly identical estimates of how Panasonic-choosers would rate the Sony camera.

Comments. Two comments are in order. First, we note that although we find a shift in evaluations and the corresponding change in beliefs about same and opposite choosers in the *aware* condition and not the *unaware* condition, the effect of the *aware* versus *unaware* conditions on the difference score (chosen – rejected) was directional but not significant (Ms = 1.80 vs. 1.38, t = 1.33, p = .19 for own ratings; Ms = 2.40 vs. 1.93, t = 1.36, $p = .18$ for estimates of same-choosers; Ms = 2.06 vs. 1.52, t = 1.77, $p = .08$ for estimates of opposite-choosers).

Second, we note a potential concern in the interpretation of these results. One could argue that when participants were aware of the missing information, they might have become more uncertain, and they may have inferred that other participants would also be uncertain in the same situation. We tested this possibility in a separate post-test (N=62) by having participants choose and rate cameras in the two experimental conditions. Then instead of estimating the evaluations of others, participants estimated how certain or uncertain they thought same- and opposite-choosers would be about their own preferences between the two cameras. Rated certainty was nearly identical in the *aware* and *unaware* conditions for same- (Ms = 4.51 vs. 4.39, $p = .78$) as well as for opposite-choosers (Ms = 4.94 vs. 4.87, $p = .82$). Thus beliefs about others' certainty do not seem to account for our findings. Note that Study 3 in the paper deals with this potential issue directly, by having people estimate the ratings of others who have had the same experience, in both conditions.

Additional Results. For comparison to our prior studies, we collapse the two conditions (which combines manipulated and spontaneously occurring variation) and replicate the correlational analyses (Tables A7 and A8). An observer projects her own option evaluation when estimating the same evaluation for same-choosers (chosen: $\beta = .49$, $p < .01$, rejected: $\beta = .78$, $p < .01$). Importantly, she projects her evaluation of the corresponding option to opposite-choosers (chosen: $\beta = .45$, $p < .01$, rejected: $\beta = .63$, $p < .01$), while she does not project her evaluation of the same option.

Beliefs about correlations in others' evaluations do not moderate our findings, as detailed in the next section. We also note that self-rated knowledge about cameras does not differ between the *unaware* and *aware* manipulated conditions (M = 2.7 vs. 2.6, $t = -.42$, $p > .1$) and it does not moderate the effect of the manipulation on estimates (interactions $\beta = .10$, $p > .1$ for chosen; $\beta =$

-.05, $p > .1$ for rejected).

Lastly, we note that (as in Study 2 in the paper) a strong majority (74%) of the participants endorsed a description of opposite-choosers as analogous ("*Their preferences are probably analogous to mine: they feel about the camera they chose the way I feel about the one I chose*"), as opposed to reflecting either (1) poor decisions, (2) decisions unrelated to one's own process, or (3) a process of direct contrast with one's own evaluations. This endorsement lends additional support to our argument that most of the participants thought about the evaluations of opposite-choosers in analogous terms, recognizing a parallel between their own evaluations and the corresponding evaluations of others.

TECHNICAL DISCUSSION

In the next section, we discuss several technical issues that we discussed only briefly in the main paper due to space constraints.

Order of elicitation.

In Study 1a, Study 2, and Study 3, we elicited opposite-choosers' beliefs first, whereas in Study 1b and Study 4, we elicited same-choosers beliefs first. Our results replicate across both elicitation orders.

In addition, two studies allow us to provide direct evidence for the lack or order effects. First, we note that in Study 1c the order was counter-balanced. Thus some voters estimated evaluations among same-choosers first, whereas other voters estimated evaluations among opposite-choosers first. This variation allows us to test whether generating estimates for one group changes the projection patterns in the subsequent estimation for the other group. Our results for opposite-choosers are fully replicated and remain statistically significant when we restrict our analyses to only those who made these estimates first. We also re-ran the regressions in Tables 1 and 2 in the paper, adding the main effect of order and the interactions between order and own ratings, and found no significant moderation of projection for beliefs about either opposite- or same-choosers. Second, in Study A2, we randomly assigned participants to estimate only same-choosers or opposite choosers. We also replicate our findings in this study, which provides additional evidence that order effects do not contribute to our results.

In most of the studies, we ask participants to choose and rate the options first, before estimating others. We do so primarily to facilitate understanding of the options and the rating scale, and to make the estimating task more intuitive. However, this task is by no means necessary for the effect—in Study A1, participants first made their estimates and then rated choice options. We show that order of elicitation does not impact projection. The previous literature on egocentrism suggests the self comes to mind automatically when trying to make sense of others. Therefore, the lack of order effects was not surprising.

Beliefs about correlations in others' evaluations.

All studies reported below asked subjects to endorse their choice of one of the following

statements regarding other participants:

1. Most people liked only one poster and disliked the other, and few people liked both or disliked both.
2. People who liked one poster were not more or less likely to also like the other poster.
3. Most people either liked both posters or disliked both posters, and few people liked one and disliked the other.

These statements were coded as indicating a belief in negatively correlated evaluations ("negcor"), uncorrelated evaluations, and positively correlated evaluations ("poscor"), respectively.

Tables A9–A17 report results from regressions we ran to test for potential moderation of our main findings by these beliefs. Beliefs in uncorrelated evaluations are taken as the base to which any impact of beliefs in negative or positive correlations are compared. We find no consistent pattern of moderation across five separate studies. We detail the results from each study below.

Study 2. Participants' ratings of the posters were weakly negatively correlated ($r = -.15$, $p < .1$). Participants' beliefs about correlations were mixed: 33% indicated a belief in negative correlations, whereas the majority indicated either no correlation (34%) or a positive correlation (33%).

We conducted several analyses to validate this measure and confirm that people understood it. First, those who endorsed a belief in negative correlations were less likely to characterize others' evaluations of the posters as similar in describing how they made their estimates ($r = -.23$, $p < .05$ same-choosers; $r = -.21$, $p < .05$ opposite choosers). Likewise, those who endorsed a belief in negative correlations were more likely to describe others' evaluations of the posters as polarized ($r = .20$, $p < .05$ same-choosers; $r = .17$, $p < .10$ opposite-choosers).

Next, we note a significant relationship between belief in negative correlations and the difference in estimates of chosen and rejected posters. People who believe others' evaluations are negatively correlated report estimates of evaluations that are more polarized as measured by the difference between chosen and rejected (Ms = 2.93 vs. 1.89, $t = 5.09$, $p < .01$ same-choosers; Ms = 2.83 vs. 1.90, $t = 4.23$, $p < .01$ opposite-choosers). Importantly, these people also have more polarized evaluations themselves (Ms = 2.45 vs. 1.58, $t = 5.03$, $p < .01$), as the previous literature (Critcher and Dunning, 2009) has documented.

For a person who believes in negative (vs. positive) correlations, we should also see a negative (vs. positive) correlation across that person's estimates for different pairs of items. Given that we only have one pair of items in our study, as a rough approximation, we calculated the correlations across participants' estimates for two posters, separately for the negative-correlation believers, no-correlation believers, and positive-correlation believers. We find a significantly stronger negative correlation among those who believed in negative correlations, in their estimates of the two posters' evaluations among same-choosers ($F(1,119)=4.04$, $p < .05$) and among opposite-choosers ($F(1,119) = 7.44$, $p < .01$).

These analyses suggest participants understood the measure of belief in correlations, as validated by confirming the predicted relationships to other measures. Therefore, we can use this measure

to test whether belief in negative versus positive correlations moderates our projection findings. Table A9 depicts the results from four regressions predicting the estimated evaluations of the two posters among same- and opposite-choosers. Independent variables include own evaluations of each poster, beliefs about correlation of others' evaluations, and the interactions between the two sets of variables. We find no significant interactions (all p's > .1).

We also replicated this analysis, using the variables coded from participants' descriptions of how they made their estimates (Table A10). "Similar" was coded as 1 if the participant's description implied others would see the two posters as similar, and 0 otherwise. "Polarized" was coded as 1 if the participant's description implied others' views of the two posters would be polarized. We find no significant interactions between the coded variables and own evaluations, suggesting that seeing the sub-group being estimated as polarized or not did not affect conditional projection to either same- or opposite-choosers (all p's > .1).

Study 3. The actual correlation in participants' ratings of the two postcards was nonnegative (r = .14, p > .1). Most of the participants endorsed a nonnegative belief, and the manipulation did not affect their beliefs about the correlation (no-reasons condition: 16% negative, 43% uncorrelated, 41% positive vs. reasons condition: 20% negative, 42% uncorrelated, 39% positive; χ^2 = .27, p > .1).

We ran two regressions that predicted the difference in estimates of chosen and rejected among same- and opposite-choosers separately based on the experimental condition, indicator variables for belief in negative and positive correlation, and the interactions of both indicator variables with experimental condition (see Table A11). We find that beliefs about the correlation between others' evaluations do not moderate the effect of the experimental manipulation on people's estimates of either opposite-choosers' or same-choosers' evaluations. We conclude that our manipulated results in Study 3 are not consistently accounted for by any belief in correlated evaluations.

We also run four regression analyses, as in Study 2, to test for moderation of the correlational findings. For the estimates of same-choosers, we find no effects of belief in either positive or negative correlations (all p's > .1, see Table A12). We find the participants who believe in positively correlated evaluations (vs. uncorrelated or negatively correlated) differed in their projection onto opposite-choosers. Those who believed in positive correlations had stronger projection of their own chosen evaluation when estimating opposite-choosers evaluations of their chosen option. However, they had weaker projection of their rejected option when estimating opposite-choosers' evaluations of their rejected option. This result does not repeat in other studies and cannot be reconciled with any theory of which we are aware.

Study 4. The actual correlation between ratings in Wave 1 was slightly positive (r = .12, p < .1), but participants' ratings for each camera in Wave 2 were uncorrelated (r = -.08, p > .1). We elicited participants' beliefs about correlation of others' evaluations only in Wave 2. A total of 30% of participants indicated a belief in negative correlations, 31% indicated no correlation and 39% indicated a positive correlation.

We test for the effect of beliefs about the correlation between others' ratings on both the experimental manipulation and the correlational analysis of changes in ratings and beliefs between Wave 1 and 2 (Tables A13 and A14). The effects of the within-subjects manipulation on estimates of others' evaluations are not moderated by a belief in either positive or negative correlations in any of the analyses (all *p*'s > .1). For the effect of changes in own ratings on changes in beliefs over time, we find no moderation by a belief in either negative or positive correlations, except in one case. The change in one's evaluation of one's own preferred option among those who believe in negative correlations is projected more onto estimates of how same-choosers rate this option (β = .24, $p < .1$).

Study A2. The actual correlation between ratings in this study was slightly negative (r = -.18, $p <$ .05). Only 27% of participants indicated a belief in negative correlations, and the majority indicated either no correlation (40%) or a positive correlation (33%).

In two regressions predicting the estimated evaluations of both options among opposite-choosers, we find no moderation of beliefs about correlation of evaluations (all *p's* > .1, Table A15). In two regressions predicting the estimated evaluations of both options among same-choosers, we find moderation in the same direction by both positive- and negative-correlation beliefs (Table A15). Specifically, when estimating same-choosers' ratings of the chosen poster, those who believed ratings were uncorrelated had more projection of own chosen rating compared to positive (β = -.49, $p < .10$) and less of an effect of own rating of the rejected poster (vs. positive-correlation belief: β =.44, $p < .05$; vs. negative-correlation belief: β = .32, $p < .05$). When estimating same-choosers' ratings of the rejected poster, on the other hand, those who believed ratings were uncorrelated had less projection of their rejected poster compared to those who believed in positive (β = .59, $p < .05$) or negative (β = .51, $p < .05$) correlations, and had more of an effect of their of own chosen rating (vs. positive-correlation belief: β = -.88, $p < .05$; vs. negative-correlation belief: β = -.62, $p < .10$). Thus estimates of same-choosers made by people who believed others' ratings are uncorrelated reflected more of the predicted projection of own chosen when estimating the chosen poster, but less of the predicted projection of own rejected when estimating the rejected poster. Thus we conclude none of the lay beliefs about correlations between others' evaluations of the two options consistently explain or contribute to our results in this study.

Study A3. The correlation between evaluations was directionally positive (r = .18, $p > .1$). The manipulation did not affect participants' beliefs about the correlation (*unaware* condition: 37% negative, 34% uncorrelated, 29% positive vs. *aware* condition: 41% negative, 41% uncorrelated, 17% positive; χ^2 = 1.15, p > .1).

We ran four regression analyses to test for moderation of the correlational findings, and found no effects of belief in either positive or negative correlations (all *p*'s > .1, see Table A16). We also ran four regressions that predicted estimates of chosen and rejected among both audiences based on the experimental condition, indicator variables for belief in negative and positive correlation, and the interactions of both indicator variables with experimental condition (see Table A17). We find that beliefs about correlation of evaluations do not moderate the effect of the experimental manipulation, with only one exception. The effect of the manipulation on same-choosers'

estimates of the chosen option is weaker, however, for those who believe others' evaluations are negatively correlated (β = .92, p < .05). We conclude our results are not consistently accounted for by any belief in correlated evaluations.

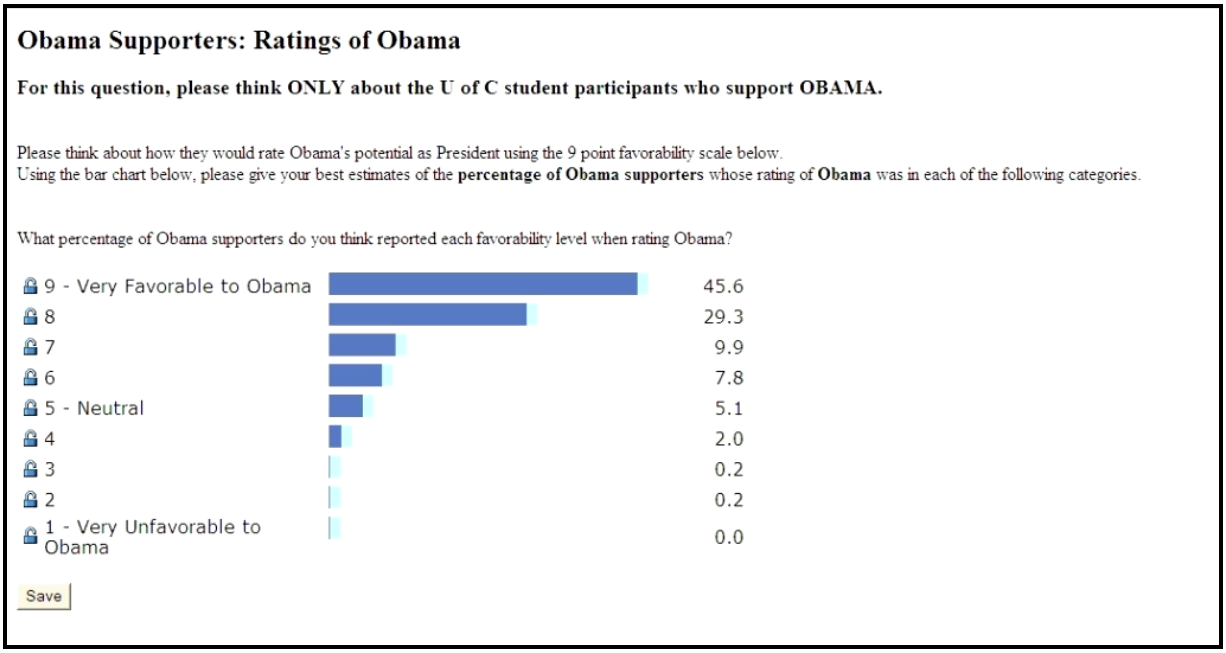
REFERENCES

Biernat, Monica, Melvin Manis and Diane Kobrynowicz (1997) "Simultaneous Assimilation and Contrast Effects in Judgments of Self and Others," *Journal of Personality and Social Psychology*, 73 (2), 254-269.

Critcher, Clayton R. and Dunning, David (2009) "Egocentric Pattern Projection: How Implicit Personality Theories Recapitulate the Geography of the Self" *Journal of Personality and Social Psychology*, 97(1), 1-16

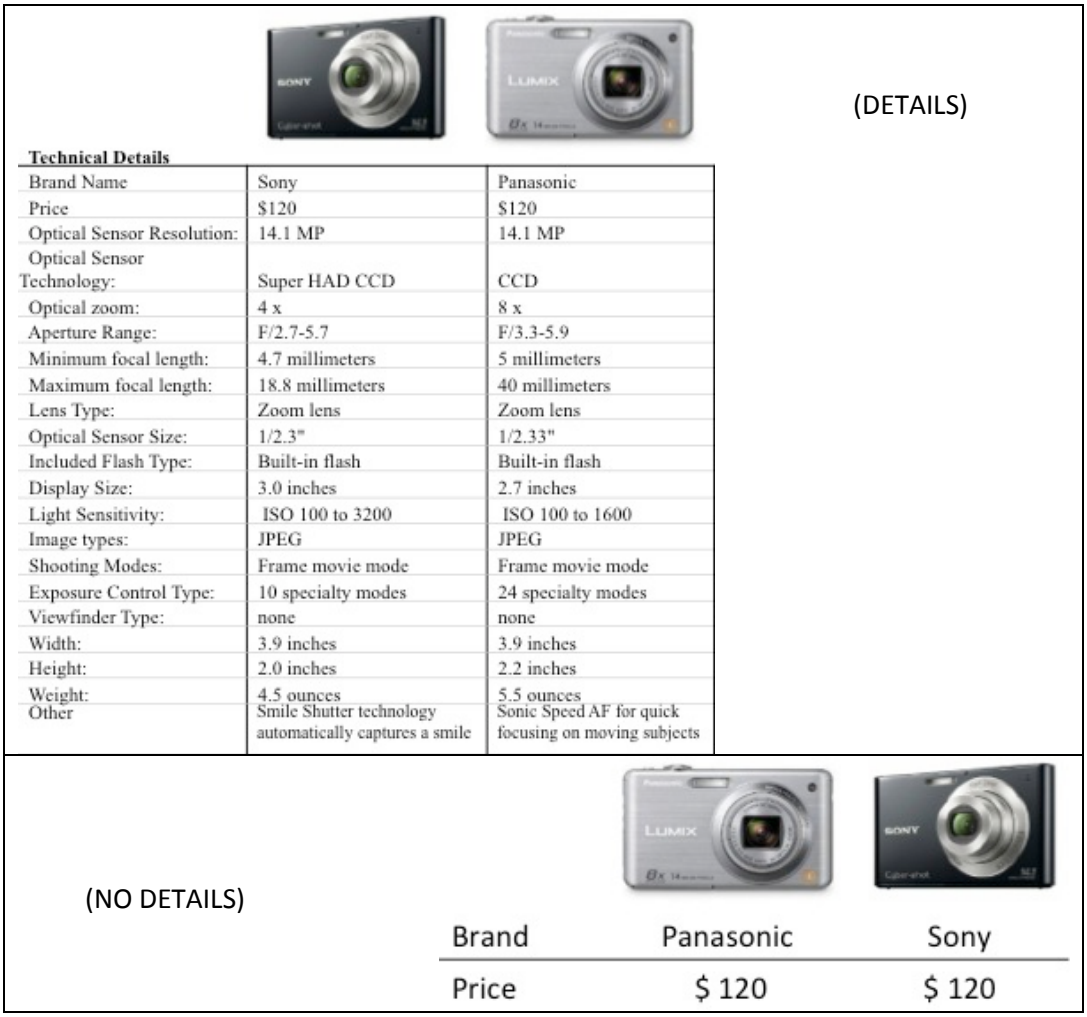
FIGURES

Figure A1. Screen shot of distribution elicitation tool used in Study 1b.



Note: The bar chart tool was custom programmed to automatically adjust the other bars in tandem, to ensure the total added to 100%. Participants could also "lock" bars if they wanted to give a fixed value to a specific bar, and freeze it from further adjustments when they changed the value of other bars. To ensure participants were comfortable using the bar chart tool, they were given a practice task in which they were told the relative popularity of ice cream flavors and asked to adjust the bar chart accordingly.

Figure A2: Cameras and characteristics in Study 4



TABLES

Table A1. Summary Statistics for Study 1

	Study 1a	Study 1b	Study 1c
Percent Female	46% (50)	53% (50)	55% (50)
Percent Voting McCain	0%	52% (50)	50% (50)
McCain rating	3.7 (1.7)	5.2 (3.1)	5.1 (2.9)
Obama Rating	7.7 (1.2)	4.8 (3.6)	5.0 (3.4)
Relative McCain rating (McCain-Obama)	-4.06 (1.8)	0.34 (5.9)	0.04 (5.5)
Observations	72	351	153

Standard errors are in parentheses.

Table A2. Evidence for choice projection to general others: Estimate of % of choice A among others.

	Study 1b	Study A1
Own choice is A	10.55***	15.9***
	(1.34)	(3.05)
Constant	32.48***	21.16***
	(2.15)	(3.83)
Observations	351	159

*** significant at p<.01. Standard errors are in parentheses

Table A3. Projection of evaluations beyond projection of choice to general others

	Study 1b	Study A1
Chose A	.418	8.64**
	(2.38)	(4.28)
Own A rating - B rating	1.03***	1.88**
	(.204)	(.79)
Constant	47.57***	32.53***
	(3.63)	(6.10)
Observations	351	159

*** significant at p<.01, ** significant at p<.05. Standard errors are in parentheses.

Note: We do not report these analyses for Studies 1a and 1c, because Study 1a had only Obama-choosers (i.e., everyone made the same choice) and candidate choices in Study 1c were the same as in Study 1b.

Table A4. Predicting estimated evaluations for typical same-chooser.

Target group: Same-choosers.	Estimated rating for *their chosen* option		
	Study 3	Study 4 (Wave 1)	Study 4 (Wave 2)
Own rating of one's	.538***	.431***	.389***
chosen option	(.081)	(.050)	(.047)
Own rating of one's	-.064	.013	-.002
rejected option	(.066)	(.040)	(.038)
Constant	3.041***	3.545***	3.795***
	(.411)	(.228)	(.242)
Target group: Same-choosers.	Estimated rating for *their rejected* option		
	Study 3	Study 4 (Wave 1)	Study 4 (Wave 2)
Own rating of one's	-.042	-.050	.059
chosen option	(.107)	(.082)	(.068)
Own rating of one's	.734***	.621***	.705***
rejected option	(.087)	(.065)	(.055)
Constant	.916*	1.830***	.663*
	(.540)	(.372)	(.349)
Number of observations	89	274	274

*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A5. Predicting estimated evaluations for typical opposite-chooser.

Target group: Opposite-choosers.	Estimated rating for *their chosen* option		
	Study 3	Study 4 (Wave 1)	Study 4 (Wave 2)
Own rating of one's	.472***	.375***	.353***
chosen option	(.092)	(.052)	(.058)
Own rating of one's	.01	.015	.022
rejected option	(.076)	(.042)	(.047)
Constant	2.994***	3.780***	3.832***
	(.468)	(.237)	(.298)
Target group: Opposite-choosers.	Estimated rating for *their rejected* option		
	Study 3	Study 4 (Wave 1)	Study 4 (Wave 2)
Own rating of one's	.079	.088	.214***
chosen option	(.105)	(.083)	(.072)
Own rating of one's	.502***	.480***	.554***
rejected option	(.086)	(.066)	(.058)
Constant	1.348**	1.745***	.581**
	(.533)	(.379)	(.368)
Number of observations	89	274	274

*** significant at p<.01, ** significant at p<.05. Standard errors are in parentheses.

Table A6. Within-person correlations of responses.

Correlation between	Own ratings (option A, B)	Own ratings (chosen, rejected)	Estimates of same choosers' ratings (chosen, rejected)	Estimates of opposite choosers' ratings (chosen, rejected)
Study 1a*	.22 (.07)	.22 (.07)	-.55 (.00)	-.05 (.69)
Study 1b (wave 1)	-.54 (.00)	.02 (.73)	.06 (.23)	.12 (.03)
Study 1c (wave 2)	-.46 (.00)	.05 (.57)	.06 (.49)	.08 (.35)
Study 2	-.15 (.09)	.58 (.00)	.61 (.00)	.32 (.00)
Study 3	.14 (.18)	.56 (.00)	.43 (.00)	.35 (.00)
Study 4 (wave 1)	.12 (.06)	.63 (.00)	.35 (.00)	.36 (.00)
Study 4 (wave 2)	-.08 (.17)	.50 (.00)	.30 (.00)	.29 (.00)

Numbers in each cell are the correlation and the p-value in parentheses. *Study 1a only included Obama voters.

Table A7. Predicting estimated average evaluations for same-choosers.

Target group: Same-choosers.	Mean estimated rating for *their chosen* option		
Variables	Study A1 Consoles	Study A2 Posters	Study A3 Cameras
Own rating of one's chosen option	.381*** (.065)	.397*** (.095)	.492*** (.080)
Own rating of one's rejected option	-.066 (.051)	.083 (.062)	.015 (.068)
Constant	1.476*** (.223)	3.144*** (.508)	3.191*** (.445)
Target group: Same-choosers.	Mean estimated rating for *their rejected* option		
	Study A1	Study A2	Study A3
Own rating of one's chosen option	-.016 (.121)	.237 (.182)	-.277** (.134)
Own rating of one's rejected option	.472*** (.094)	.241** (.120)	.781*** (.115)
Constant	2.329*** (.413)	1.559 (.979)	2.255*** (.751)
Number of observations	159	79	63

*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A8. Predicting average evaluations estimated for opposite-choosers.

Target group: Opposite-choosers.	Mean estimated rating for *their chosen* option		
Variables	Study A1	Study A2	Study A3
Own rating of one's	.154***	.319***	.446***
chosen option	(.077)	(.087)	(.086)
Own rating of one's	.142***	.108*	.104
rejected option	(.060)	(.064)	(.074)
Constant	1.432***	4.262***	2.919***
	(.263)	(.335)	(.483)
Target group: Opposite-choosers.	Mean estimated rating for *their rejected* option		
Variables	Study A1	Study A2	Study A3
Own rating of one's	.029	.108	-.164
chosen option	(.109)	(.169)	(.147)
Own rating of one's	.322***	.250**	.626***
rejected option	(.085)	(.124)	(.126)
Constant	2.795***	2.198***	2.444***
	(.372)	(.860)	(.822)
Number of observations	159	72	63

*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A9. Moderation by beliefs about others' correlations in ratings (Study 2)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Chosen	.421**	-.178	.473***	.119
	(.164)	(.256)	(.170)	(.275)
Rejected	-.049	.499***	.079	.424**
	(.116)	(.180)	(.119)	(.193)
Poscor	.332	-2.014	.515	-1.504
	(1.007)	(1.567)	(1.040)	(1.685)
Negcor	.160	-.635	.552	1.838
	(.987)	(1.535)	(1.020)	(1.651)
Poscor x Chosen	-.167	.233	-.045	.390
	(.212)	(.330)	(.219)	(.355)
Negcor x Chosen	.013	.011	-.018	-.355
	(.207)	(.322)	(.214)	(.346)
Poscor x Rejected	.186	.177	-.025	-.150
	(.172)	(.267)	(.177)	(.287)
Negcor x Rejected	-.045	.039	-.100	-.070
	(.149)	(.232)	(.154)	(.250)
Constant	3.674***	2.910**	2.859***	1.516

	(.725)	(1.128)	(.749)	(1.213)

N=123 *** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A10. Moderation by open-ended characterization of others' correlations in ratings (Study 2)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Chosen	.316**	-.278	.297	-.015
	(.143)	(.227)	(.280)	(.419)
Rejected	-.028	.869***	.188	.441
	(.127)	(.202)	(.200)	(.298)
Similar	-1.137	-.819	-.698	-1.871
	(1.068)	(1.691)	(1.392)	(2.080)
Polarized	.132	.186	-.256	-1.831
	(.965)	(1.528)	(1.355)	(2.025)
Similar x Chosen	.175	.447	.242	.426
	(.217)	(.343)	(.309)	(.462)
Polarized x Chosen	.039	.168	.103	.040
	(.188)	(.299)	(.303)	(.453)
Similar x Rejected	.049	-.361	-.210	-.178
	(.157)	(.248)	(.219)	(.327)
Polarized x Rejected	-.057	-.285	-.106	.128
	(.158)	(.251)	(.215)	(.322)
Constant	4.203***	1.799	3.623***	2.615
	(.746)	(1.183)	(1.227)	(1.833)

N=123 *** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A11. Moderation of manipulation by beliefs about others' correlations in ratings (Study 3)

	Estimated Chosen - Rejected For Same-Choosers:	Estimated Chosen - Rejected For Opposite-Choosers:
Reasons	.895***	.789***
	(.340)	(.284)
Poscor	-.295	-.310
	(.345)	(.287)
Negcor	1.030**	1.150***
	(0.463)	(0.386)
Poscor x Reasons	-.401	-.254
	(.491)	(.410)
Negcor x Reasons	-.720	-.805
	(.628)	(.524)
Constant	1.684***	1.421***
	(.240)	(.200)
Number of observations: 89		

*** significant at p<.01, ** significant at p<.05. Standard errors are in parentheses.

Table A12. Moderation by beliefs about others' correlations in ratings (Study 3)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Chosen	.482***	.130	.239	.114
	(.148)	(.184)	(.163)	(.170)
Rejected	-.111	.793***	.114	.531**
	(.125)	(.155)	(.137)	(.144)
Poscor	-.969	2.077	-1.546	1.179
	(1.013)	(1.262)	(1.118)	(1.167)
Negcor	.127	1.525	.576	.339
	(1.383)	(1.722)	(1.527)	(1.594)
Poscor x Chosen	.124	-.051	.436**	.179
	(.195)	(.243)	(.215)	(.224)
Negcor x Chosen	-.077	-.405	.114	-.179
	(.239)	(.298)	(.264)	(.276)
Poscor x Rejected	.034	-.348	-.221	-.405**
	(.168)	(.209)	(.185)	(.193)
Negcor x Rejected	.024	.159	.114	.054
	(.213)	(.265)	(.264)	(.245)
Constant	3.648***	-.372	3.895***	.974
	(.836)	(1.041)	(.923)	(.964)
Number of observations: 89				

*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A13. Moderation of manipulation by beliefs about others' correlations in ratings (Study 4)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	Δ their chosen	Δ their rejected	Δ their chosen	Δ their rejected
Baseline change	-.012	-.214**	-.071	-.190*
(i.e. constant)	(0.065)	(0.095)	(0.072)	(0.101)
Poscor	-.110	.027	-.031	-.015
	(0.086)	(0.127)	(0.096)	(0.135)
Negcor	-.048	-.207	.071	-.050
	(0.092)	(0.135)	(0.102)	(0.143)
Number of observations: 274				

** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A14. Moderation of analysis of change in evaluations and change in beliefs by beliefs about others' correlations in ratings (Study 4)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	Δ their chosen	Δ their rejected	Δ their chosen	Δ their rejected
ΔChosen	.106	.052	.189*	.247
	(0.101)	(0.146)	(0.113)	(0.153)
ΔRejected	.062	.392***	.002	.385***
	(0.08)	(0.116)	(0.09)	(0.121)
Poscor	-.107	-.136	-.003	-.010
	(0.086)	(0.138)	(0.107)	(0.145)
Negcor	-.108	-.026	-.020	-.070
	(0.096)	(0.124)	(0.096)	(0.13)
Poscor x ΔChosen	-.045	-.173	.047	-.096
	(0.139)	(0.199)	(0.154)	(0.209)
Negcor x ΔChosen	.238*	-.032	-.174	-.089
	(0.138)	(0.159)	(0.123)	(0.167)
Poscor x ΔRejected	.034	.025	-.208	-.160
	(0.116)	(0.201)	(0.156)	(0.211)
Negcor x ΔRejected	-.162	-.227	.165	-.270
	(0.11)	(0.167)	(0.13)	(0.176)
Constant	.000	-.140	-.071	-.117
	(0.065)	(0.094)	(0.073)	(0.098)

Number of observations: 274. *** significant at p<.01, * significant at p<.1. Standard errors are in parentheses.

Table A15. Moderation by beliefs about others' correlations in ratings (Study A2)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Chosen	.590***	.539**	.397***	-.111
	(0.137)	(0.22)	(0.131)	(0.259)
Rejected	-.115	-.259	.117	.447**
	(0.099)	(0.159)	(0.108)	(0.214)
Poscor	.886	2.592	1.331	-.390
	(1.534)	(2.461)	(1.045)	(2.075)
Negcor	.135	.156	1.492	-1.154
	(1.124)	(1.804)	(1.147)	(2.276)
Poscor x Chosen	-.493*	-.876**	-.118	.491
	(0.255)	(0.409)	(0.216)	(0.429)
Negcor x Chosen	-.231	-.619*	-.265	.290
	(0.21)	(0.337)	(0.211)	(0.42)
Poscor x Rejected	.444**	.592**	-.062	-.492
	(0.176)	(0.283)	(0.156)	(0.31)
Negcor x Rejected	.319**	.505**	.063	-.167
	(0.143)	(0.23)	(0.169)	(0.336)
Constant	2.846***	2.307*	2.868***	2.537**
	(0.733)	(1.177)	(0.589)	(1.169)

Number of observations: 151 (72 opposite-choosers condition, 79 same-choosers condition).
*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A16. Moderation by beliefs about others' correlations in ratings (Study A3)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Chosen	.562***	-.064	.528**	-.056
	(0.185)	(0.315)	(0.203)	(0.355)
Rejected	.120	.914***	.236	.777**
	(0.179)	(0.306)	(0.197)	(0.345)
Poscor	.310	2.328	1.787	2.263
	(1.099)	(1.877)	(1.209)	(2.117)
Negcor	2.500**	3.742*	2.486*	1.825
	(1.155)	(1.973)	(1.271)	(2.225)
Poscor x Chosen	.140	-.012	-.107	-.094
	(0.282)	(0.481)	(0.31)	(0.543)
Negcor x Chosen	-.288	-.467	-.224	-.191
	(0.221)	(0.378)	(0.244)	(0.426)
Poscor x Rejected	-.252	-.500	-.226	-.397
	(0.258)	(0.44)	(0.283)	(0.496)
Negcor x Rejected	-.181	-.302	-.297	-.196
	(0.226)	(0.386)	(0.248)	(0.435)
Constant	2.303***	.517	1.820**	1.236
	(0.697)	(1.19)	(0.767)	(1.342)

Number of observations: 64

*** significant at p<.01, ** significant at p<.05, * significant at p<.1. Standard errors are in parentheses.

Table A17. Moderation of manipulation by beliefs about others' correlations in ratings (Study A3)

	Estimate for Same-Choosers:		Estimate for Opposite-Choosers:	
	their chosen	their rejected	their chosen	their rejected
Unaware	-.750**	.250	-.750**	.083
	(0.311)	(0.524)	(0.327)	(0.531)
Poscor	-.133	.500	.133	.250
	(0.326)	(0.549)	(0.343)	(0.557)
Negcor	-.256	-.923*	-.321	-.788
	(0.304)	(0.514)	(0.321)	(0.521)
Poscor x Unaware	.150	-.550	.050	-.183
	(0.52)	(0.876)	(0.547)	(0.888)
Negcor x Unaware	.923**	.173	.737	.205
	(0.435)	(0.734)	(0.458)	(0.744)
Constant	6.333***	4.000***	6.167***	4.250***
	(0.22)	(0.37)	(0.231)	(0.375)

Number of observations: 64

*** significant at p<.01, ** significant at p<.05. Standard errors are in parentheses.