Moral Tribalism and Its Discontents:
How Intuitive Theories of Ethics Shape Consumers’ Deference to Experts

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Abstract

We study the psychology at the intersection of two social trends. First, as markets become increasingly specialized, consumers must increasingly defer to outside experts to decide among complex products. Second, people divide themselves increasingly into moral tribes, defining themselves in terms of shared values with their group and often seeing these values as being objectively right or wrong. We tested how and why these tribalistic tendencies affect consumers’ willingness to defer to experts. We find that consumers are indeed tribalistic in which experts they find convincing, preferring products advocated by experts who share their moral values (Study 1), with this effect generalizing across product categories (books and electronics) and measures (purchase intentions, information-seeking, willingness-to-pay, product attitudes, consequential choices). We also establish the mechanisms underlying these effects: Because many consumers believe moral matters to be objective facts, experts who disagree with those values are seen as less competent and therefore less believable (Studies 2 and 3), with this effect strongest among consumers who are high in their belief in objective moral truth (Study 4). Overall, these studies seek not only to establish dynamics of tribalistic deference to experts, but to identify which consumers are more or less likely to fall prey to these tribalistic tendencies.

*Keywords:* moral psychology, tribalism, expert choice, social evaluation
Moral Tribalism and Its Discontents: How Intuitive Theories of Ethics Shape Consumers’ Deference to Experts

If your dentist defended the Iraq War, would you let her near your teeth? If your stylist refused to recycle, would you believe him on the latest hair trends? One of us once bought a complex financial product from a service provider we’ll call Ted. Your author became increasingly alarmed as he received annual Christmas letters from Ted, hinting at a variety of ideological positions at odds with your author’s. On subsequent visits to Ted’s office, further clues were observed—newspaper clippings and annoying little slogans on the bulletin board. Increasingly, Ted’s expertise was thrown into doubt as minor snafus proliferated and questionable advice proffered. Your author’s horror was mixed with vindication when a serious error, years into this relationship, nearly led to a large financial exposure.

This paper looks at how and why alignment in moral values influences our evaluations of experts. This issue has become increasingly important, lying at the confluence of two social–economic trends. First, society increasingly functions through a division of cognitive labor (Hayek, 1945; Keil et al., 2008; Kitcher, 1990; Sloman & Fernbach, 2017), with knowledge distributed widely across individuals. Knowledge about cars, annuities, and teeth are clustered in mechanics, bankers, and dentists; since most consumers have limited expertise in these areas, they pay these experts for their knowledge. Likewise, consumers often are unable to evaluate a product—a movie, a kitchen gadget—before they have bought it, relying on expert product reviewers to inform their choices. Adam Smith (1776) noted that “the division of labour is limited by the extent of the market,” with jobs being divided up into smaller and smaller pieces as more and more people are available to do them, resulting in increased economic efficiency. Globalization and technology have kicked this process into over-drive, so that consumers seek outside expertise increasingly where they might once have developed internal expertise. One industry survey, for example, claims that only 42% of U.S. motorists have full confidence in their ability to change a flat tire, and only 26% in their ability to change their car’s oil (Spector, 2016). Such reliance on outside service providers would be unthinkable mere decades ago. Increasingly, knowledge workers devote their cognitive resources to their own specialized areas of expertise, relying on experts to fill the gaps.

Second, have entered an era of moral tribalism unprecedented in modern memory. People prefer to live near and befriend others with similar values, resulting in dramatic moral sorting by geography and occupation (Bonica, 2014; Pew Research Center, 2014). Of course, moral tribalism is not new. Ever-observant Adam Smith spotted this trend too, noting in his Theory of Moral Sentiments (1759) that “nothing pleases us more than to observe in other men a fellow-feeling with all the emotions of our own breast; nor are we ever so much shocked as by the appearance of the contrary.” But moral tribalism seems to be accelerating. People increasingly express displeasure at the thought of relatives marrying members of the opposite political party: Thanksgiving dinners in 2016 were nearly one hour shorter when they included guests from opposite-party precincts (Chen & Rohla, 2018). Moral tribes define a dominant divide in our culture (Chua, 2018; Goldberg, 2018; Greene, 2013; Haidt, 2012).

These two phenomena are fundamental to society, growing in power, and—we suggest—one on a collision course. In this article, we map the psychological mechanisms by which tribalism over moral issues leaks into consumers’ choices of experts. Our analysis depends on a fundamental distinction in consumers’ intuitive morality. On the one hand, people differ in their specific moral values (Haidt, 2012; Kahan et al., 2010). We will argue that consumers prefer experts who agree with them on moral matters, and this deference is due to the perception that experts in the consumer’s moral out-group are less competent. But at the same time, people also differ in their broader meta-ethical views (Goodwin & Darley, 2008)—their intuitive theories about how morality works. Some people believe that moral truths are objective (like science or mathematics), while others believe they are subjective (like aesthetics).
We contend that this latter group of subjectivists—the titular “discontents” of tribalism—should be less prone to use their own moral values to evaluate experts: If one cannot be right or wrong on issues of morality, then differences in moral opinion shouldn’t signal broader incompetence.

The Paradox of Expertise

Consumers face an increasingly wide array of complex, specialized, and novel products. Although consumers can sometimes develop internal expertise (Alba & Hutchinson, 1987), they must frequently defer to external experts to evaluate such products (Kiel & Layton, 1981; Naylor et al., 2011; Solomon, 1986; White, 2005). Customers rely on expert opinions for a variety of products, including books (Chevalier & Mayzlin, 2006), movies (Basuroy et al., 2003), hospitals (Pope, 2009), automotive products (Simonsohn, 2011), and technological controversies (Brossard & Nisbet, 2007).

But expertise poses a paradox. Knowledge is widely distributed across clusters of experts (Hayek, 1945; Sloman & Fernbach, 2017), who often disagree. How do we decide which expert deserves our deference? Consumers can try to evaluate the quality of the expert’s advice on its own terms (Chaiken, 1980). Yet, the same limits on our knowledge that lead us to consult experts, paradoxically, make it difficult to know which experts to believe (Gershoff et al., 2001; Goldman, 2001).

How do consumers nevertheless evaluate experts despite this ignorance? Researchers distinguish between two dimensions of trust (Siegrist et al., 2000, 2003; Sperber et al., 2010; Twyman et al., 2008; see also Fiske et al., 2007 and Goodwin et al., 2014). First, experts vary in perceived competence or epistemic trustworthiness—the quality of their judgment. Critical consensus is far from universal in cultural domains from restaurants to television to wine. Expert stock analysts often clash in their predictions and recommendations, fueling endless debates on networks such as CNBC. Even aggregated user reviews often differ from more objective measures of quality (De Langhe et al., 2016). When opinion varies so wildly, how can one decide which expert has the best judgment?

Second, experts differ in perceived truthfulness or moral trustworthiness—the absence of ulterior motives and willingness to express their true view. Experts, by definition, know more than consumers about the relevant field, producing an information asymmetry (Akerlof, 1970). In the financial domain, trust is among the strongest determinants of financial advice-seeking (Lachance & Tang, 2012). In the consumer goods domain, consumers may be becoming increasingly wary of user reviews, given cases in which reviews are manipulated by companies to enhance perceived product quality (Hu et al., 2012). In a world with both honest and deceptive experts, how can one decide who to trust?

These evaluations of competence and truthfulness themselves are made by using heuristics (Bonaccio & Dalal, 2006), such as the reliability of past advice (Gershoff et al., 2003; Yaniv & Kleinberger, 2000). When reputational cues are unavailable, consumers use heuristics such as the expert’s confidence (Price & Stone, 2004), knowledgeability (Sternthal et al., 1978), consensus with other experts (Budescu et al., 2003), and personal factors (e.g., education and life experience; Feng & MacGeorge, 2006). These heuristics often work because they are based on a sound underlying principle: An expert who is accurate in one instance is likely to be accurate in other instances too. Thus, even if a consumer cannot judge the expert’s accuracy on one occasion, these heuristics help a consumer to infer the expert’s broader competence. We argue below that consumers who believe in objective morality would likewise perceive an expert with “correct” moral values to be more accurate.

Moral Truth and Tribalism

Cooperation is essential to survival (Tomasello & Vaish, 2013), but creates the risk of exploitation. For this reason, humans have a set of biologically and culturally evolved mechanisms for assessing
who is likely to cooperate versus defect (Boyd & Richersen, 2009; Rand & Nowak, 2013). Chief among
these are evaluations of moral reputation (Fehr & Fischbacher, 2002; Uhlmann et al., 2015).

Some moral values are essentially universal. For example, harm and fairness are prized across many
cultures and political orientations (Graham et al., 2011; Haidt, 2012). But other moral values are more
contested, particularly those concerning the organization of society. In our studies, we focused
especially on egalitarianism–hierarchy (should a social order be flexible and equal vs. rigid and stratified?)
and communitarianism–individualism (should the group vs. the individual be the unit of moral analysis?).
These dimensions appear to be more fundamental than partisanship (Douglas, 1970; Wildavsky &
Dake, 1990). In the United States, for example, Republicans are stereotypically
individualist/hierarchist, and Democrats communitarian/egalitarian, but other combinations are
observed in other countries’ politics. These higher-order values are contested because they invoke
trade-offs among other values (Berlin, 1969). For instance, a more egalitarian society may satisfy our
appetite for fairness but not authority; a more individualist society may be fairer, in treating all
individuals alike, but harm those who are less well-off. Social harmony, such as it is, is maintained in
the face of this disagreement in part by separating ourselves into groups that share these values.

This is a recipe for tribalism. Since one expects to be able to cooperate with in-group members
(Balliet et al., 2014), we believe in the moral superiority of our in-group and inferiority of our out-
group (Leach et al., 2007; Parker & Janoff-Bulman, 2013). Indeed, apostasy—disavowal of one’s own
tribe’s values—is intensely taboo because it not only signals uncooperativeness but betrays the group’s
shared identity. Moral beliefs act both as norms that coordinate activity within a group and as markers
that distinguish one group from another (Haidt, 2012).

Given the dynamics of moral reputation and use of moral values to mark group identity, we expect
shared values with a communicator to signal credibility. Specifically, we predicted:

**H1**: Consumers defer to an expert to the extent that they share the expert’s moral values.

This basic phenomenon is tested in Study 1 using two distinct product categories—books (Study 1A)
and consumer electronics (Study 1B).

This prediction gains some plausibility from two related studies. Kahan et al. (2010) found that
people are likelier to defer to experts on controversial scientific issues (e.g., mandatory HPV
vaccination) when those experts share one’s moral values. Kahan et al. (2010) argue that experts’
values are a cue to the social acceptability of a viewpoint relative to one’s cultural group, and that this
drives political polarization in science. This can be rational because citizens have little incentive to
hold the correct beliefs about political issues (as individual citizens have little influence over policy)
but a strong incentive to act in accordance with norms held by one’s group (Ajzen, 1991; see also
Caplan, 2006). However, if this explanation is correct, these results would be unlikely to generalize to
consumption behavior, where the consumer does bear the consequences of holding true or false beliefs.
Aggravating this problem, Kahan et al. (2010) studied the relationship only between expert values and
beliefs about other value-laden topics (such as public policy) rather than topics unrelated to morality.

Recently, Marks et al. (2019) documented “epistemic spillovers” such that agreeing with a person
about one domain (political facts) interferes with the ability to assess that person’s skill in unrelated
domains (shape categorization) because political agreement is seen to signal competence. Although it
may be problematic to generalize from artificial shape categorization tasks to consumer decision-
making, the greater limitation in extrapolating from Marks et al. (2019) is their operationalization of
political agreement: Agreement over factual political issues (e.g., the effects of decreasing the voting
age) as opposed to moral issues. Disagreement over values, unlike disagreement over facts, may not
trigger perceptions of incompetence. Thus, though suggestive, this study does not test H1.
Nonetheless, based on research in moral psychology, we expected that shared moral values with an expert would continue to drive deference among a subset of people. This is because some people believe that moral values can be objectively right or wrong (Goodwin & Darley, 2008).

Individuals differ not only in their moral values themselves, but in their meta-ethical intuitions about how morality works. Indeed, moral philosophers themselves have differed over whether morality is objective—whether there are moral facts, in the sense that there are mathematical or scientific facts. On this view, some acts violate moral laws and are objectively immoral. Variants of moral objectivism have been argued by many historical and contemporary philosophers (Kant, 1785; Nagel, 1986). Other philosophers (Harman, 1975; Nietzsche, 1887) have viewed morality as subjective, like one’s preference for gelato flavors or painting styles. Although some acts might be better for maximizing individual happiness or social welfare, there would be no moral facts that obligate people to take such acts.

Given disagreement among philosophers, it may be unsurprising that ordinary people also differ in their meta-ethical intuitions. Like philosophers, however, most laypeople lean toward objectivism. For example, participants in one study (Goodwin & Darley, 2008) viewed morality as much more objective than preferences (e.g., about music) but less objective than straightforward facts (e.g., the size of Mars). Since moral values are socially central as well as objective in the eyes of many, someone who disagrees with one’s values would be seen not only as objectively wrong, but objectively wrong about something of fundamental importance. We therefore hypothesized that most people would view moral disagreement as a signal of incompetence. If an expert is wrong about something as important as morality, what else are they wrong about? Thus, Studies 2 and 3 test:

**H2:** The link between shared moral values and expert deference is mediated by perceptions of the expert’s competence.

This prediction distinguishes our model of value-based deference from that of Kahan et al. (2010), who theorize that we defer to experts sharing our worldview because they communicate the norms dominant in our social group. This can influence who we trust irrespective of the expert’s reliability. Although this mechanism may also be at play, we argue that value-based deference in our paradigm arises mainly from processes aimed at inferring competence.

Yet, people differ in the extent of their moral objectivism. People are less objectivist when there is a lack of consensus over a value (Goodwin & Darley, 2012) or when considering individuals from very different cultures (Sarkissian et al., 2011), and more objectivist in competitive social interactions (Fisher, Knobe, Strickland, & Keil, 2017). People also differ dispositionally in moral objectivism; people who ground their moral systems in religion or self-identity are likelier to be objectivists (Goodwin & Darley, 2008). Given that H2 is based on the assumption that consumers tend to be moral objectivists, individual differences in this trait should influence the magnitude of this effect. Specifically, those higher in objectivism should place greater weight on shared values when assessing expert competence. Therefore, Study 4 tests:

**H3:** The role of perceived competence in mediating between shared moral values and expert deference is moderated by individual differences in moral objectivism.

However, there are several other reasons why people might defer to morally similar experts. As we noted earlier, expert deference is determined by perceptions of both perceived competence (the quality of the expert’s opinion) and truthfulness (the honesty of the expert’s opinion). Moral similarity could influence perceived truthfulness if values signal in-group membership (Balliet et al., 2014) or moral character (Uhlmann et al., 2015). Although we are mainly interested in competence here, it is
plausible that truthfulness might also contribute, and thus these mechanisms need to be distinguished empirically. We do so in Studies 2 and 3.

In addition, similarity more broadly influences compliance. People are likelier to comply with requesters who are similar in musical taste (Woodside & Davenport, 1974), clothing (Emswiller, Deaux, & Willits, 1971), personality (Burger et al., 2001), height (Evans, 1963), birthday (Jiang et al., 2010), or even fingerprints (Burger et al., 2004). Three mechanisms mediate between similarity and compliance. First, we feel we understand the mental states of similar others, which creates a feeling of certainty about information they provide (Faraji-Rad et al., 2015). Second, we believe others are likely to have similar preferences, making their input more relevant in matters of taste (Hovland et al., 1953; Price et al., 1989). Third, we comply with similar others to satisfy a need to connect (Jiang et al., 2010), because we like and identify with similar others (Byrne, 1969; Kelman, 1961; Smeaton et al., 1989).

There is little direct work on the persuasive role of similar moral values (except Kahan et al., 2010). But shared values could plausibly signal several other kinds of similarity that could in turn trigger these mechanisms. For example, morally similar others could have similar personalities, leading us to more readily imagine their mental states; morally similar others could have similar preferences, making their opinions more diagnostic; and morally similar others could have similar social circles, triggering a desire to connect with them. Study 4 measures these other aspects of similarity and competes them against perceived competence as mediators, to address these alternative explanations.

Overall, we anticipated that we would find a consistent link between an expert’s shared values with a consumer and that consumer’s tendency to defer to the expert. Across four studies we test our proposed mechanism—moral objectivism creates a link between shared values and perceived expert competence—and pit this mechanism against several competitors. In the Supporting Information, we report four additional studies replicating key results under differing conditions.

**Study 1: Tribalistic Deference**

Study 1 tests the basic relationship between shared values and expert deference. Adapting the method of Kahan et al. (2010), these studies introduced participants to two experts who differed in one value (egalitarianism) but not a second value (communitarianism), as normed in a pretest (Appendix A in the Supporting Information). Since egalitarianism and communitarianism are perceived as moderately correlated, we always adjust for both traits in our regression models to test for effects of one value over-and-above the other.

Participants read a series of product reviews by each expert, who disagreed on some of the products. When the experts disagreed, we anticipated that participants would rely on the overlap in their moral values to determine which expert to trust, affecting purchase intentions and their interest in seeking information about that product.

We used two types of products—fiction books (Study 1A) and consumer electronics (Study 1B). These categories have distinct pros and cons, leading us to look for common patterns across categories. Fiction books are ecologically realistic because people widely rely on reviewers to determine which cultural products to consume. However, such products may be especially likely to be polarized, if people consume such products in part for social signaling (Berger & Heath, 2007). Electronics tend not to have strong signaling value, thereby avoiding this issue.

**Methods**

All sample sizes were set *a priori*. For Studies 1A–B, we targeted $N=200$, achieving 90% power for small to medium effects ($r>0.22$). For Studies 2–4, we targeted a larger sample size ($N=400$) to achieve
sufficient power to test our mediation and moderation hypotheses. For all studies, we report all measures, conditions, and exclusions.

Except as indicated, participants in all studies were from the U.S. and recruited using Amazon Mechanical Turk. Although Mechanical Turk workers are more diverse than traditional undergraduate samples, they do not fully reflect U.S. demographics, as Mechanical Turk workers tend to be younger, more educated, and more politically liberal than the general public. For Study 1, we recruited 398 participants \((M_{\text{age}}=36.1, 63\% \text{ female}; N=198 \text{ and } 200 \text{ for Studies 1A–B})\). Participants were excluded \((N=46)\) if they failed an attention check (see below).

In Study 1A, participants read about two book critics. These experts were similar in expertise, but differed in values. One expert espoused \textit{egalitarian} values:

Tim Harrison is a San Francisco-based columnist who also writes book reviews. John studied English Literature at the University of California-Berkeley and writes for \textit{The San Francisco Chronicle} and has also been published in the \textit{New York Times}. He is the author of \textit{Three Social Evils: Sexism, Racism, and Homophobia}. Tim spends time in community service activities, enjoys traveling, and coaches his son’s youth basketball team.

The other expert espoused \textit{hierarchist} values:

John Minerd is a Dallas-based columnist who also writes book reviews. John studied communications at Texas A&M University and has been published in \textit{The Wall Street Journal} as well as several other outlets. He is the author of \textit{The Crisis of Authority: The Assault on Traditional Values in America}. John is an avid hunter, enjoys fine wine, and plays golf regularly.

A photo was provided for each expert, normed by Kahan et al. (2010) as signaling egalitarian or hierarchical values. Following Kahan et al. (2010), both experts were white males, to avoid possible interactions with the expert’s race or gender. The experts were introduced in a counterbalanced order. On the same page as each expert, participants answered a series of factual multiple-choice check questions (e.g., “What city is Tim based out of?”) to verify comprehension. These questions did not ask about the columnists’ values. Participants answering more than 30% of these questions incorrectly were excluded from analysis.

Next, participants read reviews of 14 books. For each book, the cover was shown, along with the title, author, and brief synopsis. Below, each critic provided a rating from 1 to 4 stars and a brief review (based on real reviews from lithub.com), roughly equated for length. The first two books were respectively reviewed positively and negatively by both critics. The critics’ reviews differed for the other 12 books (presented in a random order), with the egalitarian critic positive and the hierarchist critic negative for half of the books, and the converse pairing for the other half (counterbalanced). Due to an error in the study materials, two items were removed from analysis, but the results are similar if these items are included. The order of the two reviews was always the same (egalitarian or hierarchist critic first), matching the order in which the critics were introduced. For each book, participants rated purchase intention (“What is the probability that you would consider reading \textit{White Tears} by Hari Kunzru?”) on a 0–100 scale. Participants could also check a box on each page to receive more information at the end of the study, to measure information-seeking.

After the main task, participants completed several additional measures: (1) A memory task asking participants to check boxes corresponding to the covers of books they had seen during the study (participants incorrectly answering more than 30% were excluded from analysis). (2) Six-item scales measuring egalitarianism (e.g., “We have gone too far in pushing equal rights in this country” [reverse-coded]; \(\alpha=.85\)) and communitarianism (e.g., “The government should do more to advance society’s goals, even if that means limiting the freedom and choices of individuals”; \(\alpha=.90\)) on short versions of standard scales (Kahan et al., 2010) in a counterbalanced order. (3) A qualitative question about
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participants’ thought processes and a forced-choice question asking explicitly whether participants tended to follow one or the other expert’s advice or instead made decisions case-by-case. (4) A checklist of which books participants had previously read (we did not exclude any items on this basis, since on average only 1% of participants had read a given book). (5) Basic demographics.

Study 1B used the same method except participants read about consumer electronics. The expert biographies were lightly altered to reflect consumer product rather than book reviewers. Participants read about 10 products, based on real Amazon customer reviews, for which the experts disagreed on 8. For each product, a photograph, brief description, and retail price were shown. Below, each reviewer provided a star rating on a 1 to 4 scale and a brief review. Products included a tablet, camera, blender, dehumidifier, printer, microwave, pressure cooker, sound bar, vacuum cleaner, and watch, with retail prices ranging from $45 to $240; as for Study 1A, two items were omitted from analyses due to an error with the materials. The measures were similar to Study 1A.

Results

Overall, participants reported higher purchase intentions and sought additional information more frequently for products advocated by experts sharing their values. In addition to the analyses reported here, further analyses are reported in the Supporting Information (Appendix B).

In preparation for analysis, purchase intentions were averaged separately for products recommended by the egalitarian and hierarchist experts. The key dependent measure is the difference in purchase intentions between the two sets of recommendations (egalitarian – hierarchist). Thus, positive difference scores indicate a preference for products reviewed positively by the egalitarian, holding constant the product and review content (since the experts’ views were counterbalanced). Since the two experts differed in egalitarianism but not communitarianism (Appendix A), H1 predicts that participants’ egalitarianism, but not communitarianism, should predict these scores. This is equivalent to predicting an interaction effect between expert values and participant values, since expert values are manipulated within-subjects and participant values measured between-subjects.

This was indeed the case. We used multiple regression, with the difference scores as the dependent variable and egalitarianism and communitarianism as predictors (centered at their midpoints and scaled by their standard deviations). Communitarianism was included as a covariate in all models, since egalitarianism and communitarianism are perceived as correlated (Appendix A). We repeated all analyses in the main text without this covariate, finding that the results are unchanged except as noted.

More egalitarian consumers preferentially deferred to the egalitarian reviewer in Studies 1A ($b=11.15, p<.001$) and 1B ($b=7.98, p<.001$). Communitarianism did not predict deference in any of these studies ($ps>.53$). The simple slopes are depicted in Figures 1A–B and regression coefficients in Table 1. Thus, people defer to experts sharing their moral values along one dimension (egalitarianism, which differed across experts), with the effect specific to that dimension (no effect of communitarianism, which was equated). Results are similar when both participants and items are treated as random in a multilevel model (Appendix B).

Differences in purchase intentions translated into information-seeking behavior, measured by the frequency of clicks. Differences in clicks between products advocated by the egalitarian versus hierarchist were predicted by egalitarianism in Study 1A ($b=.0354, p=.025$) and 1B ($b=.0223, p=.037$), as shown in Figures 2A and 2B, with regression results in Table 1. Communitarianism did not positively predict clicks in Study 1A ($b=-.0278, p=.032$; note that this effect is negative) or 1B ($b=-.0147, p=.18$); however, the effect of egalitarianism becomes non-significant in Study 1A and marginally significant in Study 1B if the communitarianism covariate is dropped from the model. As shown in Appendix B, the effect of values on clicks is mediated by purchase intentions.
Despite the strong influence of expert values on deference, many participants were unaware of this influence. When participants were asked at the end of study to indicate whether they tended to side with one advisor over the other, most (79.4% and 69.3% in Studies 1A–B) denied doing so. When this subset is analyzed separately, they continue to show a strong influence of shared values on purchase intentions ($b=4.89$, $SE=2.03$, $p=.017$ and $b=4.38$, $SE=1.64$, $p=.009$ for Studies 1A–B). Consumers thus may fail to detect persuasion attempts made by experts with shared values, perhaps not even recognizing that their attitudes are being influenced.

**Discussion**

These results show that consumers defer to experts who share their moral worldview. Participants holding strong egalitarian values deferred to experts with egalitarian values, and vice versa. This was manifested both in purchase intentions and in information-seeking behavior; Study S1 in Appendix A replicates the effect among British students facing consequential choices. Despite the large magnitudes, most participants appeared unaware of these influences (Nisbett & Wilson, 1977). Inferences about experts may occur largely unconsciously, analogous to how people infer competence from brief exposure to faces (Todorov et al., 2005) or use complex rules to guide their moral intuitions without being able to articulate those rules (Cushman et al., 2006; Mikhail, 2009).

Moreover, the role of shared moral values takes a similar shape across very different product categories. Shared values guided deference about fiction books—a cultural and principally hedonic product, where arguably one’s choices act as signals of one’s social group. But shared values also guided deference about consumer electronics and appliances, such as printers, blenders, and...
dehumidifiers—comparatively utilitarian products. Consumers prize expert reviews of such products—since such reviews litter the pages of the *New York Times* and *Consumer Reports* alike. We suspect that similar effects are at play for a variety of other product categories where expert reviews are sought, such as cars, resort travel, movies, and restaurants.

*Figure 1A.* Simple slopes of egalitarianism on purchase intention for each critic’s recommendations in Study 1A (books). Bars indicate 1 SE of the coefficient estimates.

*Figure 1B.* Simple slopes of egalitarianism on purchase intention for each critic’s recommendations in Study 1B (electronics and appliances). Bars indicate 1 SE of the coefficient estimates.
Figure 2A. Simple slopes of egalitarianism on clicks (information-seeking) for each critic’s recommendations in Study 1A (books). Bars indicate 1 SE of the coefficient estimates.

Figure 2B. Simple slopes of egalitarianism on clicks (information-seeking) for each critic’s recommendations in Study 1B (electronics and appliances). Bars indicate 1 SE of the coefficient estimates.
Study 2: Is Tribalistic Deference Driven by Perceived Competence or Truthfulness?

We proposed two reasons why shared values might influence consumers’ deference. Consumers might deem an expert sharing their values to be more *competent*—believing that morally similar experts have generally more accurate beliefs. According to H2, this would be the primary link between shared moral values and deference. Or consumers might deem an expert sharing their moral values to be more *truthful*—believing that the expert has superior moral values and is therefore less likely to deceive. Study 2 teases apart these possibilities.

**Methods**

We recruited 399 participants (\(M_{age}=37.9\), 35% female). Participants were excluded (\(N=22\)) based on the same criteria as Study 1.

The procedure was the same as Study 1B, except two changes. First, instead of measuring purchase intentions, we measured willingness-to-pay (WTP) for each product (e.g., “What is the maximum price you would be willing to pay for this blender?”) on a scale centered at the product’s retail price (rounded to the nearest $10) and ranging from $0 to twice the retail price. Second, three sets of rating scales were included after the main task (between the check questions and the values scales). These scales asked participants to rate each critic on truthfulness (“I trust Tim to give his objective opinion about the products”), competence (“I believe that Tim has good judgment about products”), and similarity (“Overall, I find myself to be similar to Tim”) on 0–10 scales. These ratings were made on three separate pages (in a random order for each participant), with the experts listed in the same (counterbalanced) order as in earlier parts of the study.

**Results**

Consumers’ moral values again shaped their deference, manifesting in greater willingness-to-pay for products endorsed by morally similar experts (H1). Perceived competence, but not truthfulness, was the key mediator (H2).

In preparation for analysis, WTP for each item was normed as a proportion of retail price. The normed WTP was averaged separately for the products recommended by the egalitarian and the hierarchist recommenders, and the difference between these WTP scores (egalitarian – hierarchist) used as the key dependent measure, analogous to Study 1.

<table>
<thead>
<tr>
<th>DV: Willingness-to-Pay (Proportion of Retail Value) (Egal – Hier Difference Scores)</th>
<th>Intercept 0.019 (0.019)</th>
<th>Egalitarianism 0.075 (0.017) ***</th>
<th>Communitarianism 0.017 (0.015)</th>
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<tbody>
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<td>R²</td>
<td>.18</td>
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* < .10  * < .05  ** < .01  *** < .001

**Table 2.** Regression model (Study 2).

*Note.* Entries are unstandardized bs and SEs.
These difference scores were predicted using multiple regression, with egalitarianism and communitarianism as predictors (Table 2). Like Study 1, egalitarianism was a significant predictor of WTP differences ($b=.0745$, $p<.001$), while communitarianism was not ($b=.0169$, $p=.27$). The simple slopes (Figure 3) show that among those low in egalitarianism, WTP was about 10% higher for books recommended by the hierarchist expert, whereas among those high in egalitarianism, WTP was about 20% higher for books recommended by the egalitarian. Results are similar when both participants and items are treated as random in a multilevel model (Appendix B).

Next, we test mediators to understand the mechanisms underlying this relationship between shared values and deference. We used parallel mediation (PROCESS Model 4; Hayes, 2013) to simultaneously test the independent contributions of the competence and truthfulness pathways. As shown in Figure 4, the shared values $\rightarrow$ competence $\rightarrow$ WTP path was significant ($b=.0429$, 95% CI[.0151,.0742], $p=.001$) while the shared values $\rightarrow$ truthfulness $\rightarrow$ WTP path was not ($b=-.0111$, 95% CI[-.0411,.0174], $p=.46$). This supports H2.
Discussion

These results support our framework. First, shared values once again guided expert deference. As in Study 1, egalitarianism but not communitarianism predicted deference, consistent with H1 since the experts differed in egalitarianism but not communitarianism. Second, this result was underpinned by judgments about competence. Shared values informed judgments of similarity, influencing beliefs about expert competence, which in turn fueled deference. This is consistent with H2: Consumers take the fact that an expert has adopted the “correct” views on morality as evidence of broader competence.

Appendix A reports a replication (Study S2), which used the book stimuli from Study 1A and purchase intention as the dependent measure. The results were similar, indicating that these effects and mechanisms generalize across stimuli.

Perceived truthfulness, however, was not a significant mediator. Even though shared values did inform perceived truthfulness, these intuitions did not drive purchase intentions. This may be because their chief source of uncertainty was not the risk of deception, but of error. If so, perceived truthfulness may emerge as a mediator in situations where deception is more plausible (e.g., experts paid by a company) or costly (e.g., medical advice).

Study 3: Non-Tribal Moral Values

Previous studies operationalized moral similarity in terms of agreement on values that systematically differ across individuals. This approach captures the notion of tribalism, but has the shortcoming that we cannot randomly assign agreement with expert values. In Study 3, we relied instead on non-tribal moral values—fairness and harm (Graham et al., 2011)—manipulating whether the advisor embraced or rejected these socially normative values. Study 3 thus aimed to adopt a design conducive to random assignment and extend our results to a new dimension of values.

Methods

We recruited 400 participants ($M_{\text{age}}=37.3$, 50% female). Participants were excluded from all analyses ($N=50$) based on the same criteria used in Studies 1 and 2 (see below for additional exclusion criteria). The methods and analyses were pre-registered at OSF (https://osf.io/k9bqv).
Participants were introduced to a single reviewer. The main portion of the biography was analogous to that used in Studies 1 and 2, but designed to be ideologically neutral. The reviewer’s values were instead manipulated between-subjects by including titles of recent newspaper columns he had written. In the normative expert condition, the titles implied socially common moral positions (e.g., “Why community matters” and “How and why to honor our parents”), whereas in the counter-normative expert condition, the titles implied rejections of socially common values (e.g., “The virtue of unfairness” and “Why the age of consent should be lowered to 14”). All other details about the reviewer’s biography and photograph were held constant across conditions. After reading the biography, participants answered a set of multiple-choice questions to verify comprehension, as in other studies.

Next, participants rated 8 consumer electronics products. For 4 products (counterbalanced), the reviewer positively reviewed the product, whereas for the other 4 products the reviewer negatively reviewed the product. This was analogous to Studies 1 and 2, except only one reviewer’s opinion was given for each item. The dependent measures were the same as in Study 1. After providing all product ratings and completing a recognition memory check (same as Studies 1 and 2), participants rated the reviewer on the same dimensions used in Study 2—perceived competence (“I believe that Tim has good judgment about products”), truthfulness (“I trust Tim to give his objective opinion about the products”), and similarity (“Tim and I share the same moral value system”).

Results and Discussion

As predicted, the reviewer was deemed less competent when he rejected rather than embraced socially common moral beliefs, which in turn predicted lower levels of deference to the reviewer’s recommendations, supporting H2. The total effect of the moral values manipulation on deference was less consistent and depended on analytical choices (see below).

Our predictions were predicated on the manipulation of reviewer values successfully leading to higher vs. lower levels of perceived moral similarity. Perceived moral similarity did indeed differ across conditions [$M=6.17$, $SD=1.93$ vs. $M=4.48$, $SD=2.79$; $t(348)=6.62$, $p<.001$, $d=0.71$], though perhaps not as much as one would expect. To address the problem that some participants may themselves hold counter-normative values, we analyze the data in multiple ways. Here, we report a (pre-registered) analysis that removes the 20% of participants rating themselves least similar in the normative expert condition and the 20% most similar in the counter-normative expert condition. In Appendix B, we report two other analyses (a pre-registered analysis excluding no one based on similarity and an exploratory analysis that uses a stricter exclusion rule), noting below where these analyses differ.

To measure deference, we average separately the product ratings across the four items reviewed positively by the expert and the four items reviewed negatively, taking the difference score so that positive numbers indicate higher ratings for the recommended items. Results are similar when both participants and items are treated as random in a multilevel model (Appendix B).

Overall, there was a highly significant effect of expert values on perceived competence [$M=7.91$, $SD=1.39$ vs. $M=6.04$, $SD=2.14$; $t(258)=8.22$, $p<.001$, $d=1.02$], as well as a strong correlation between perceived competence and deference [$r(258)=.42$, $p<.001$], pooling across conditions. These two effects led to a marginally significant effect of expert values on deference [$M=41.2$, $SD=29.0$ vs. $M=34.9$, $SD=24.4$; $t(258)=1.92$, $p=.056$, $d=0.24$]. The effect of condition on competence and the correlation between competence and deference are consistent across exclusion criteria, whereas the total effect of expert values on deference becomes stronger when using a stricter exclusion rule and non-significant when using no exclusion rule (Appendix B).
Figure 5. Parallel mediation of condition–deference relationship by perceived competence and truthfulness in Study 3.

To test whether the effect of condition was mediated by competence (H2), we conducted a parallel mediation analysis (PROCESS Model 4) analogous to Study 2. Condition was the independent variable, perceived competence and truthfulness were mediators, and purchase intention was the dependent variable. As shown in Figure 5, this analysis uncovered a significant pathway via perceived competence ($b=9.27$, 95% CI[3.76,15.54], $p<.001$), but not via perceived truthfulness ($b=1.90$, 95% CI[−1.93,5.93], $p=.32$).

Overall, Study 3 found further support for H1 and H2—the impact of shared values on expert deference due to competence inferences. Results were similar to previous studies despite key methodological differences—showing only a single reviewer, manipulating values between-subjects, and using acceptance or disavowal of socially normative values to operationalize values.

Study 4: The Role of Moral Objectivism

Study 4 tested the moderating role of moral objectivism (H3). People vary in their belief that moral truths are objective rather than subjective (Goodwin & Darley, 2008). We theorized that moral similarity is a better guide to competence when morality is thought to be objective, since disagreement on objective (but not subjective) matters signals incompetence. The mediating role of competence should be moderated by moral objectivism.

Study 4 also looked at alternative mechanisms. Shared moral values might signal other socially important types of similarity. Experts with similar values might have (i) similar preferences, which might increase the relevance of the expert’s judgment and therefore compliance (Price et al., 1989); (ii) similar social groups (Kahan et al., 2010), which may trigger a need to connect and therefore increase compliance (Jiang et al., 2010); and (iii) similar personalities, making it easier to simulate the expert’s mental states and therefore comply (Faraji-Rad et al., 2015). We were agnostic about these other potential mediators, aiming to empirically separate any such effects from perceived competence. If these effects were observed, we would not expect them to be moderated by moral objectivism.

Methods
We recruited 395 participants ($M_{age}=38.9$, 57% female). Participants were excluded ($N=8$) based on the same criteria used in previous studies.

The task was streamlined, relative to previous studies. After reading the same descriptions of the experts used in Study 1A and answering the same questions about them, participants read two sets of reviews—one for a book recommended by the egalitarian but not the hierarchist expert, and one the converse. The order of the books and reviews was counterbalanced. For each book, participants answered two questions measuring their attitude toward the book (rating each book from “poor quality” [0] to “high quality” [10] and their opinion from “unfavorable” [0] to “favorable” [10]; $r>.80$) and one question about their purchase intention (“What is the probability that you would purchase a copy of [book] in the future?” from 0 to 10). The composite attitude ratings and purchase intention ratings were highly correlated ($r>.70$), so we averaged them for analysis. After completing all ratings, participants were given the opportunity to enter themselves into a lottery, choosing which of the two books they wished to receive if they won; most participants participated ($N=331$). One participant was selected at random as the winner and mailed her choice.

Next, participants made judgments about each expert. This included four aspects of similarity [moral (“John’s moral values are similar to mine”), preference (“John’s preferences are similar to mine”), social (“John’s friends are similar to mine”), and personality (“John’s personality is similar to mine”) and competence (“I believe that John has good judgment about books”), all on 0–10 scales. The four similarity judgments were completed in a random order on a separate page for each expert, followed by the competence judgments on the subsequent page.

Finally, participants completed the same egalitarianism and communitarianism scales used in previous studies, followed by a five-item moral objectivism scale derived from previous research (Goodwin & Darley, 2008, 2012), measuring beliefs about the objectivity of morality (“Considering your values and beliefs on questions of morality, how strongly do you agree or disagree with each of these statements?”). An exploratory factor analysis revealed two distinct subscales ($r=-.03$): One measuring the normativity of the beliefs (“Every good person on earth, regardless of culture, holds these beliefs,” “The truth of these beliefs is self-evident,” and “A society could not survive without its citizens holding these beliefs”; $\alpha=.77$) and one measuring subjectivity (“If someone strongly disagreed with you about one of these beliefs, it is possible that neither you nor the other person are mistaken” [reversed] and “There are no clearly true or false answers to these questions” [reversed]; $\alpha=.61$).

Results

The composite product ratings (collapsing across attitude and purchase intention ratings) were predicted by the consumers’ alignment with the advisors’ values. A regression on the difference scores (analogous to previous studies; Table 3) found that egalitarianism was associated with the difference between ratings for products endorsed by the egalitarian and hierarchist advisors ($b=1.32$, $SE=0.15$, $p<.001$), while communitarianism was not ($b=0.14$, $SE=0.15$, $p=.36$).

Moreover, these effects on product ratings manifested in consequential choices. In a binary logistic regression (Table 4), egalitarianism ($\chi^2=5.11$, $p<.001$) but not communitarianism ($\chi^2=0.53$, $p=.60$) predicted choices aligned with the egalitarian. A participant one SD above the midpoint on egalitarianism was predicted to side with the egalitarian expert 65% of the time, whereas a participant one SD below would side with the egalitarian 29% of the time.
Next, we tested the moderating role of moral objectivism (Figure 6). We used a moderated mediation model, with objectivism as the moderator; PROCESS Model 7 was used because our theory predicts that objectivism should moderate the effect of shared values on perceived competence. Egalitarianism (but not communitarianism) predicted differences in perceived competence ($b=1.60, SE=0.18, p<.001$), but this was qualified by the interaction between egalitarianism and objectivism ($b=0.47, SE=0.16, p=.003$), such that the effect of egalitarianism was stronger for participants higher in objectivism. Consequently, the indirect effect of egalitarianism on product ratings was stronger for participants 1 SD above the mean on egalitarianism ($b=1.18, 95\% CI[0.87,1.51], p<.001$) compared to participants 1 SD below the mean ($b=0.70, 95\% CI[0.39,1.03], p<.001$), leading to a significant index of moderated mediation ($b=0.27, 95\% CI[0.07,0.47], p=.009$). (This moderation was driven by the subjectivism, rather than the normativity, subscale of objectivism scale.) This supports H3.

Finally, we tested the alternative similarity-based mechanisms against perceived competence. We fit a parallel mediation model (PROCESS Model 4), to separate the independent contributions of each mechanism. The model used moral similarity as the independent variable, product ratings as the dependent variable, and our four mechanistic proxies as mediators: competency, preference similarity, social similarity, and personality similarity. (All of these variables are egalitarian – hierarchist difference scores.) As shown in Figure 7, the indirect effect via perceived competence was the strongest ($b=0.23, 95\% CI[0.15,0.31], p<.001$) and the indirect effect via preference similarity was also significant ($b=0.13, 95\% CI[0.04,0.22], p=.005$), while the other indirect effects were not significant. The results are similar with egalitarianism instead of moral similarity as the independent variable.
To test which of these pathways were moderated by moral objectivism, we fit a moderated mediation model (PROCESS Model 7), with objectivism as a moderator of the links between moral similarity and the four mediators. Consistent with our theory, the index of moderated mediation was significant only for the competence pathway ($b=0.030$, 95% CI $[0.006, 0.059]$, $p=.016$), but only marginally for similarity of preferences ($b=0.006$, 95% CI $[-0.001, 0.015]$, $p=.095$). This further supports our reasoning behind H3. Shared moral values would be an equally good guide to shared preferences regardless of whether those values are objective, since subjective values could still correlate with subjective preferences. But shared values would not be a good guide to competence if those values are subjective, since competence reflects objective abilities.
Discussion

Participants higher on moral objectivism used shared values as a stronger cue to competence compared to participants lower on moral objectivism—the discontents of tribalism. These results not only support our theoretical model, but help to shore up various empirical questions about Studies 1–3. Study 4 used a wider variety of product ratings, as well as a consequential choice (since participants could win their chosen book in a lottery). Nonetheless the basic influence of shared values on deference was robust across these measures. Moreover, relying on a much smaller number of items decreases the risk that participants would infer and comply with the experimenter’s intention. A near-exact replication (Study S3 in Appendix A) found very similar results.

General Discussion

Consumers must often defer to experts when they seek out products that are novel or complex. But experts often disagree, leaving consumers to choose who to believe. Consumers use a variety of cues as they attempt to discern which experts are truthful (accurately reporting their opinions) and competent (having opinions worth reporting).

Here, we demonstrated a potent cue consumers use for deciding which experts are worthy of deference—the similarity between the expert’s and consumer’s moral value system. We reasoned that many consumers are *moral objectivists*, believing that moral values are facts in the same kind of way that scientific or mathematical facts can be objectively true or false (Goodwin & Darley, 2008). Thus, experts who differ from a consumer’s moral values would be making an objective error of judgment, leading to decreased perceptions of competence and in turn less deference. In support of this framework, we found that consumers express stronger purchase intentions and willingness-to-pay for products recommended by experts who share their values (H1) and that this effect is mediated by perceived competence (H2) and moderated by individual differences in moral objectivism (H3). Thus, we not only identified that moral similarity predicts deference to experts, but why and for whom this effect occurs most strongly.

Robustness and Limitations

Between the main text and Appendix A, we report 9 studies with collectively over 3000 participants, which consistently support our theoretical framework. The basic effect of moral similarity on expert deference was robust across every measure we could think of (purchase intention, willingness-to-pay, product attitudes, information-seeking, and consequential choices) and across hedonic (books) and utilitarian products (consumer electronics). In a separate line of research, we are finding similar results for investors’ deference to financial advisors with shared or unshared values. Further, we showed that the results are robust to different statistical models, including hierarchical models and alternate mediation specifications (Appendix B). Despite this robustness over measure, product type, and modeling choices, there are several areas where the robustness is arguably less clear-cut and where future research may be illuminating.

First, how well would these results would generalize across populations? We do find similar results across two different populations (American online panel and British students). Yet, as with much behavioral research, it is unclear how well the results would generalize to non-Western populations.

If our theoretical framework has correctly identified moral objectivism as the key lever that determines the extent of tribalistic deference, then our theory might in fact predict differences in value-based deference across cultures, to the extent that objectivism differs across cultures. Since existing research
finds similar patterns of objectivism in China, Poland, and Ecuador as in the United States (Beebe et al., 2015), we would expect our results to generalize to those countries. Still, additional cross-cultural research could illuminate both tribalistic deference and moral objectivism itself.

Second, how well would the results generalize across different kinds of moral values? In Studies 1, 2, and 4, we operationalized similarity on moral values in terms of differences in egalitarianism (Wildavsky & Dake, 1990), which is correlated with political party, lending credence to the concern that the results could be specific to partisanship. Two considerations weigh against this possibility. First, our experts differed principally in egalitarianism rather than communitarianism (Appendix A), and consequently we find that participants’ egalitarianism but not communitarianism predicts deference. This was true even though both values are associated with political party in the United States. Second, Study 3 operationalizes values in terms of those that are shared across political parties (e.g., fairness; Haidt, 2012), and value similarity in terms of whether experts share these normative values versus counter-normative values. This study found very similar results, including mediation by competence. However, even though these results do not appear to be specific to political partisanship, further research might test whether they would generalize to other dimensions of moral values.

Third, how strong is the support for the competence-inference mechanism? The results suggest it is quite robust across measures, but it may be less robust across contexts. We find support for perceived competence as a mediator in six studies (between the main text and Appendix A) which rely on many different measures of deference and two different measures of competence (a single-item measure and a scale). Moreover, we pitted this mechanism against several other explanations. Perceived truthfulness was tested as a potential mediator in several studies, never finding any support. Study 4 tested several other dimensions of similarity that might be confounded with moral similarity (preference, social, or personality similarity), competing them as alternate mediators. There was some support for preference similarity as an additional mediator, but perceived competence remained a significant mediator in that analysis (and only the perceived competence pathway was moderated by moral objectivism, consistent with our theory). While these results provide confidence that perceived competence is the key mediator in the contexts we studied, other mechanisms might come into play in other situations. We studied contexts where there is no particular reason to doubt that experts would lie. In contexts where experts are known to often have conflicts of interest or a history of malfeasance or where the stakes are high, truthfulness may emerge as a potent mechanism.

Finally, how confident can we be in the strength of our causal claims? Many of the studies rely on individual differences (rather than randomly assigned treatments) for making mechanistic claims, particularly individual differences in egalitarianism and in moral objectivism. We acknowledge that these studies are potentially susceptible to “lurking third variable” problems, and would encourage other researchers to test specific alternative theories about the causal relationships among these variables. However, these concerns are mitigated by three considerations. First, although mediation analyses are themselves correlational, they reveal an intricate pattern of effects—including selective mediation by theoretically relevant variables (perceived competence) and not by irrelevant variables (perceived truthfulness), as well as moderation by moral objectivism. Any alternative explanation would need to account for this full pattern of mediation and moderation effects. Second, we statistically adjust for one potential lurking variable—communitarianism—which helps to buttress our account. Third, Study 3 replicates our key findings using random assignment, rather than individual differences, to capture moral similarity. Although the correlational nature of many of these studies precludes airtight causal inference, our theoretical framework appears to be the best available explanation, absent an alternative that can explain the full pattern of results.

Theoretical Contributions and Further Research
Influence and expertise. How do attitudes change and opinions spread? Here, we have documented a novel mechanism of attitude change—objectivist views of morality lead shared moral values to signal competence. This research can be extended in two directions.

First, we tested this mechanism in the domain of expertise. Would this mechanism extend to other sources of influence? Attitude change is at the center of several inter-related literatures, including persuasion (Hovland et al., 1953), advice-taking (Bonaccio & Dalal, 2006), and social contagion (Iyengar, Van den Bulte, & Valente, 2010). Moreover, various forms of interpersonal influence are increasingly crucial in the marketplace, including mavens (Clark & Goldsmith, 2005), influencers (Brown & Hayes, 2008), and celebrity endorsements (Erdogan, 1999). Could salespeople signal their values in order to increase their persuasiveness? Is a consumer likelier to defer to one’s friends who most closely share her values? Do shared values within an organization increase the likelihood of compliance with advice? If they exist, these effects could be driven by competence, as in the current studies, but it would be interesting to test the role of truthfulness in these contexts.

Second, there has been relatively little work within consumer behavior looking at cues people use to evaluate experts—a surprising gap, given the large amount of attention given to this topic within psychology and philosophy (Goldman, 2001; Keil et al., 2008; Kitcher, 1990; Marks et al., 2019; Mills, 2013; Sperber et al., 2010; Suldoevsky et al., 2019). Therefore, we believe the mechanisms by which consumers defer to experts is a ripe topic for exploration within consumer behavior, with the current results constituting one of the initial steps. Further work might examine when and which consumers are likelier to rely on their own judgment versus that of an expert; whether the mechanisms of expert deference differ across product categories; and what cues, aside from shared values, drive consumers’ deference to particular product experts.

We suspect that one useful theoretical approach toward answering both sets of questions is to more carefully consider the roles of narratives and identity in economic decision-making (Shiller, 2017; Tuckett & Nikolic, 2017). For example, managers often seek to imbue their employees with organizational identity to improve worker productivity (Akerlof & Kranton, 2005) and professional money managers use narratives to make sense of company activities to make judgments about the future with sufficient conviction to act (Tuckett, 2011). More broadly, we often make choices with the goal of enacting our identities—adding harmonious deeds to the stories we tell about ourselves (McAdams & McLean, 2013). Likewise, here we document a way that consumers enact their social identities—here in the form of closely held moral values—in their patterns of deference and consumption. When experts and other agents of influence promote a particular point of view, it is often useful to think of their view as a narrative in competition with other narratives the consumer is exposed to. An area of great importance for consumer behavior and the social sciences more broadly is to understand how these narratives are taken up and the effects of this process on decision-making.

Ideology and morality. How do values shape consumption? In addressing this question, our work contributes to three distinct literatures. First, a growing literature studies the relationship between ideology and consumption (Fernandes & Mandel, 2014; Jung et al., 2017; Khan et al., 2013; Kidwell et al., 2013; Shepherd et al., 2015). Whereas most of these studies have focused on ways that individual differences in ideology influence consumption (e.g., conservatives are more prone to variety-seeking; Fernandes & Mandel, 2014), we focus on a mechanism common to individuals across the moral spectrum: Tribalism with respect to expert deference.

Second, many researchers are interested in moral judgment and behavior in consumption contexts (e.g., Reczek et al., 2018; Samper et al., 2018; White & Simpson, 2015; among many others). Separately, social and cognitive psychology have been experiencing a revolution in research on moral judgment. We bring one aspect of this emerging research tradition—the construct of moral objectivism (Goodwin & Darley, 2008)—to the consumption domain. However, we suspect that there
is much more to learn about consumption behavior from new discoveries in the basic science of moral psychology. For example, in addition to warmth and competence, morality has been argued to be a third fundamental dimension of person perception (Goodwin et al., 2014), and some research has already begun to demonstrate the profound impact of perceived morality on consumers’ judgments of service providers (Kirmani et al., 2017). There is much work to be done in unpacking the implications of this insight for consumer behavior.

Third, several streams of research examine how culture shapes behavior, both within and across countries (Arnould & Thompson, 2005; Luna & Gupta, 2001). Here, we take an experimental approach to the question of how intra-national culture, manifested in moral tribes, influences consumer behavior. By focusing on social groups defined by shared moral commitments, this work bridges the literatures on culture (Arnould & Thompson, 2005) and intergroup dynamics (Kahan et al., 2010; Tajfel & Turner, 1986).

Practical Implications

This work has implications for marketing consumer products (since they are often recommended by experts, such as salespeople or product reviewers) and for the expertise market itself (such as publishers of product reviews or providers of expert services). Most straightforwardly, putative experts should seek to establish connections with consumers via shared values whenever feasible. For example, if salespeople are to provide expert advice to consumers (e.g., in cosmetics or car sales), this advice is more likely to be taken seriously if the salesperson can establish shared values. Shared values would not only be likely to benefit interpersonal trust, but, as we have shown, consumers would be more likely to consider the underlying advice valid. As another example, if a company wishes to market a product toward a specific moral demographic, the company should take steps to get the product reviewed by members of that moral in-group. More speculatively, negative reviews by the ideological out-group may be less harmful or perhaps even beneficial in some cases; for example, a book reviewed negatively by Ann Coulter could appeal to those who do not respect Coulter’s judgment. Future work might examine this potential “backfire” effect.

This research also points to strategies for segmenting along ideological lines and marketing separately to each demographic. For example, marketers could choose to seek reviews in publications differentially read by different ideological groups (e.g., the New York Times vs Wall Street Journal, Fox News vs MSNBC, or Breitbart vs the Huffington Post). Given concerns about misinformation, perceived truthfulness as well as competence could play a mediating role in evaluating reviews published in different outlets. In addition, ideological groups tend to be geographically clustered. Marketing communications sent by mail could feature different product reviewers depending on the postal code. Religiosity—another geographically clustered characteristic—is linked with moral objectivism (Goodwin & Darley, 2008), potentially increasing the power of this segmentation strategy.

In other contexts, the effect of shared values may be a threat rather than an opportunity. For example, if the strongest endorsement for a product comes from an expert who is known to be highly religious, this may undermine the effectiveness of this endorsement for secular consumers. Indeed, more than cultivating perceived competence by signaling shared values, it is probably especially important to avoid cultivating the perception of incompetence among sales staff by signaling unshared values (as in our opening anecdote), since negative effects are often more powerful than positive effects (Baumeister et al., 2001). One possibility is that framing endorsements from moral out-group members as an exception may prove more effective (e.g., “This is not normally the sort of book I read, but I couldn’t put it down!”). Such framing differentiates the product from the endorser’s typical judgment (which the ideological out-group may not respect) while maintaining the positive content of
the message. More generally, understanding how message framing interacts with shared and unshared values may be of great practical significance.

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Appendix A: Supplementary Studies

Pretest: Norming Expert Biographies for Moral Values

We recruited 100 participants (M_{age} = 37.7, 61% female). Participants were introduced to one of the two book critics used in Study 1A and completed the same multiple-choice comprehension questions. Next, participants were asked to predict the critic’s responses to the egalitarianism and communitarianism scales, with the biography still displayed.

Three results emerged. First, participants perceived a modest correlation between egalitarianism and communitarianism (adjusting for which expert), r_p(97) = .21, p = .041. This is a reasonably accurate perception: Across the studies reported in the main text and Supporting Information, these two scales were moderately correlated among participants, r(3310) = .32, p < .001. Consequently, all regression models in this paper include communitarianism as a covariate when testing the effect of egalitarianism. Second, participants perceived the experts as differing widely in egalitarianism (b = 2.08, SE = 0.25, p < .001), adjusting for communitarianism. Third, participants did not perceive the experts as differing in communitarianism (b = 0.11, SE = 0.29, p = .72), adjusting for egalitarianism. Thus, the experts indeed differ in egalitarianism, but are roughly equated for communitarianism.

Study S1: Replication with Consequential Choices

Study S1 sought to address two methodological concerns about Study 1. First, the purchase intention ratings were hypothetical rather than consequential choices. The seriousness of this issue is ameliorated by the finding that participants’ information-seeking behavior (clicking on links to Amazon pages) followed the same pattern as their overt ratings. Nonetheless, Study S1 further addressed this issue by entering participants into a lottery to receive a book they had chosen (as do Studies 4, S3, and S4). Second, the other studies reported in this article rely on online samples of U.S. consumers from Mechanical Turk. Although such samples are arguably more representative of the population than more traditional samples of undergraduates, it is useful to show that effects converge in different populations. To this end, Study S1 sought to replicate the findings with British university students.

Method

We recruited 23 participants (78% female) from business modules at an elite British university. Participants were offered the opportunity to volunteer for the study in exchange for entry into a lottery to win a book chosen in the study in addition to an Amazon gift card. Participants were excluded (N = 1) using the same criteria as Study 1A.

The procedure was the same as Study 1A, except that books were reviewed in five pairs of two books (i.e., ten books total) and participants chose between these books (i.e., five binary choices). For the first pair, the critics both rated one book highly and the other book poorly. For the remaining pairs, the critics disagreed, with the hierarchist critic giving a high rating to one book and low rating to the other book, and the egalitarian critic giving the opposite ratings. After reading both critics’ reviews of both books in a pair, participants were asked, “Which of these two books would you prefer
to receive?" as a forced choice. After the experiment, one participant was selected to receive one of these choices. The assignment of reviewers to books and order of the reviews were counterbalanced.

![Figure S1. Scatter plot of participants' choices as a function of their egalitarianism in Study S1.](image)

**Results and Discussion**

The results replicated Study 1A (Figure S1). We calculated the number of times (out of 4) that participants chose the book recommended by the egalitarian rather than hierarchist critic. In a regression analogous to Study 1A, these scores were significantly predicted by egalitarianism ($b = 0.86$, $SE = 0.40$, $p = .043$) but not communitarianism ($b = 0.08$, $SE = 0.22$, $p = .71$).

Overall, these results are consistent with Study 1, with a similarly large magnitude. Together with the effects on information-seeking from Study 1, these results suggest that value-based expert deference is likely to extend to consequential choices made by consumers.

**Study S2: Replication of Mediation Study**

Study S2 sought to replicate the mediation effects found in Study 2, with three main changes. First, Study S2 used books (as in Study 1A) rather than consumer electronics, to examine whether the mediating factors differ across product categories. Second, Study S2 measured purchase intentions (as in Study 1) rather than willingness-to-pay, as a further robustness check. Finally, Study S2 manipulated advisor gender orthogonally to advisor values, to test whether value similarity plays an outsized role compared to a different, highly salient dimension of similarity. Finding a stronger effect of shared values rather than shared gender would constitute some initial evidence for the especially powerful role of moral similarity compared to other dimensions.
Methods

We recruited 200 participants ($M_{age} = 38.5$, 57% female). Participants were excluded ($N = 7$) based on the same criteria used in Studies 1A–B.

The procedure was the same as Study 1A, except two changes. First, one critic was male and the other female, with their values counterbalanced so that for some participants, the male critic was the egalitarian (and the female hierarchist) and sometimes the female critic was the egalitarian (and the male hierarchist). The biographies of the critics were lightly altered, and the hobbies changed to reflect gender-stereotypical activities (e.g., brewing beer for the male critic and gardening for the female critic) to further differentiate the critics on gender. The photographs used were royalty-free stock photos. Second, the scales used in Study 2 (similarity, truthfulness, and competence) were included for each critic after the main task.

Results

Overall, the effect of shared values on purchase intentions was replicated, but there was no effect of shared gender. This supports the idea that shared values are particular influential in selecting experts. Moreover, the mediation by perceived competence was significant, mirroring Study 2.

As in Study 1, a regression was conducted with the difference in average purchase intention ratings between the egalitarian and hierarchist experts as the dependent variable. Replicating Study 1, egalitarianism significantly predicted these difference scores ($b = 4.84$, $SE = 2.04$, $p = .019$), while communitarianism did not ($b = 0.74$, $SE = 1.74$, $p = .67$). Thus, shared moral values once again guide expert deference.

![Figure S2](image-url)

**Figure S2.** Parallel mediation of values–deference relationship by competence and truthfulness in Study 2.

Shared gender, however, had no such effect. We fit two regression models, predicting deference separately to the female and the male experts. As predictors we entered participant gender, a dummy-code for counterbalancing condition (0 = female-egalitarian/male-hierarchist, 1 = female-hierarchist/male-egalitarian), and their interaction, in addition to communitarianism and egalitarianism. The only significant effect was the effect of counterbalancing condition on deference to the female expert ($b = -11.02$, $SE = 4.79$, $p = .023$). Specifically, participants deferred less to female experts who expressed hierarchist values, perhaps because people prefer experts whose moral views are consistent with gender stereotypes. The converse effect for male experts did not reach significance, however, nor did any main effect or interaction with participant gender. The key point is that
participants were no likelier to defer to experts of the same gender, pointing to a special role for shared moral values but not shared gender.

Next, we turned to mediation evidence to understand the mechanisms underlying this relationship between shared values and deference. We pitted perceived competence and truthfulness against one another in a parallel mediation model (PROCESS Model 4), as depicted in Figure S2. The shared values → competence → purchase intention path was significant ($b = 3.00$, 95% CI[0.40,6.40]) while the shared values → truthfulness → purchase intention path was not ($b = 0.00$, 95% CI[–1.77,2.32]). These results are very similar to Study 2.

**Study S3: Replication of Moral Objectivism Moderation**

The goal of Study S3 was to replicate the moderation by moral objectivism found in Study 3. This replication study differs from Study 3 in omitting the similarity measures but including a more detailed measure of competence.

**Methods**

We recruited 398 participants ($M_{age} = 36.8$, 55% female). Participants were excluded ($N = 23$) based on the same criteria used in previous studies.

The procedure was the same as Study 3, except for three changes. First, the similarity questions were omitted. Second, perceived competence was measured with separate four-item scales for each expert (e.g., “John’s opinions about books are likely to be dependable” and “John cannot distinguish between high and low quality books” [reversed]; $\alpha > .64$). Third, rather than answering the moral objectivism questions with respect to their general moral beliefs, they reported their beliefs about the objectivity of the values reported on the egalitarianism and communitarianism scales (“Considering your answers to the questions on the previous two pages, how strongly do you agree or disagree with each of these statements?”).

**Results and Discussion**

Replicating other studies, differences in the composite product ratings (averaging attitude and purchase intention ratings) across advisors were predicted by egalitarianism ($b = 0.82$, $SE = 0.16$, $p < .001$) but not communitarianism ($b = –0.03$, $SE = 0.15$, $p = .82$). These effects also manifested in participants’ book choices. In a binary logistic regression, choices were predicted by egalitarianism ($\chi^2 = 2.49$, $p = .013$) but not communitarianism ($\chi^2 = 0.15$, $p = .88$). A participant one SD above the egalitarianism scale midpoint would be expected to choose the egalitarian-condensed book 57% of the time, compared to 39% for a participant one SD below the midpoint.

The mechanistic story also matched Study 4. A moderated mediation model (PROCESS Model 7) was fit, using egalitarianism as the independent variable, difference scores in product ratings as the dependent variable, difference scores in perceived competence as the mediator, and the subjectivism subscale of the moral objectivism scale as the moderator (Figure S3); this subscale was used since it was the subscale that drove the moderation in Study 4. Once again, egalitarianism (but not communitarianism) predicted differences in perceived competence ($b = 1.27$, $SE = 0.15$, $p < .001$), with this effect qualified by the significant interaction with the subjectivism subscale ($b = 0.31$, $SE = 0.13$, $p = .018$). The mediating effect via perceived competence was thus lower for participants 1 SD below the mean in objectivism ($b = 0.66$, 95% CI[0.33,1.02]) compared to those 1 SD above the mean ($b = 1.11$, 95% CI[0.76,1.45]). This led to a marginally significant index of moderated mediation ($b =$
The corresponding index for the equivalent model using the normativity component did not reach significance ($b = 0.09$, 95% CI $[-0.12, 0.29]$).

These results help to buttress the results of Study S4 by replicating the key moderation—once again, it is the subjectivity subscale, not the normativity subscale, that moderates the link between shared values and perceived competence. Moreover, we add further empirical support by measuring competence on a four-item scale, gaining a broader grasp of this key construct.

**Study S4: Manipulating Moral Objectivism**

Studies 4 and S3 treated moral objectivism as an individual difference. Could we induce similar effects through a manipulation of moral objectivism? This was the goal of Study S4. (To forestall the reader's optimism, we note that the manipulation was not entirely successful, but we report it nonetheless in the interest of completeness.)

A subsidiary goal was to alleviate one possible concern about our objectivism measure, namely that it could be confounded with the extremity of one's values. That is, people who claim that they view moral truths as objective could simply be those who have particularly extreme egalitarian or hierarchist views. Study 4 does take steps to avoid this problem, in two ways. First, these values were measured, so that our models would pick up only the variance due to moral objectivism over-and-above the reported levels of egalitarianism. Second, the value scales were measuring one's attitudes about specific moral values (e.g., egalitarianism), whereas the objectivism scale was measuring one's general beliefs about morality. Study S3, in contrast, measured one's objectivism toward the specific moral beliefs of egalitarianism and communitarianism. Since these two approaches yielded similar results, it seems unlikely that the extremity confound explains the moderation.

Nonetheless, Study S4 sought to further rule out this concern. First, we manipulated objectivism rather than measuring it. Second, we conducted two versions of Study S4, varying in whether values were measured before or after the objectivism manipulation. If objectivist meta-ethics translate into more extreme moral values, then we would expect higher variance in values when the values scales are administered after, rather than before, the objectivism manipulation.

**Methods**
We recruited 1201 participants \((M_{\text{age}} = 38.1, 61\% \text{ female})\) for Studies S4A \((N = 601)\) and S4B \((N = 600)\). Participants were excluded \((N = 73)\) based on criteria similar to previous studies (see below).

The procedure for Study S4A was the same as Study 3, with one important change. Before the main task, participants were given a reading comprehension task (“This study will require you to read and comprehend some information. To familiarize you with this procedure, we ask you to read the following brief passage and answer the following questions to verify your comprehension”). Participants in the objectivism condition read the following passage:

Many philosophers have argued that morality is fundamentally objective. This position has been adopted by a number of important moral philosophers, ranging from Immanuel Kant in the 18th century to Thomas Nagel today. According to these philosophers, moral values are facts, like the circumference of the Earth or the solution to a mathematical equation. In support of this position, anthropologists have discovered that some moral values are widely held across cultures, both across the span of history and across the modern globe. For example, numerous cultures have believed that one has a moral obligation to treat others as one wishes to be treated. Thus, according to this important philosophical position, moral values reflect a universal tendency among humans to tap into deep moral truths.

In contrast, participants in the subjectivism condition read the following passage:

Many philosophers have argued that morality is fundamentally subjective. This position has been adopted by a number of important moral philosophers, ranging from Friedrich Nietzsche in the 19th century to Gilbert Harman today. According to these philosophers, moral values are opinions, like the beauty of a painting or the best-tasting ice cream. In support of this position, anthropologists have discovered that some moral values vary widely across cultures, both across the span of history and across the modern globe. For example, some cultures have believed that people owe their primary moral duties to their families, whereas other cultures have believed that people owe their primary moral duties to their society. Thus, according to this important philosophical position, moral values reflect arbitrary preferences that some people have, rather than any kind of deep moral truth.

The information given in both passages is true (although most moral philosophers subscribe to other, more nuanced, positions; of the two extremes, variations on objectivism tend to be more popular among philosophers). Participants then answered five multiple choice questions about their passage, and were excluded if they answered more than 30% incorrectly. Participants were also excluded if they failed the criteria used in other studies. Study S4A was otherwise identical to Study 3, except that the similarity questions were omitted.

Study S4B was similar to S4A, with the main difference being that the egalitarianism and communitarianism scales were given before the objectivism manipulation and main task, rather than after. This was done to avoid the possibility that the objectivism manipulation itself contaminated the value scales. Participants were instructed that they would participate in a study about “how individual characteristics are related to book preferences,” and that these measurements would occur in three phases. The first phase measured demographics, including the egalitarianism and communitarianism scales. The second phase measured reading comprehension, using the same procedure as Study S4A. The third phase measured book preferences, using the same procedure as S4B. The moral objectivism scale was included at the end of the study as a manipulation check. Participants were excluded on the same basis as Study S4A, except the comprehension questions about the advisors’ biographies were omitted to disguise the true purpose of the study (and therefore not used for exclusion).

**Results and Discussion**

Participants in the objectivism condition had more objectivist scores on subjectivism subscale \((M = 2.92, SD = 1.21)\) compared to those in the subjectivism condition \((M = 2.67, SD = 1.19)\), \(t(1127) = \)
3.43, \( p < .001 \). Although the manipulation led to a statistically significant difference, it is worth noting that the effect size was quite small (\( d = 0.20 \)).

Despite the manipulation, there was no evidence that the objectivism manipulation affected the variance in the values scale, as it would if it pushed people toward the extremes of the scale. In Study S4A, where values were measured after the manipulation, variance was similar across the objectivism and subjectivism conditions [\( SD = 1.25 \) vs. 1.27; \( F(1,1560) = 0.08, p = .78 \), Levene’s test]. Moreover, variance was similar across Studies S4A and S4B, despite differing in whether the objectivism manipulation occurred after or before values measurement [\( SD = 1.26 \) vs. 1.21; \( F(1,1127) = 1.83, p = .18 \)]. Similar results held for the variance in communitarianism scores across conditions of Study S4A [\( SD = 0.95 \) vs. 0.93; \( F(1,1560) = 0.06, p = .81 \)] and across Studies S4A and S4B [\( SD = 0.94 \) vs. 0.95; \( F(1,1127) < 0.01, p = .95 \)]. This casts further doubt on the deflationary explanation of Study 4, that the moderation results were due to confounds between objectivism and value extremity.

As for the main findings, like in previous studies, differences in the composite product ratings (averaging attitude and purchase intention ratings) across advisors were predicted by egalitarianism (\( b = 0.95, SE = 0.09, p < .001 \)) but not communitarianism (\( b = 0.09, SE = 0.08, p = .27 \)). Likewise, choices were predicted by egalitarianism (\( z = 5.82, p < .001 \)) but not communitarianism (\( z = 0.04, p = .57 \)) in a logistic regression. A participant one SD above the egalitarianism scale midpoint would be expected to choose the egalitarian-endorsed book 62% of the time, compared to 37% for a participant one SD below the midpoint.

![Figure S4. Moderated mediation of values–deference relationship by perceived competence (mediator) and moral objectivism (moderator) in Study S4.](image)

Note that a model that includes study (S4A or S4B) and its interaction with egalitarianism revealed that the effect of egalitarianism was larger in Study S4B (\( p = .027 \)). This could be because participants were primed to think of their moral values by completing the values subscales before the main task, or because the comprehension check questions following the advisors’ biographies were removed, which could have produced reactance in the other studies, working against our hypothesis.

To test the effects of the objectivism manipulation on book choices, we fit a moderated mediation model (PROCESS Model 5), using the same specification as Study S3, except that the individual difference measure of objectivism was replaced by a contrast-coded variable for objectivism condition and study (S4A or S4B) was added as a covariate. This model tested moderation of our manipulation of both the direct and indirect pathways (Figure S4). As in previous studies, egalitarianism (but not communitarianism) predicted differences in perceived competence (\( b = 1.36, SE = 0.09, p < .001 \)). However, the interaction between egalitarianism and objectivism condition was not significant as a predictor of perceived competence differences (\( b = -0.09, SE = 0.09, p = .30 \)). Thus, the indirect
effect via perceived competence was not moderated by the objectivism manipulation, with the indirect pathway of similar magnitude in the objectivism ($b = 0.84$, 95% CI $[0.65,1.05]$) and subjectivism conditions ($b = 0.96$, 95% CI $[0.75,1.19]$). On the other hand, there was some evidence for moderation for the direct pathway. The interaction between egalitarianism and objectivism condition was marginally significant in predicting differences in product ratings ($b = 0.12$, $SE = 0.07$, $p = .087$). This interaction occurred because direct effect was directionally negative in the subjectivism condition ($b = -0.08$, 95% CI: $-0.28$ to $0.13$) but directionally positive in the objectivism condition ($b = 0.16$, 95% CI $[-0.05, 0.36]$).

This study replicated two key results—the large effect of moral values on expert deference (including incentive-compatible choices) and the mediating effect of perceived competence. In addition, it provides initial evidence that beliefs about moral objectivism can be manipulated, with some possible downstream effects on behavior. However, the manipulation was relatively weak, manifesting in a small difference in measured objectivism. Moreover, the mechanism differed from the one supported by Studies 4 and S3. In those previous studies, objectivism moderated the indirect pathway via competence, whereas in the current study the manipulation moderated the direct pathway. One possibility is that measured objectivism taps into more implicit attitudes about moral truth, whereas our manipulation was able only to influence explicit attitudes. Such explicit attitudes may play a weaker role in guiding expert deference. Nonetheless, we hope that reporting these results here can act as a catalyst for other researchers to further investigate the role of moral objectivism in consumer behavior.

**Appendix B: Additional Analyses**

**Multilevel Regression Analyses**

The analyses in the main text for Studies 1–3 treat participants as random and items as fixed, meaning that the analyses are designed to generalize across participants but not across items (i.e., they treat participants as randomly sampled from the population but items as exhausting the “population” of potential items). This approach has been long criticized (Clark, 1973), with recent authors (Judd, Westfall, & Kenny, 2012) recommending a multilevel modeling approach to treat both participants and items as random simultaneously. This approach allows the slope and intercept to vary for each participant and for each item, rather than averaging across items and simply looking at variability among participants. Here, we repeat the key tests of H1 for Studies 1–3, using this method. (Study 4, having only one observation per condition, does not lend itself to a multilevel approach.) The results are highly similar to those reported in the main text.

**Study 1**

To test H1 for Study 1, we model each participants’ purchase intention ratings for each of the 10 stimuli they rated (recall that two items were excluded from analysis due to an error in the materials). That is, the unit of analysis is each participant–item combination (i.e., 1600 and 1152 observations for Studies 1A and 1B, respectively, after participant exclusions). For each observation, we include as fixed effects (i) a contrast-coded recommender variable indicating whether the egalitarian advisor favored that product ($= 1$) or the hierarchist advisor favored that product ($= -1$); (ii) the participant’s egalitarianism and communitarianism scores, centered at their midpoints and scaled by their standard deviations (as in the main text analysis), and (iii) the interaction terms between egalitarianism and recommender and between communitarianism and recommender. In addition, we included four
random effects—random intercepts for item and participant and random slopes on recommender for item and participant.

<table>
<thead>
<tr>
<th>DV: Purchase Intention</th>
<th>Study 1A</th>
<th>Study 1B</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>39.41 (2.39)</td>
<td>39.35 (2.17)</td>
</tr>
<tr>
<td>Recommender</td>
<td>−1.92 (1.15)</td>
<td>1.40 (1.09)</td>
</tr>
<tr>
<td>Egalitarianism</td>
<td>2.33 (1.70)</td>
<td>−0.68 (1.34)</td>
</tr>
<tr>
<td>Egalitarianism x Recommender</td>
<td>5.46 (1.03)</td>
<td>4.12 (0.92)</td>
</tr>
<tr>
<td>Communitarianism</td>
<td>−0.31 (1.39)</td>
<td>2.20 (1.38)</td>
</tr>
<tr>
<td>Communitarianism x Recommender</td>
<td>0.16 (0.85)</td>
<td>−0.36 (0.94)</td>
</tr>
</tbody>
</table>

Table S1. Fixed effects in multilevel regression (Study 1).

Note. Entries are unstandardized bs and SEs.

The results were similar to those reported in the main text (Table S1). For Study 1A, the interaction between egalitarianism and recommender was significant [$F(1,156.13) = 28.01, p < .001$], using the Kenward–Roger approximation (Kenward & Roger, 1997) recommended by Judd et al. (2012) to compute $p$-values. This was also the case for Study 1B [$F(1,188.76) = 20.08, p < .001$]. The interaction between communitarianism and recommender was not significant in either study [$F(1,156.12) = 0.04, p = .85$ and $F(1,188,16) = 0.15, p = .70$]. For both studies, the results for egalitarianism are similar if communitarianism and its interaction with recommender are omitted as covariates.

<table>
<thead>
<tr>
<th>DV: Willingness-to-Pay (Proportion of Retail Value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$b$ (SE)</td>
</tr>
<tr>
<td>Intercept</td>
</tr>
<tr>
<td>Recommender</td>
</tr>
<tr>
<td>Egalitarianism</td>
</tr>
<tr>
<td>Egalitarianism x Recommender</td>
</tr>
<tr>
<td>Communitarianism</td>
</tr>
<tr>
<td>Communitarianism x Recommender</td>
</tr>
</tbody>
</table>

Table S2. Fixed effects in multilevel regression (Study 2).

Note. Entries are unstandardized bs and SEs.

Study 2

We performed a multilevel regression analysis for Study 2, using the same specification as described above, except using WTP as the dependent variable (Table S2). The interaction between egalitarianism and recommender was significant [$F(1,373.03) = 24.07, p < .001$], mirroring the finding in the main text. Unlike the analysis in the main text, where the communitarianism covariate was not
a significant predictor, the interaction between participant communitarianism and recommender reached significance \[ F(1,373.04) = 4.16, p = .042 \]. That said, it is worth noting that the coefficient on the egalitarianism interaction term was about three times larger and far more statistically robust. Moreover, the interaction between participant egalitarianism and recommender is highly significant regardless of whether communitarianism and its interaction are omitted as covariates.

**Study 3**

We also supplemented the analysis of Study 3 with a multilevel regression. Analogous to the multilevel analyses for Studies 1 and 2, this tested whether the effectiveness of a recommendation differed depending on the alignment between the recommender’s values and those of the participant. In Study 3, participants were randomly assigned to a single recommender with normative vs. counter-normative values (unlike Studies 1 and 2, where the two recommenders differed in egalitarianism vs. hierarchy) and each book was either recommended or dis-recommended by that single recommender (rather than having opposite recommendations from the two recommenders as in Studies 1 and 2). Thus, the key prediction of Study 3 is that the effect on purchase intention of whether a book is positively or negatively reviewed would be larger when the recommender has normative rather than counter-normative values—that is, an interaction between recommendation (pro or con) and recommender (normative vs. counter-normative values).

We tested this by fitting a multilevel model, analogous to the above, with the unit of analysis being each participant-item combination (2080 observations after the participant exclusions reported in the main text). Fixed effects include (i) a contrast-coded *recommender* variable indicating whether the recommender held normative (= 1) or counter-normative values (= −1); (ii) a contrast-coded *recommendation* variable indicating whether the recommendation was positive (= 1) or negative (= −1), (iii) the interaction term between these two variables. The model also included a random intercept for item and for participant, and a random slope on recommender for item (but not for participant, since the manipulation was between-subjects).

As shown in Table S3, the results are similar to those reported in the main text. There was a large effect of recommendation (pro or con), indicating that people based their purchase intentions in part on the recommendation provided. But the significant interaction between recommendation and recommender \[ F(1,1805.79) = 10.46, p = .001 \] indicates that these effects were larger when the recommender espoused normative rather than counter-normative values.

<table>
<thead>
<tr>
<th></th>
<th>( b ) (SE)</th>
<th>( t )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>41.63 (1.65)</td>
<td>25.21</td>
</tr>
<tr>
<td>Recommender</td>
<td>4.02 (1.06)</td>
<td>3.80</td>
</tr>
<tr>
<td>Recommendation</td>
<td>18.95 (0.46)</td>
<td>41.30</td>
</tr>
<tr>
<td>Recommendation x Recommender</td>
<td>1.48 (0.46)</td>
<td>3.23</td>
</tr>
</tbody>
</table>

**Table S3.** Fixed effects in multilevel regression (Study 3).

*Note.* Entries are unstandardized \( bs \) and \( SEs \).
Further Mediation Analyses

Study 1

In the main text, we reported that shared values led not only to higher purchase intentions for products recommended by an expert, but also a greater tendency to seek out (click on) information related to that product. Here, we test whether purchase intentions mediate the relationship between shared values and information seeking, which would be further evidence for the proposed process and for the practical importance of measured purchase intentions (our main dependent measure across most studies).

Indeed, we find that the relationship between shared values and clicks was mediated by purchase intention ratings (PROCESS Model 4), with the shared values → purchase intentions → information-seeking path reaching significance in Study 1A ($b = .0267, 95\% \text{ CI} [.0127,.0446]$) and marginal significance in Study 1B ($b = .0053, 95\% \text{ CI} [-.0013,.0152]$), as shown in Figure S5.

![Figure S5. Mediation of values–information-seeking relationship by purchase intention in Study 1A (books; top panel) and Study 1B (electronics and appliances; bottom panel).](image)

Study 2

In the main text, we tested parallel mediation models, pitting perceived competence against perceived truthfulness. Since we also measured similarity, plausibly it is appropriate to test for serial mediation as well (PROCESS Model 6). As shown in Figure S6, the shared values → similarity → competence → WTP path was significant ($b = .0310, 95\% \text{ CI} [.0122,.0531]$), while the shared values → similarity → truthfulness → WTP path reached only marginal significance ($b = .0174, 95\% \text{ CI} [-.0017,.0372]$).
Figure S6. Serial mediation of values–deference relationship by similarity and competence (top panel) and similarity and truthfulness (bottom panel) in Study S2.

Alternate Exclusion Rules

Here, we check the robustness of the Study 3 results by analyzing the results using two alternative exclusion rules with respect to the manipulation check question (reported moral similarity).

In the main text, we report a preregistered analysis that excludes the 20% of participants reporting the lowest similarity in the normative expert condition (less than or equal to 5) and the 20% of participants reporting the highest similarity in the counter-normative expert condition (greater than or equal to 7.1). In this analysis, there is still considerable overlap in the distribution of perceived similarity between the two conditions.

No exclusions

A second pre-registered analysis does not exclude any participants based on the manipulation check question. As in the analysis reported in the main text, there is a highly significant effect of condition on perceived competence [$M = 7.42, SD = 1.82$ vs. $M = 6.56, SD = 2.23$; $t(348) = 3.93, p < .001, d = 0.42$] and a strong correlation between perceived competence and deference [$r(348) = .45, p < .001$]. However, the overall effect of condition on deference here is not significant [$M = 38.6, SD = 28.1$ vs. $M = 37.3, SD = 26.6$; $t(348) = 0.43, p = .67, d = 0.05$]. The results are similar using a multi-level approach (using the same specification reported above).

The mediation analysis sheds some light on this. Just as in the analysis reported in the main text, there is a robust indirect effect of condition on deference via perceived competence [$b = 4.16, 95\% CI[1.69,7.34]]$ but not via perceived truthfulness [$b = 0.78, 95\% CI[-0.27,2.30]]$. However, when these indirect effects are accounted for there is a numerically negative (though non-significant) direct effect of condition on deference [$b = -3.68, 95\% CI[-8.94,1.57]]$. This trend counteracts the significant indirect effect, making it non-significant under these loose exclusion criteria. It is unclear what accounts for this trend or whether this is best chalked up to noise.
The pre-registered analysis in the main text led to greater-than-anticipated overlap in perceived similarity across the two conditions. A natural way to address this problem is to exclude all participants from the normative expert condition reporting similarity less than or equal to the scale midpoint (5) and all participants from the counter-normative expert condition reporting similarity greater than or equal to the midpoint. This analysis is exploratory as it was not pre-registered.

As in the other analyses, there is a large effect of condition on perceived competence \( M = 7.91, SD = 1.39 \) vs. \( M = 5.32, SD = 2.30; t(199) = 9.97, p < .001, d = 1.44 \) and a strong correlation between perceived competence and deference \( r(199) = .46, p < .001 \). There was also an effect of condition on deference \( M = 41.2, SD = 29.0 \) vs. \( M = 29.9, SD = 24.9; t(199) = 2.87, p = .005, d = 0.41 \), suggesting that the weaker effects reported with looser exclusion rules are due to the noise associated with the overlapping distributions of perceived moral similarity. The results are similar using a multi-level approach (using the same specification reported above).

The mediation results are similar to the results using other exclusion rules. There is once again a significant indirect effect of condition on deference via perceived competence \( [b = 16.37, 95\% CI[6.88,26.64]] \) but not via perceived truthfulness \( [b = 0.80, 95\% CI[-5.71,7.91]] \). The remaining direct effect was non-significantly negative \( [b = -5.83, 95\% CI[-14.47,2.81]] \), but with this more exclusive exclusion rule the positive indirect effect is strong enough to overpower this negative direct trend, resulting in the overall significant effect of condition on deference seen above.