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Political ingroup conformity and pro-environmental behavior: Evaluating the evidence from a survey and mousetracking experiments

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ABSTRACT

Previous work reveals that political orientation is a relevant social identity for many people and that the desire to conform to political ingroup norms can drive belief and behavior change. Because pro-environmental behaviors are viewed as stereotypically liberal in the US, American conservatives may be less likely to engage in pro-environmental behavior, particularly when political identity and normative information are made salient. In four studies, we examine whether heightening the salience of political identity and providing information that one is conforming to or failing to conform to political group norms influences engagement in a pro-environmental behavior (recycling). Study 1 showed that undergraduates falsely believed that liberal students at their university recycled more than conservatives. In turn, while liberal and moderate students' self-reported recycling behavior was predicted by their perceptions of liberals' (but not conservatives') behavior, conservative students' behavior was predicted by perceptions of other conservatives' (but not liberals') behavior. Studies 2–4 use a novel computerized recycling task and mouse-tracking software to examine whether, among politically conservative Americans, receiving feedback that their recycling behavior is inconsistent with stereotypic ingroup norms modifies behavior and motivates individuals to "recycle" less in the computerized task. In Studies 2 (university student sample) and 3 (preregistered; MTurk worker sample), those who received this feedback adjusted their automatic, but not deliberate responses, although patterns differed slightly between studies. However, in Study 4 (preregistered; MTurk worker sample), this effect was not found. Collectively, these studies suggest that inaccurate meta-beliefs may drive political polarization with respect to pro-environmental behavior, but inconsistencies in results across studies leave open questions about how they do so. This research also contributes to the literature by introducing new methodologies to study pro-environmental decision-making processes.

1. Introduction

In countries such as the United States (US), concern about climate change and environmental issues more generally is becoming increasingly politically polarized (Pew Research Center, 2019), and this divergence filters down into pro-environmental behaviors and purchases at the individual level (Sexton & Sexton, 2014). Multiple factors explain this political bifurcation, including differences between political liberals and conservatives in underlying moral foundations and values (Dickinson, McLeod, Bloomfield, & Allred, 2016; Farrell, 2013; Feinberg & Willer, 2013; Milfont, Davies, & Wilson, 2019) and desires to remain cognitively consistent (Baron & Jost, 2019; but also see Gehlbach, Robinson, & Vriesema, 2019). Yet, recent research increasingly notes the role of social identity processes in determining pro-environmental behavior (Bashir, Lockwood, Chasteen, Nadolny, & Noyes, 2013; Fritsche, Barth, Jugert, Masson, & Reese, 2018; Geiger & Swim, 2018; Kahan, 2012a; Schuldt & Pearson, 2016) and that political orientation may be a particularly relevant and understudied social identity

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elucidating these processes (Ehret, Van Boven, & Sherman, 2018; Kahan, 2012a; Van Boven, Ehret, & Sherman, 2018). Specifically, as we explain in more detail below, the desire to conform to one's political ingroup and distance from one's outgroup might drive pro-environmental attitude and behavior change.

In the present research, we examine whether *perceived norms for political ingroups and outgroups* (with regard to pro-environmental behaviors) might magnify political variation in pro-environmental behavior. Although there are sizeable genuine differences between political liberals and political conservatives on many environmental issues, these differences are perceived to be even larger than they actually are (Van Boven et al., 2018). In particular, people tend to underestimate conservatives' environmental concern (while being fairly accurate with regard to estimates of liberals' environmental concern; Van Boven et al., 2018). In turn, exaggerated perceptions of differences can enhance behavioral polarization, motivating political liberals to conform to the supposed norms of their political ingroup—by appearing pro-environmental—and political conservatives to conform to the supposed norms of their ingroup—by purposefully not appearing pro-environmental (Ehret et al., 2018; Sexton & Sexton, 2014).

2. Social identity and political polarization

The present work draws from decades of research on *social identity theory,* which suggests that individuals have a basic desire to conform, in their thoughts and actions, to ingroup members and to differentiate themselves from outgroup members (Branscombe, Ellemers, Spears, & Doosje, 1999; Hogg, Turner, & Davidson, 1990; Hogg & Reid, 2006; Sherif, 1961; Steele, 1988; Tajfel & Turner, 1979). Notably, the desire to 'fit in' with ingroup members and maintain a strong social identity can outweigh individuals' desire to be accurate (Festinger, 1954; Van Bavel & Pereira, 2018). While individuals generally are predisposed to modify their behavior to conform to ingroup norms, this motivation tends to exert a greater influence on behavior change when ingroup norms are made salient and conflict with an individuals' existing behavior (Cialdini et al., 1990, 1991; Terry & Hogg, 1996; Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). The potential for social identity processes to interact with normative influence is made clear by research on masculinity, which finds that men tend to engage in compensatory hyper-masculine behavior when led to believe that they fail to adhere to salient group stereotypes (Bosson, Vandello, Burnaford, Weaver, & ArzuWasti, 2009; Bosson & Vandello, 2011; Dahl, Vescio, & Weaver, 2015; Vandello, Bosson, Cohen, Burnaford, & Weaver, 2008; Weaver & Vescio, 2015).

There is a growing consensus that, in addition to representing a coherent ideology or stance on relevant policies, self-described political orientations reflect social identities. Although political identities and policy preferences sometimes align, in other cases individuals' political self-identification (i.e., self-described ideological and partisan labels) can clash with their actual ideologies and beliefs (Brandt, Sibley, & Osborne, 2019; Conover & Feldman, 1981; Mason, 2018). As a result, in some cases individuals adjust their political beliefs and behaviors to conform to their perceptions of what others in their political ingroup are thinking and doing (Carmichael & Brulle, 2017; Ehret et al., 2018) and distance themselves from political outgroup members (Geiger & Swim, 2018; Mason, 2013). In this manner, social identity processes can influence a range of political beliefs and behaviors, such as voting preferences and civic engagement on specific issues (also see Mason, 2018; Van Bavel & Pereira, 2018).

Although mismatches between individuals' political identities and policy support exists at both ends of the political spectrum, in the US, it may be particularly common among those who identify as politically conservative. In the US, a sizable minority of individuals self-identify as conservative and vote for conservative politicians, yet hold left-leaning positions on most political topics (the converse phenomenon amongst self-identified political liberals is less common; Feldman & Johnston, 2014; Mason, 2018). This reality is clearly demonstrated within the environmental domain: a majority of American Republicans (the conservative party in the US) prefer pro-environmental policies such as regulating CO_2 emissions and increasing renewable energy funding (Howe, Mildenberger, Marlon, & Leiserowitz, 2015), yet (at the time this paper was written) an overwhelming majority of Republicans also express approval for President Donald Trump, whose views on climate change align closely with the organized climate denial movement (see Oreskes & Conway, 2011) and who opposes the aforementioned environmental policies (Shear, 2018).[2] In most representative democratic systems, especially those with only two viable political parties, such disconnects between citizen and elite attitudes might be expected to arise on certain issues, in particular on those not important enough or for which the disconnect is not salient enough for individuals to reevaluate their support for elites based on that particular issue. For example, in the past there was often a disconnect between elite attitudes and supporter attitudes on abortion, though these attitudes have converged over time as the issue has become more salient (see Adams, 1997).

Even if the disconnect between one's views and elite views on a particular issue is not typically salient, individuals may have enough of a baseline awareness of political stereotypes such that "cueing" (or making salient) the relation between a domain and political identity could alter attitudes and behaviors within this domain as individuals attempt to conform to norms of the ingroup. This can lead individuals to support policies that contradict their own personal values (Cohen, 2003) and to accept misleading or 'fake' news (Pereira & Van Bavel, 2018). Interventions designed to counter social identity threat, such as values affirmation interventions, have been shown to reduce conformity and to increase willingness to negotiate and compromise across group lines (Binning, Brick, Cohen, & Sherman, 2015; Cohen et al., 2007). The fact that these interventions can counteract conformity goals serves as further evidence of the powerful role that social identity processes can play in guiding political behavior.

3. Automatic and deliberate processes

Recent research demonstrates that conformity can influence both automatic and deliberate decision-making processes (Burdein, Lodge, & Taber, 2006; Van Bavel & Pereira, 2018). Thus, in addition to seeking to elucidate *whether* social identity processes drive political conformity (and as a result polarization), we seek to identify the cognitive pathways through which they do so.

According to many accounts (e.g., Kahan, 2012b), political conformity typically occurs via the deliberative process of motivated reasoning—what Pennycook and Rand (2019) refer to as *Motivated System 2 Reasoning*—by which individuals are consciously motivated to reach a specific conclusion in advance and selectively seek out and process information in a manner conducive to reaching this conclusion (Kunda, 1990; Taber & Lodge, 2006). Motivated reasoning occurs when individuals hold identity-protective goals (Kahan, 2010); in these situations, individuals alter their political opinions to conform to positions believed to be endorsed by political ingroup members (Cohen, 2003; C. T. Smith, Ratliff, & Nosek, 2012) and selectively draw upon values that allow them to accept claims made by political ingroup members without cognitive inconsistency (Kahan, 2015). Because motivated reasoning is a deliberative process, evidence for motivated reasoning would be provided by political conformity manipulations exerting effects under conditions or within timeframes in which deliberation could occur. That is, if people deliberately conform to their political ingroup's behavior as it relates to pro-environmental behavior, manipulations making political

[2] Although this disconnect may partly reflect the low importance of environmental opinions in determining candidate preference among many Republicans (Pew Research Center, 2019), many Republicans also largely support progressive policies across many other domains (Ellis & Stimson, 2012).

identities and stereotypical behavior salient should decrease pro-environmental behavior among conservatives (or increase pro-environmental behavior among liberals) after individuals have time to deliberately control their behavior (provided that they have cognitive resources and motivation to control their behavior in a particular situation).

Automatic processes, which involve individuals' initial responses to stimuli prior to conscious deliberation, can also be influenced by political and other social identities (Brick & Lai, 2018; Gampa, Wojcik, Motyl, Nosek, & Ditto, 2019; Kahan, 2015). For example, individuals express implicit preferences (i.e., preferences that individuals did not indicate awareness of; reflecting automatic processes) for policies proposed by political ingroup members versus outgroup members (C. T. Smith et al., 2012). Pennycook and Rand (2019) demonstrate that susceptibility to partisan fake news appears not to be driven by cognitive reflection and deliberation, but rather by the lack thereof. Evidence for automatic processes influencing decision making can be found if behavior changes when individuals are unwilling or unable to pay close attention to the situation (Petty & Cacioppo, 1986), such as shortly after receiving a stimuli and before deliberative cognitive processes can influence decisions. Thus, if social identity processes motivate people to conform at an automatic level to stereotypic behavior in the environmental domain, increasing the salience of political identities and leading people to believe that they are behaving in counter-stereotypic ways should decrease pro-environmental behavior among conservatives (or increase pro-environmental behavior among liberals) before individuals have time to deliberately control their behavior (or in situations where they do not have cognitive resources or motivation to control their behavior).

4. Present research: do political ingroup conformity pressures affect environmental decision-making?

We report results of four studies—two studies with undergraduate students and two studies with MTurk worker samples— that examine the potential for social identity processes to motivate political conformity and polarization in environmental decision making. For the purpose of these studies, we operationalize pro-environmental behavior as recycling. We use recycling behavior as our operationalization for two reasons. First, recycling is a well-known prototypical pro-environmental behavior that we expected to be well-understood among both college and adult samples. Second, recycling is a fairly mundane behavior that we anticipated most across the political spectrum would hold positive attitudes towards (thus reducing the likely effect of individual differences in attitudes toward the behavior) and might have substantial variance in their perceptions of politicization (we verify these assumptions in Study 1). We operationalize political identity as liberal and conservative (as opposed to Democrat or Republican) for purposes related to the manipulation used in Studies 2–4 (to allow for a meaningful midpoint on a continuum).

In Study 1, we examined whether self-reported recycling was predicted by perceptions of the extent to which the political ingroup and outgroup recycled. In Studies 2–4, we experimentally manipulated perceived conformity to political ingroup environmental norms among three samples of politically conservative participants. We then used mousetracking software to examine unfolding decision-making processes and thereby assess both automatic and deliberate decision-making processes.

5. Study 1

Study 1 is a correlational study in which we first examined (a) the extent to which recycling is perceived to be a politicized behavior (i.e., if liberals are perceived to recycle more than conservatives) and (b) whether this perception reflected actual differences in self-reported recycling behavior based on political identification. Next, we examined whether perceptions of liberals' and conservatives' recycling behavior predicted individuals' own recycling behavior and whether this relation differed based on individuals' own political identity.

5.1. Method

5.1.1. Participants

We recruited 96 participants from The Pennsylvania State University psychology department subject pool who received course credit in exchange for participation. All participants gave informed consent to participate in the research. After removing data from 10 students who reported not being US citizens[3] and an additional two students who failed all of three attention checks, our final sample consisted of 84 university students. A power analysis using G*Power 3.1 (Faul, Erdfelder, Lang, & Buchner, 2007) indicated that this sample size yielded a power of .99 to detect a medium-sized difference between two paired means, and a power of .94 to detect a single medium-sized effect in a regression.

5.1.2. Measures

Participants completed the following measures on recycling behavior and similar measures regarding a list of other behaviors that were not used in the present work. For a full list of measures see https://osf.io/nsybc/. See Table 1 for descriptive statistics and a zero-order correlation table.

5.1.2.1. Political orientation. Participants indicated their political orientation on an 8-point "Very Liberal" to "Very Conservatives" scale, which was coded such that -3.5 was the most liberal, and +3.5 was the most conservative, with 0 being the scale mid-point.

5.1.2.2. Perceptions of others' recycling. Participants indicated the extent to which they believed that (a) politically liberal students at their university recycled, and (b) politically conservative students at their university recycled on single-item scales from 1 (*Never*) to 7 (*Always*).

5.1.2.3. Recycling behavior (self-report). Participants self-reported their own recycling behavior on a single-item scale from 1 (*Never*) to 7 (*Always*).

5.2. Results

More details of analyses and R code can be found at https://osf.io/nsybc/.

5.2.1. Preliminary tests

A paired-sample *t*-test revealed that overall, participants believed that liberal students at their university ($M = 5.04$, $SD = 1.10$) recycled more than conservative students ($M = 4.65$, $SD = 1.37$), $M_{diff} = 0.38$, *95% CI*$_{diff}$ [0.05, 0.72], $t(83) = 2.26$, $p = .03$, Cohen's $d = 0.31$. Yet, contrary to this perception, we did not find a statistically significant relationship between participants' own self-reported recycling behavior and political identity (although results were trending in the expected direction), $b = -0.15$, $SE = 0.09$, *95% CI* [−0.33, 0.04], $t(82) = -1.57$, $p = .12$, $\eta_p^2 = 0.03$.[4]

5.2.2. Primary analyses

To examine whether individuals' own (self-reported) recycling

[3] We made the a priori decision to exclude noncitizens' information because there were many international students at the specific university and we anticipated that some might not be socialized into American political identities.

[4] Note Table 1 results showing that this effect is statistically significant when controlling for perceptions of the extent to which liberal and conservative students recycle, $p = .04$.

Table 1
Means, standard deviations, and correlations with confidence intervals.

Variable	*M*	*SD*	1	2	3
1. Self-reported recycling frequency	4.49	1.44			
2. Perceptions of conservative students' recycling frequency	4.65	1.37	.12 [-.09, .33]		
3. Perceptions of liberal students' recycling frequency	5.04	1.10	.36** [.16, .53]	.23* [.02, .43]	
4. Political conservatism	−0.04	1.67	-.17 [-.37, .04]	.12 [-.10, .32]	.10 [-.12, .30]

Note. M and *SD* are used to represent mean and standard deviation, respectively. Values in square brackets indicate the 95% confidence interval for each correlation. The confidence interval is a plausible range of population correlations that could have caused the sample correlation (Cumming, 2014). * indicates $p < .05$. ** indicates $p < .01$.

behavior was predicted by perceptions of their ingroup's (or the outgroup's) recycling behavior, we regressed individuals' self-reported recycling on (a) participants' own political orientation, (b) perceptions of conservative students' recycling behavior, and (c) perceptions of liberal students' recycling behavior, and the interaction between participants' own political orientation and perceptions of both of these groups. Results revealed significant interactions between individuals' own political orientation and both perceptions of conservative students' recycling behavior, $b = 0.14$, $SE = 0.07$, 95% CI [0.01, 0.27], $t(78) = 2.08$, $p = .04$, $\eta_p^2 = 0.05$, and perceptions of liberal students' recycling behavior, $b = -0.17$, $SE = 0.08$, 95% CI [−0.33, −0.02], $t(78) = -2.30$, $p = .02$, $\eta_p^2 = .06$[5] on individuals' (self-reported) recycling behavior.

Simple slopes analyses (Aiken, West, & Reno, 1991) revealed that for individuals who were themselves politically conservative (1 *SD* more conservative than the sample mean), self-reported recycling behavior was predicted by perceptions of the extent to which conservative students recycled, $b = 0.40$, $SE = 0.18$, 95% CI [0.04, 0.76], $t(78) = 2.20$, $p = .03$, but not the perceived extent to which liberal students recycled, $b = 0.09$, $SE = 0.20$, 95% CI [−0.30, 0.49], $t(78) = 0.48$, $p = .83$. In contrast, among individuals who were themselves politically liberal (1 *SD* more liberal than the sample mean), self-reported recycling behavior was predicted by perceptions of the extent to which liberal students recycled, $b = 0.68$, $SE = 0.17$, 95% CI [0.34, 1.02], $t(78) = 3.97$, $p < .001$, but not the extent to which conservative students recycled, $b = -0.06$, $SE = 0.13$, 95% CI [−0.32, 0.20], $t(78) = -0.46$, $p = .65$. Likewise, for those who were themselves politically moderate (relative to the target population; i.e., at the sample mean for political orientation), self-reported engagement in recycling was uniquely predicted by perceptions of liberal students' recycling behavior, $b = 0.38$, $SE = 0.13$, 95% CI [0.11, 0.65], $t(78) = 2.81$, $p = .006$, but not perceptions of conservative students' recycling behavior, $b = 0.17$, $SE = 0.11$, 95% CI [−0.05, 0.40], $t(78) = 1.55$, $p = .13$.

5.3. Discussion

Results of Study 1 suggest that individuals' own recycling behavior (as measured via self-report) can be predicted by their perceptions of how much their political ingroup recycles. Results showed evidence for *ingroup conformity* effects: the more political liberals believed other political liberals recycled, the more they self-reported recycling. Conversely, political conservatives' self-reports of recycling were predicted by perceptions that other political conservatives recycled more. We did not find evidence for *outgroup distancing*: individuals' self-reported recycling was not negatively affected by perceptions of what those on the opposite end of the political spectrum were doing. Interestingly, politically moderate students' behavior was also correlated to their perceptions of politically liberal students' behavior (but not politically conservative students' behavior), suggesting the possibility that political conservatives on college campuses might view themselves as part of a unique reference group conforming to separate ingroup norms from both liberals and moderates.

Although this study provides initial evidence that individuals might conform to perceived political ingroup norms, there are several limitations. For example, some or all of the identified statistical relationships could alternatively be explained by *false consensus effects* (Ross, Greene, & House, 1977), whereby individuals might have no idea how much other members of their political ingroup recycle and base these estimates on their own recycling behavior, thus creating a correlation between one's own recycling and perceptions of ingroup members' recycling. Further, because Study 1 took place in a university setting, it is possible that political identities could be confounded with other social identities, such as identification as a university student. It is also possible that participants might have guessed the hypotheses and effects could reflect demand characteristics. Finally, we are limited by the use of self-reported behavior; it is possible that individuals' self-reports might not have reflected their actual behavior. Thus, in later studies, we use experimental designs to directly assess causality and rule out alternative possibilities and directly measured participants' behavior.

6. Study 2

In Study 2 we experimentally manipulated individuals' perception of whether their behavior conformed to political ingroup norms to directly assess whether the drive for conformity influences recycling behaviors. Halfway through a computerized recycling sorting task, participants were randomly assigned to receive false feedback that they were either (a) failing to conform to ingroup norms or (b) conforming to ingroup norms.

To assess automatic and deliberate processes, we created a novel computerized task in which participants were asked to sort various items into either the recycling or trash. We then utilized mouse-tracking software, which records both decisions and participants' intermediate mouse movements in computerized tasks (for a review, see Hehman, Stolier, & Freeman, 2015; Stillman, Shen, & Ferguson, 2018), to examine participants' decision-making processes over time. From a *dynamic accounts* perspective of cognitive processing, mouse-tracking provides a means to test competing parallel processes that converge into a stable and integrated response over time (Spivey & Dale, 2004). For instance, in a task in which study participants are asked to categorize various animals as either a 'fish' or a 'mammal,' when presented a whale stimulus (a mammal that is superficially similar to a fish in many ways),

[5] Interactions between curvilinear effects of political orientation and perceptions of others' recycling were also tested and were not statistically significant, *ps* < .54.

many participants initially drag the mouse towards the 'fish' response category (i.e., a faster-acting automatic response) before ultimately making the correct choice of 'mammal' (i.e., a slower-acting deliberate correction). This example illustrates how mouse-tracking provides a means to detect the subtle magnetism of competing processes that can guide respondents' initial and final judgements. The degree to which competing processes deviate respondents' mouse trajectories away from a known correct response informs the relative strength to which respondents hold countering beliefs, attitudes, cognitive associations, or motivational states that detract from making direct, accurate responses (Freeman & Ambady, 2010). Mouse-tracking software has been previously used to identify implicit racial biases (Freeman & Ambady, 2009; Freeman, Pauker, Apfelbaum, & Ambady, 2010), understand the process of reactions on an implicit associations task (Yu, Wang, Wang, & Bastin, 2012), assess processes of self-control (Ha et al., 2016), and compare evidence for top-down versus bottom-up processes in person perception (Johnson, Freeman, & Pauker, 2012). Thus, these data allow us to unpack processes unfolding on different timescales, and provide a lower-cost alternative to using neuroimaging techniques to assess environmental decision-making (e.g., Geiger, Bowman, Clouthier, Nelson, & Adams, 2017; Sawe & Knutson, 2015).

As noted above, we focused on political conservatives in this task for two primary reasons. First, the means indicated in the Study 1 results suggested that recycling was a common behavior and widely considered a good thing to do; thus, to avoid ceiling effects, it made more sense to study a group in which ingroup normative information would decrease (e.g., political conservatives) rather than increase (e.g., political liberals) recycling. This is consistent with another reason behind this decision: as noted in the introduction, it may be more common for political conservatives, relative to political moderates and liberals, to perceive a disconnect between the perceived norms of their ingroup and their personal attitudes for pro-environmental behaviors.

6.1. Method

6.1.1. Participants

We determined sample size based on a power analysis using G*Power 3.1 (Faul et al., 2007) for a statistical interaction in a 2 × 2 mixed-effects ANOVA (based on default assumptions for test-retest correlations), and assuming effect sizes would be similar to the two key results in Study 1: η_p^2s of 0.06 and 0.05. Depending on which effect size was used, power analyses suggested that we needed either 44 or 52 participants to yield a statistical power of .90.[6] Thus, in order to achieve adequate statistical power we aimed to recruit 50–60 participants before data collection was complete at the end of the spring semester.[7]

Fifty-five university students (from the same psychology department subject pool as Study 1) who had previously identified as politically conservative in a prescreening survey participated in Study 2. All participants gave informed consent to participate in the research. Participants were 14 men and 41 women, mean age = 18.80 (range = 18–22). The majority of participants identified as White/Caucasian (95%), with some identifying as East Asian (6%) and/or Hispanic (4%).[8] No other ethnicities were represented in the present sample. We excluded the data from one subject who had 40% missing responses on the mousetracking task, (no other participants had greater than 5% missing responses), leaving 54 participants considered in final analyses.

6.1.2. Procedure

Participants completed two rounds of a recycling sorting task in which they sorted stimuli into either the recycling or trash by dragging the object from the bottom of the screen to either the top left (for recycling) or top right (for trash). After completing a brief practice round of sorting example items, participants sorted 20 unique recycling items and 20 unique trash items in each of two rounds. Participants were instructed to begin moving the mouse quickly, and if they did not begin moving within 1000 ms after the beginning of a trial, they received a warning message asking them to begin moving the mouse more quickly on subsequent trials. During each trial, participants' mouse movements were tracked using MouseTracker Version 2.82 software (Freeman & Ambady, 2010).

6.1.3. Stimuli

Experimenters and research assistants worked together to select 80 photos (representing 40 recyclable and 40 nonrecyclable items) from a larger database of photos. Two undergraduate research assistants who had previously worked in the campus sustainability office and had expertise in the recyclability of various items verified the selection of recyclable and nonrecyclable stimuli in the local university context. Stimuli ranged in difficulty from objects such as a soda can (which 100% of participants sorted correctly into the recycling) and a disposable diaper (which 91% of participants correctly sorted into the trash) to objects such as a recyclable fork (which only 69% of participants correctly sorted into the recycling) and an empty prescription medication bottle (which only 42% of participants sorted correctly into the trash). Participants received all stimuli exactly once, in random order, throughout the sorting task.

6.1.4. Design

We used a 2 (conformity feedback: *failure to conform* vs. *assurance of conformity*) × 2 (sorting round: pre-vs. post-feedback) mixed-effects experimental design with multiple trials (20 recyclable and 20 nonrecyclable items per round) nested within each experimental condition. All participants completed one round of this task prior to receiving any feedback. In between the first round and the second round of the experiment, participants received false feedback ostensibly based on an analysis of their sorting behavior in the first round. In reality, participants were randomly assigned to be informed that either their recycling behavior was more similar to an average liberal than an average conservative (i.e., *non-conformity* condition; 28 participants), or their recycling behavior was similar to that of the average conservative (i.e., *conformity* condition; 27 participants). After receiving this false feedback, participants completed a second round of the sorting task (with novel stimuli).

A manipulation check showed that participants tended to accurately recall the manipulation, median recalled scores = 75 (liberal) vs. 25 (conservative), $p < .001$, Cohen's $d = 6.5$. Most participants were close to the scale midpoint on belief that conservatives and liberals would recycle differently, $M = 1.49$, $SD = 1.07$ (on a 0 "not at all true" to 4 "very true" scale).

6.1.5. Data collection and analyses

To measure the effect of cognitive biases that compete with the desire to be accurate (as we study in the present work), *maximum*

[6] We note that since we conducted our a priori power analysis, recent blog posts (e.g., Giner-Sorolla, 2018) have critiqued using this method to estimate sample size necessary to detect interactions. This work suggests that sample size should be increased by 2x (for crossover interactions) or 4x (for interactions in which a factor has an effect on the DV in one condition of the other factor, but no effect in the other condition of the other factor). Thus, based on these blog posts, our study may have lower power than our a priori power analyses suggested.

[7] Due to software limitations, we based this power analysis on the test performed in section 7.2.2, which used a continuous DV. Section 7.2.3 involved a dichotomous DV which means that power analysis results could be slightly different if this analysis had been considered.

[8] Percentages add up to greater than 100% because participants were able to select more than one ethnicity.

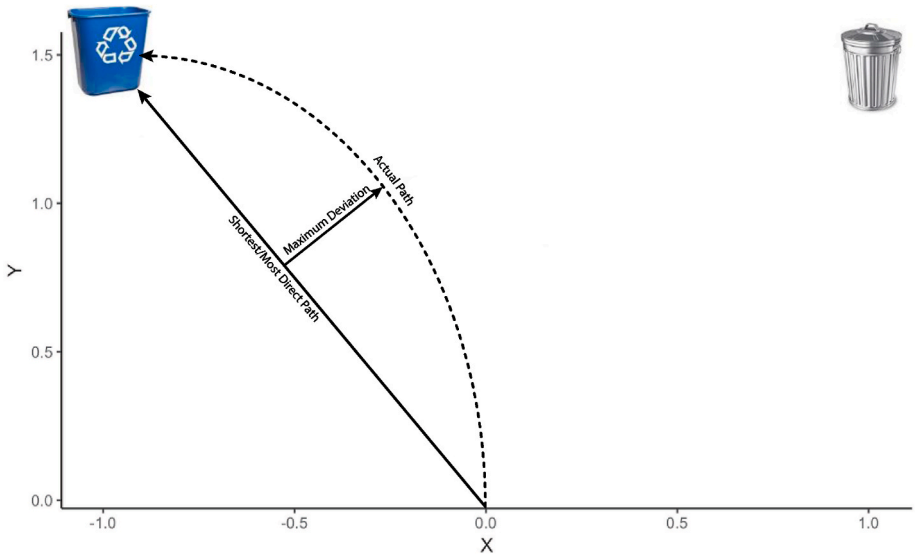

Fig. 1. Illustration of maximum deviation (MD) score. Maximum deviation is calculated within each trial by the maximum distance that the participants' mouse moves away from a straight line connecting the starting point and the ending point.

deviation from the line (i.e., the largest distance between the mouse cursor and the straight line at any point during a trial) is typically used in mouse-tracking analyses (Freeman & Ambady, 2010). Larger maximum deviations from the straight line suggest a larger maximum attraction to the alternative option, and thereby, a greater conflict between automatic and deliberate cognitive processes (see Fig. 1).

6.2. Results and discussion

6.2.1. Descriptive statistics

To assess descriptive statistics, we used the *lme4* package in R (Bates, Maechler, Bolker, & Walker, 2014; R Core Team, 2018), a package used for multilevel modeling, to address nesting and crossing of variables. Because stimuli were fully crossed within participants, we included random intercepts and slopes of observations, rounds, and types of stimuli (i.e., recycling vs. trash) by participants, and independent of this, random intercepts and slopes of observations by each item used (because certain specific items might have tended to elicit unique responses shared across participants). Degrees of freedom, *t*-values, and *p*-values were examined using the *lmerTest* package (Kuznetsova, Brockhoff, & Christensen, 2015). For all outcomes, chi-square tests of random effects were used to justify including each random intercept and slope term ($ps < .05$). More details of analyses and R code can be found at https://osf.io/nsybc/ .

We conducted preliminary analyses looking at overall sorting decisions. Participants sorted 64% of items into the recycling and 36% into the trash. Overall, participants sorted 23% of items into the incorrect receptacle. A multilevel logistic regression showed that participants were more accurate at sorting recycling items (91%) than trash items (63%), $b = 1.46$, $SE = 0.21$, 95% CI [1.07, 1.93], $z = 7.06$, $p < .001$, odds ratio (OR) = 0.23.

Considering only trials in which participants accurately sorted items,[9] we examined whether maximum deviation scores were larger in trials where participants sorted items into the recycling vs. the trash. Results showed that maximum deviation scores were smaller for items correctly sorted into the recycling (vs. the trash), $b = -0.22$, $SE = 0.01$, 95% CI [−0.25, −0.19], $t(67.44) = -14.77$, $p < .001$, with this effect explaining 81.4% of the variance in item-level maximum deviation scores. Together, these two sets of results suggest that across conditions and rounds, participants were more conflicted about throwing items away than recycling them.

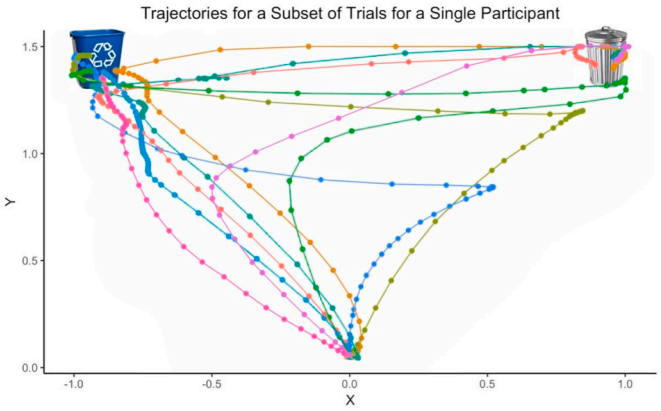

Fig. 2. Trajectory of a subset of trials for a single participant. This figure depicts the variety of responses that a single, randomly selected participant exhibited as shown by mouse motions.

6.2.2. Automatic effects of conformity

To assess whether the conformity manipulation exerted *automatic effects* on sorting behavior, we used data from 100 points of the trajectory of each of the trials that each of the participants completed (see Fig. 2 for a depiction of sample trajectories among one study participant). We compared overall changes from Round 1 to Round 2 in the failure to conform condition to those in the assurance condition while accounting for differences across trials. Automatic effects in the direction of predictions would be illustrated by participants who were led to believe that they were failing to conform to ingroup norms (relative to those who were led to believe they were conforming to ingroup norms) showing greater conflict between their desire to recycle items and their desire to conform when sorting items in Round 2 (i.e., a sorting round × conformity condition interaction). To assess this tendency using the combination of recycling and trash trials, and to ease interpretation, we

[9] Because there were no significant differences across condition or round for percentage of items sorted correctly or tendencies to incorrectly sort items into the recycling vs. trash (see 7.2.3, below), for simplicity and to avoid outlier trials we only examine trials in which participants correctly sorted items into the appropriate receptacle in analyses below.

standardized the maximum deviation scores within each stimuli type (i.e., recycling and trash) and then reverse-scored this standardized measure in the recycling trials, such that more strongly negative scores indicated a larger deviation away from the recycling in these latter trials. We then averaged scores by participant by round so that each participant had a Round 1 and Round 2 average maximum deviation score.

A 2 × 2 mixed-effect ANOVA using the *afex* package (Singmann, Bolker, Westfall, Højsgaard, & Fox, 2015) in R showed the hypothesized round × conformity condition interaction on maximum deviation scores, $F(1, 52) = 5.54$, $p = .02$, generalized $\eta_p = .01$. Simple effects tests, using the *lme4* package, revealed that this significant interaction was driven by changes by condition in the expected directions: those in the conformity condition trended toward an increased tendency to recycle items in Round 2 vs. Round 1, $b = -0.07$, $SE = 0.05$, 95% CI [−0.16, 0.01], $t(53) = -1.59$, $p = .12$, and those in the nonconformity condition trended less toward the recycling in Round 2 vs. Round 1, $b = 0.08$, $SE = 0.04$, 95% CI [−0.01, 0.17], $t(52) = 1.75$, $p = .09$. Taken together, the significant condition × round interaction suggests that, as predicted, participants who were led to believe they were not conforming, relative to those led to believe they were conforming, showed a decreased tendency to recycle.

6.2.3. Deliberate effects of conformity

To assess whether the conformity manipulation exerted *deliberate effects* on sorting behavior, we examined the percentage of items sorted into the recycling across condition and round. Explicit effects consistent with our hypothesis would be evidenced by those informed they were failing to conform (vs. those informed they were conforming) sorting fewer items into the recycling (and more into the trash) than before receiving this feedback. Thus, we tested a round × condition interaction on the proportion of items sorted into the recycling. Results did not support our prediction: there was no effect of conformity condition × sorting round proportion of items sorted into the recycling, $F(1, 52) = 0.29$, $p = .59$. These results suggest that participants corrected for the automatic effects described in the above analyses prior to decision-making (i.e., no *deliberate effects* of conformity condition on sorting behavior).

7. Study 3

Study 3 was a pre-registered replication of Study 2 (go to https://osf.io/nsybc/ for details) conducted in an online setting with a non-student sample. We conducted Study 3 in the first week of April 2020, which in the US fell within the first month of widespread social distancing and economy closure due to the COVID-19 pandemic.

7.1. Method

7.1.1. Participants

A priori power analyses were conducted using Monte Carlo simulations in the R package *simr* (Green & MacLeod, 2016). Using Study 2 results to inform expected effect sizes, simulated power analyses suggested that a sample of 150 participants would yield 0.90 power to observe both a round × condition interaction and a simple effect of round in the non-conformity condition. Due to uncertainty about how our procedure would translate to an online setting and an MTurk worker sample, we opted to oversample, with a target sample of 400 participants.

Participants, who were American and who identified as politically "conservative" or "very conservative," were recruited via TurkPrime (Litman, Robinson, & Abberbock, 2017). To avoid highly active MTurk workers who might be attuned to deception, we screened out the most active 10% of MTurk workers. In addition, those who had an approval rating of less than 90% were not eligible to complete our survey. In total, 450 participants completed the survey in exchange for $1.25. As described in our pre-registration, in an effort to remove data from participants who were likely to reside in other countries, we asked participants to label a picture of an eggplant in a post-task survey, a task which has demonstrated high discriminant ability (Moss & Litman, 2018). We considered "eggplant" and "egg plant" to be consistent with how an American would label this vegetable and all other responses, such as "brinjal" (a common term for eggplant used by English-speakers in India) and "aubergine" (an internationally used English term that is less common in the US) to warrant removal from the study. Thirty-five participants incorrectly labeled this picture. Additionally, we removed eight participants who reported using a touchscreen to complete this task (we had attempted to screen out all touchscreen users prior to beginning the task).[10] After these removals, we retained 407 participants in our final analysis.

7.1.2. Procedure, stimuli and design

Procedure, stimuli, and design were largely similar to Study 2 but adapted to an online setting and for a less homogenous sample. We used the same stimuli materials as in Study 2 but did not evaluate accuracy of sorting decisions because recyclable items would likely differ based by area. We configured online settings to screen out those using touchscreen devices and additionally requested that participants not use touchscreens. A post-survey question suggested that 71% of participants reported using a mouse and 29% reported using a trackpad. A manipulation check showed that participants in the two identity salience groups tended to accurately recall the manipulation, median recalled scores = 74 (liberal) vs. 28 (conservative), $p < .001$, Cohen's $d = 2.10$. Most participants were close to the scale midpoint on belief that conservatives and liberals would recycle differently, $M = 1.83$, $SD = 1.28$ (on a 0 "not at all true" to 4 "very true" scale). Similar to Study 2, overall, participants sorted 39% of items into the trash and 61% into the recycling.

Based on informal experimentation with the online protocol, we adapted settings to the medium: if participants did not begin moving the mouse within 1200 ms after the beginning of a trial, they received a warning message asking them to begin moving the mouse more quickly on subsequent trials. As before, participants also received a warning if they began moving the mouse before the object was displayed. Across all trials, 10.5% of trials had early start warning and 5.7% had a late start warning. We tracked participants' mouse motions using Mathur and Reichling's (2019) Qualtrics protocol.

7.2. Results

More details of analyses and R code can be found at https://osf.io/nsybc/.

7.2.1. Confirmatory analyses

Using a mixed-effects ANOVA (see Study 2), we first examined the effects of the manipulation by condition on automatic responses as operationalized by maximum deviation. We found a main effect of round on recoded maximum deviation scores, $F(1, 404) = 48.99$, $p < .001$, generalized $\eta_p = .04$. Yet, inconsistent with predictions and Study 2 results, this round effect was not moderated by condition, $F(1, 404) = 0.00$, $p = .94$. Rather, results suggested that people's automatic bias toward the recycling was lower in Round 2 than Round 1 across *both* conditions, $b = -18.54$, $SE = 2.65$, $t(405) = -7.01$, 95% CI [−23.74, −13.35], $p < .001$.

For deliberate responses, there was no overall effect of round on sorting choices, $F(1, 396) = 0.82$, $p = .36$, and this (null) effect was not moderated by condition, $F(1, 396) = 0.26$, $p = .61$.

[10] We did not initially plan to remove touchscreen users in our preregistration. When touchscreen users are included in analyses, all results are similar.

7.2.2. Exploratory analyses

We conducted follow-up analyses at the trial-by-trial level to verify that differences between Round 1 and Round 2 occurred immediately following the experimental manipulation and were not similarly due to gradual change over time, for example due to a familiarity effect or fatigue. We first centered maximum deviation data within whether the item was sorted (recycling vs. trash) and then reverse coded recycling items, such that a number above zero represented a maximum deviation more toward the recycling (less toward the trash) than average. Then, in a multi-level model, using the *nlme* package (Pinheiro, Bates, DebRoy, & Sarkar, 2014), we regressed this recoded maximum deviation score upon trial number (from trial numbers 1 through 80, recoded to be between −1 and + 1, with 0 representing the onset of the intervention), condition, and round (in Step 1), the interaction between condition and round (in Step 2) and all remaining two- and three-way interactions (in Step 3). Main effects from Step 1 suggested that after accounting for the post-manipulation decrease in recoded maximum deviation (which was significant in the expected direction), $b = -36.34$, $SE = 5.86$, $t(23{,}157) = -6.20$, $p < .001$, 95% CI [−47.83, −24.85], the effect of trial number on recoded maximum deviation was actually in the opposite direction, $b = 17.49$, $SE = 4.98$, $t(23{,}157) = 3.51$, $p < .001$, 95% CI [7.74, 27.25], suggesting that the increase in automatic tendency to favor the recycling that occurred post-manipulation was buffering against an overall decline in automatic tendency to favor the recycling. None of the two-way or three-way interactions were significant, $ps > .05$. Results of Step 3 are depicted in Fig. 3.

7.3. Discussion

Overall, results from Studies 2 and 3 were similar in many ways. First, across both studies, the non-conformity condition showed a shift in automatic processes to favor the recycling less following the feedback. Second, this shift did not appear to translate into an overall shift in sorting behavior; rather, the overall percentage of items sorted into the recycling and trash remained constant across both studies. The key difference between Study 2 and 3 results was that in Study 3, but not 2, the group told that they behaved in an identity-consistent manner also shifted their automatic processes. One possibility is that, among the MTurk worker sample of Study 3 (who are less naïve to experimental studies than college students), telling participants that they behaved in identity-consistent manner activated group-level stereotypes and thus still motivated individuals to conform to perceived group norms. To test this possibility, in Study 4 we include a control condition that received no manipulation in between rounds.

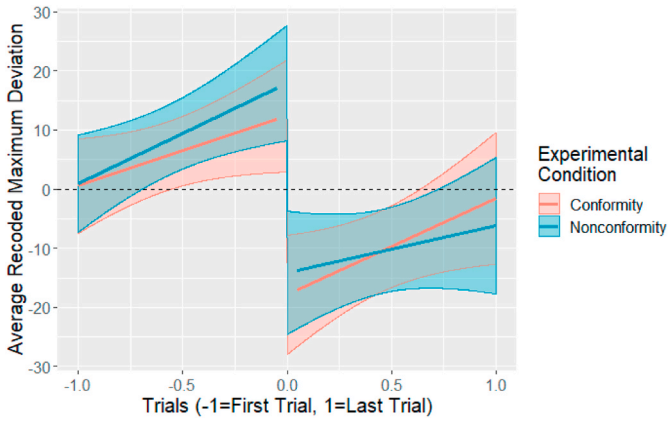

Fig. 3. Trial-by-trial regression discontinuity model of round effects in Study 3. Experimental condition effects and interaction are not statistically significant.

8. Study 4

Study 4 was a preregistered replication of Study 3 (go to https://osf.io/nsybc/ for details) with a third, between-participants control condition added. In this condition, participants did not receive any feedback. Study 4 was conducted in the first week of May 2020, which in the US represented the end of the second month of widespread social distancing and economy closure due to the COVID-19 pandemic.

8.1. Method

8.1.1. Participants

A priori power analyses were conducted using Monte Carlo simulations in the R package *simr* (Green & MacLeod, 2016). We used Study 3 results to inform expected effect sizes, estimating no change from Round 1 to Round 2 for the novel control condition and conservatively assuming that effects in the other two conditions might be slightly smaller than found in Study 3. These simulations suggested that a sample of 700 participants would provide at least 90% power to observe interactions between round and condition (conformity or non-conformity vs control) and a simple effect of round in both the conformity and non-conformity conditions. We attempted to recruit participants using the exact same method as Study 3, but excluding those who had participated in Study 3 (because they would not be naïve to study hypotheses) and increasing the payment to reflect a greater-than-expected average time to complete in Study 3. However, the pool of volunteers who both met the required criteria and had not participated in the previous study proved too small, so halfway through the study we allowed the most active 10% of MTurk workers to also participate. In total, 705 participants completed the study in exchange for $2.00. After removing 63 participants who incorrectly labeled the picture of the eggplant (see Study 3) and 26 who reported using a touchscreen or "other" to complete tasks, our final data consisted of 619 participants.

8.1.2. Procedure, stimuli and design

The Study 4 design was similar to Studies 2 and 3, but with the addition of a control group as a third between-participants condition that received no feedback in between rounds. Thus, Study 4 used a 3 (feedback: nonconformity vs. assurance vs. control) x 2 (round: pre-feedback vs. post-feedback) design.

Procedure and stimuli were identical to Study 3. Seventy-four percent of participants reported using a mouse and 26% reported using a trackpad. Similar to Study 3, participants sorted 62% of items into the recycling and 38% into the trash. Also, similar to Study 3, participants in the two identity salience groups tended to accurately recall the manipulation, median recalled scores = 74 (liberal vs. 26.5 (conservative), $p < .001$, Cohen's $d = 2.10$. However, in contrast to Study 3, Study 4 participants reported lower belief that liberals and conservatives would recycle differently, $M = 1.34$, $SD = 1.26$ (on a 0 "not at all true" to 4 "very true" scale). Across all trials, 15.0% of trials had early start warnings and 4.5% of trials had late start warnings.

8.2. Results and discussion

We used mixed-effects ANOVAs as in Studies 2 and 3 to examine the effect of the manipulations on automatic and deliberate responses. Contrary to predictions, we did not find the expected round × condition interaction on maximum deviation scores, $F(2, 595) = 0.72$, $p = .49$, nor was there a main effect of round, $F(1, 595) = 0.00$, $p = .98$. Similarly, and consistent with previous studies, there was no round × condition interaction on overall sorting decisions, $F(2, 595) = 0.95$, $p = .39$. More details of analyses and R code can be found at https://osf.io/nsybc/.

Unlike in Studies 2 and 3, in Study 4 our manipulation did not appear to influence automatic behavior on the mousetracker task. One potential explanation for the lack of replication is that participants in Study 4 (vs.

Study 3), on average, reported lower belief that recycling behavior would differ based on political ideology. Although these explicitly stated beliefs did not moderate automatic effects (*ps* > .05), implicit and explicit processes operate independently (Frith & Frith, 2008); thus, implicit beliefs about differences between conservative and liberal recycling behavior (which we did not directly measure) might have also differed and could have reduced the observed effects. In turn, this may have rendered our manipulation moot in Study 4, as it may have failed to evoke automatic stereotype activation needed to drive conformity effects.

It is possible that null effects in Study 4 are an artifact of broader shifts in society taking place as a result of the COVID-19 pandemic. For example, changes in lifestyles and media coverage occurring as a result of the pandemic could have dampened automatic associations between political ideology and recycling behavior, as evidenced by the decreased explicit association in Study 4 (vs. Study 3). Further, it is possible that stay-at-home orders across the US, and accompanying unemployment, may have increased the amount of surveys MTurk workers are taking each day. In addition, as we noted above, in Study 4 (but not Study 3) we were forced to include participants who were highly active on MTurk. To the degree that participants are completing more social psychological studies, their naiveté may be compromised, and manipulations that involve mild deception, like ours, may lose their effectiveness among such participants (Chandler, Mueller, & Paolacci, 2014). Unfortunately, we did not include a measure of the extent to which participants believed our manipulation, limiting our ability to investigate this post-hoc speculation. Regardless of these speculations, results from Study 4 dampen overall confidence in effects and raise questions about whether, and how, social identity processes affect decision-making processes in this recycling task.

9. General discussion

Across multiple studies, we examine the influence of political identity in pro-environmental decision-making. Although the particular populations of interest in the present work may have shown an overall bias toward recycling (even conservative participants; as illustrated by Study 2), this research also suggests that in some cases this tendency may be influenced by participants' desire to conform to the norms of their political ingroup. This work therefore suggests that for political conservatives, perceived norms to not act in a pro-environmental manner, potentially induced via priming political identity, can in some cases inhibit engagement in pro-environmental behavior. This conclusion is consistent with Wolsko's (2017) suggestion that interventions to foster pro-environmental behavior might be more effective at engaging individuals when they explicitly consider barriers posed by individuals' social identities, or conversely, facilitators posed by these identities. Yet, it is important to note that inconsistencies across studies (particularly null effects observed in Study 4) highlight the need for more research to fully understand these processes.

Results of Studies 2 and 3 suggest that increasing the salience of group norms, and perhaps in particular, the belief that one is not confirming with stereotypic ingroup behavior, exerts automatic but not deliberate effects on recycling behavior in this experimental paradigm. Data collected with mouse-tracking software showed that the experimental manipulation caused changes in the early stages of participants' decision-making processes but participants corrected for this influence by the end of the trials. Taken together, these findings suggest that providing feedback that participants' behavior differed from the ingroup norm affected faster-acting, presumably automatic cognitive processes but not slower-acting, presumably deliberate cognitive processes which influenced decision-making only after the individual has taken time to consciously evaluate the situation. However, failure to replicate automatic effects in Study 4 highlights the need for caution in interpreting results, as well as the need for additional studies to elucidate when and how these processes might apply.

Studies 2–4 also suggest limitations of the potential effects of conformity pressures on pro-environmental behaviors. Across all three mousetracking studies, fostering the belief that one was failing to conform did not foster deliberate behavior change (even when it fostered automatic behavior change, as it did in Studies 2 and 3). One possibility for the lack of effects of manipulations on deliberate responses is that individuals may have held an accuracy goal that overpowered the effects of the manipulation on deliberative processing (Van Bavel & Pereira, 2018). Within the specific, tightly controlled context of the mousetracking studies, in which participants were closely attending to decision-making, this resulted in deliberate processes correcting for the automatic influence on conformity pressures. These results suggest that when individuals are able and willing to attend to the task and when they are motivated to be accurate over conforming to social norms (e.g. when not being observed by other ingroup members), conformity pressures may exert limited influence over behavior. In contrast, when individuals are unable or unwilling to attend to the task at hand, any automatic conformity effects that exist may be likely to play a larger role in influencing behavior (Petty & Cacioppo, 1986). Additionally, when conformity-promoting concerns, such as social desirability, conflict with and outweigh the desire to be accurate or the desire to reduce waste, conformity-promoting deliberate processes, such as motivated reasoning, might also exert a substantive influence on pro-environmental behavior (Kunda, 1990).

Previous work examining relations between political orientation and pro-environmental attitudes and behavior has often concluded that conservatives' reduced interest in protecting the environment reflect motivated reasoning (Campbell & Kay, 2014; Kahan, 2012b), which would reflect deliberate processes (Van Bavel & Pereira, 2018). Yet, the present work raises the possibility that in some cases, this disconnect may be instead motivated by automatic processes. That is, conformity motives may influence pro-environmental decision-making by altering automatic associations in addition to and beyond the effects of deliberate processes (e.g., motivated reasoning).

An important caveat of these findings is that the processes identified in Studies 1–3 can potentially promote either socially beneficial or harmful outcomes. This is illustrated in Study 2, the study in which university student participants were given explicit instructions to recycle items consistent with the recycling guidelines at their university. In this study participants were more likely to sort nonrecyclables into the recycling than to sort recyclables into the trash. Mixing nonrecyclable items in with recycling can create problems for the recycling facility, and in many ways is more deleterious than putting recyclable items in the landfill (C. Smith, 2019). Thus, the results of this study suggest potential societal benefits of conformity to anti-environmental social norms under some circumstances—here, informing conservative individuals that their behavior was too pro-environmental to meet ingroup norms ironically led to automatic processes in the direction of more accurate item sorting. Yet, in other contexts, such as addressing the threat posed by climate change (IPCC, 2014), conformity pressures that discourage pro-environmental behavior are likely to exert societal effects which are more unambiguously harmful to society.

9.1. Limitations and future directions

Because we focus on a specific cultural context, domain, and behavior, our work is limited in our ability to generalize. First, our work was conducted in the US, a country in which environmentalism is viewed as politically polarized (Ehret et al., 2018). Because this perceived polarization fosters the association between environmentalism and liberalism, our theoretical perspective may be more applicable to regions where this polarization is fairly strong (e.g., Western Europe) rather than where it is weak (e.g., Eastern Europe; Poortinga, Whitmarsh, Steg, Böhm, & Fisher, 2019). Further, we focused on a stereotypically liberal behavior (recycling) within a stereotypically liberal domain (the environment); the effects of political conformity pressures

could potentially differ with other behaviors or in other domains. Researchers investigating these sorts of questions might also consider whether measures of political identity related to political party affiliation (e.g., Democrat, Republican) or political ideology (e.g., liberal, conservative) are most appropriate here; although we used political ideological categories to assess identity, recent work suggests that party affiliation may have recently become a stronger predictor of pro-environmental concern than self-reported political ideology (Cruz, 2017; also see; Hornsey, Harris, Bain, & Fielding, 2016).

Future work should also examine *for whom* and *in what situations* the automatic processes revealed in Studies 2 and 3 will most strongly influence environmental decisions. Although ambiguity has been cited as a factor that can increase the pull of automatic processes (e.g., Petty & Cacioppo, 1986), given the ambiguity of many items in the present task, we do not believe that creating a more ambiguous task would necessarily influence patterns of results. Instead, we suggest that increasing the speed of the task or decreasing the amount of cognitive resources available (for example, by putting participants under cognitive load) may potentially lead to a greater impact of the experimental manipulation on final decisions of where to sort items. Increasing the cognitive load might also add to the experimental realism of this future work as many people are focused on other things when making recycling decisions in the real world. In addition, making the feedback and resulting decision publicly visible to ingroup members might enhance the effect size and downstream effects on later behavior as well (Brick, Sherman, & Kim, 2017; Geiger, Swim, & Glenna, 2019). Future work might also consider collecting between-participant individual difference measures of existing implicit bias and implicit identity (e.g., Greenwald, McGhee, & Schwartz, 1998) and to see how these constructs interact with the experimental manipulation employed.

Future research should also build upon related work illustrating how experimental manipulations can *increase* pro-environmental intent among political conservatives. For example, conservatives' environmental concern increases when the topic is framed using prototypically conservative values (vs. politically shared values; Feinberg & Willer, 2013). This might reflect either greater personal resonance with the message or a desire to conform to ingroup norms, a distinction that can be tested in future work. Likewise, conservatives report greater acceptance of climate science when free-market solutions to climate change are presented (Campbell & Kay, 2014). It is unclear whether this actually reflects greater support for free-market solutions per se or whether it might instead reflect greater belief that the message would be accepted by other ingroup members. For example, Wolsko, Ariceaga, and Seiden (2016) find that effects of "conservative" moral frames are mediated by perceptions that the moral frame came from the ingroup. However, it is important to note that addressing polarizing social norms about the environment is not the only viable solution to reduce polarization: fostering a conflict between polarizing social norms and the competing desire to remain cognitively consistent can also promote depolarization on environmental issues (Gehlbach et al., 2019).

Finally, future work might consider taking a more macro-level approach, seeking to understand not only the effects of implicit biases but also understanding how these implicit biases could arise in the first place. For example, future work could assess whether these biases might arise due to media effects (Slater, 2007), personal observation, and/or false consensus arising from one's own personal behaviors and attitudes. With regard to the possibility of false consensus, we note that it is possible that both false consensus and normative influence could be simultaneously occurring, causing a self-reinforcing feedback loop and explaining the patterns identified in Study 1.

10. Conclusion

The present work examines the process by which political identity affects judgement and decision-making within the context of pro-environmental behavior. This research suggests that individuals' pro-environmental behavior may be guided at least in part by the desire to conform to the perceived norms of one's political ingroup, and in countries like the US where environmentalism is associated with political liberalism, these conformity pressures can motivate political conservatives to avoid pro-environmental behavior. Further, some of these effects can occur via automatic, rather than deliberate, processes. This latter finding has important implications for both theory and practical advocacy, as they suggest that political orientation might be influencing pro-environmental decision-making processes through automatic associations more so than motivated reasoning. However, inconsistencies across studies suggest that more research is needed to elucidate when and how these processes apply. Despite these inconsistencies, our work lays a foundation for future research by demonstrating how this novel mousetracking task can be employed to assess the role of automatic processes in environmental decision-making.

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Author contributions

Nathaniel Geiger – Conceptualization, investigation, formal analysis, methodology, original draft preparation, supervision, funding.

Michael Pasek – Conceptualization, investigation, writing, editing.

Michael Grusczcynski – Investigation, formal analysis, project administration, funding, editing, methodology, data curation.

Nathaniel Ratcliff – Conceptualization, investigation, editing.

Kevin Weaver – Conceptualization, investigation, editing.

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