



Contagion in social attitudes about prejudice

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ABSTRACT

Members of the same household share similar social attitudes, but the source of the similarity in attitudes may be attributed to many processes. This study uses data from a randomized field experiment to identify contagion in attitude change about anti-transgender prejudice. During a face-to-face canvassing experiment, registered voters who answered the door were exposed to either a message encouraging active perspective taking intended to reduce transphobia or a recycling message. Here, I show that the messages delivered to one household member are likely to reduce anti-transgender prejudice in the cohabitants as well. This finding suggests that door-to-door canvassing messages intended to elicit attitude change are likely to be socially transmitted.

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Introduction

Observational studies have shown that political behaviors and social attitudes are often correlated in social networks (Bond & Messing, 2015; McPherson, Smith-Lovin, & Cook, 2001). However, the source of this correlation is uncertain, as it may arise from social influence, social selection, or from shared context (Christakis & Fowler, 2013). Field experiments have been widely used to understand how campaigns may influence the behaviors of not only those they directly contact, but also how the effects of such campaigns may spread in social networks (Bond et al., 2012; Foos & de Rooij, 2017; Jones, Bond, Bakshy, Eckles, & Fowler, 2017; Nickerson, 2008). Recent work has focused on how such campaigns may also affect the attitudes of those who are contacted (Broockman & Kalla, 2016). A report on such a field experiment was recently published by Broockman and Kalla (2016), henceforth BK, which demonstrated that an intervention delivered by canvassers that encouraged voters to take the perspective of transgender people decreased prejudice towards an out-group (transgender people) using a single conversation. An open question from this research is whether the effects on social attitudes of the intervention travel through social networks, suggesting social influence accounts for the correlation in attitudes.

Lab experiments often fail to document attitude change in closely held attitudes (Paluck, 2009), and when they do, the change is frequently short-term (Paluck et al., 2015). However,

a recent study demonstrated that participants with anti-gay-rights attitudes who were exposed to discussion where discussants opposed their views were likely to conform to the group opinion (Walker, Sinclair, & MacArthur, 2014), suggesting attitudes could change through social contact. Recent field experiments demonstrate methods by which door-to-door canvassers may similarly elicit attitude change (Broockman & Kalla, 2016; Enos, 2014). Importantly, this evidence suggests that effective efforts use social appeals, particularly by asking the person being contacted to take the perspective of another.

Perhaps the most likely set of relationships in which such spillovers could take place are members of the same household. Cohabitants have long been found to share similar political attitudes (Niemi, Hedges, & Jennings, 1977). Observational research shows that spouses may even influence one another's party identification (Kenny, 1994), which some scholars argue is a relatively fixed attribute (Green, Palmquist, & Schickler, 2002). This suggests that if one cohabitant changes an attitude other cohabitants may be influenced.

A natural mechanism that would explain the similarity in political attitudes among cohabitants is social influence through political discussion. Spouses are the most commonly named political discussion partner (Beck, 1991). This discussion may frequently be an attempt to persuade, as family members (Nickerson, 2008), and spouses more specifically (Stoker & Jennings, 1995), are cited as the most frequent targets of attempts at persuading another person to change a political attitude or behavior. Huckfeldt and Sprague (1995) argue that these attempts at persuasion are often successful, and they estimate that spouses are approximately three times as influential than are other types of relationships.

One explanation for the success in political persuasion of cohabitants is the high level of intimacy that conversations between spouses have. Morton (1978) finds that conversations between married couples showed significantly higher intimacy of the information exchanged, while conversations among strangers tended to trivialize intimate topics. Further, personal influence related to politics is thought to be influenced by the intimacy of the social relationship (Dawson, Prewitt, & Dawson, 1977). A model of influence based on social cohesion among discussants suggests that influence is most likely when discussants are socially close and trust one another (Katz, 1957). Subsequent research on voter turnout has shown that the closest ties are the most influential (Bond et al., 2012). It is likely, then, that spillovers from campaigns intended to affect attitudes would occur in close ties, such as cohabitants.

A number of studies have investigated the important role that cohabitants play in influencing the political behavior and attitudes of other cohabitants. These studies have included investigations of social influence on voter turnout using field experiments (Nickerson, 2008; Sinclair, McConnell, & Green, 2012), observational work on correlated turnout in two-voter households (Cutts & Fieldhouse, 2009; Fieldhouse & Cutts, 2012; Hobbs, Christakis, & Fowler, 2014), and observational research on attitudinal similarity between spouses (Zuckerman, Fitzgerald, & Dasović, 2005). While some of these studies have used experimental designs to study social influence in voter turnout, none have used experiments to investigate how social attitudes may also transfer from between cohabitants.

Tie strength and social influence

Importantly, the above theory about interpersonal influence suggests that there is likely heterogeneity in the likelihood of interpersonal influence between cohabitants. If influence

is a product of intimate conversation, relationships in which intimate conversations occur at a higher rate should be more likely to elicit persuasion. The data collected and analyzed here do not contain measures of intimacy, conversation, or even the nature of the relationship between cohabitants. Fortunately, research has shown that individual characteristics, and the relationships between them in dyads, are predictive of the strength of a social relationship (Jones et al., 2013).

Studies have shown that social influence varies across individual characteristics (Burger, 2010) and is more likely to take place between individuals in closer relationships (e.g. Bond et al., 2012; Jones et al., 2017). Importantly, this research has also shown that the relationship between tie strength and social influence is non-linear – the strongest relationships have a substantially higher likelihood of social influence. Because of this, we might expect that the effects of an intervention to change political attitudes would be transmitted only to cohabitants who share especially close relationships.

In cases in which the exact nature of the relationship between individuals is unknown, the relationship between the characteristics of a pair of individuals is predictive of tie strength. One predictor that has been shown to be negatively related to tie strength is difference in age (Jones et al., 2013). That is, individuals who are further apart in age are less likely to share a strong tie. Similarly, pairs of individuals who are closer in age are more likely to influence each other (Aral & Walker, 2012). By applying the same principles to cohabitants, I expect that social influence is more likely to take place among cohabitants who are more similar in age. In particular, cohabitants who are relatively close in age are more likely to be romantic partners or siblings, while those who are relatively far apart in age may have different relationships (e.g. parent-child).

Method

I analyze data from a previously published field experiment studying the effects of canvassing on prejudice toward transgender people (Broockman & Kalla, 2016). In this field experiment, canvassers attempted to reduce prejudice toward transgender people by contacting potential voters at their doors. Canvassers attempted to conduct a conversation with the voter and first explained that they may have to vote on an upcoming law intended to protect transgender people from discrimination. Informing voters that a vote may soon occur encourages active processing, as research has shown that active processing is more likely when people know they may have to make a decision (Petty, Cacioppo, & Schumann, 1983). Next, canvassers encouraged analogic perspective taking (Gehlbach & Brinkworth, 2012) by first describing a time when the canvasser had experienced judgment, then asking the voter to describe a time when they had experienced judgment themselves or had witnessed another person experiencing judgment. Finally, canvassers asked the voter to think about how transgender individuals feel when they are judged, which encouraged those contacted to think of the judgment transgender people may feel as analogous to their own feelings of having been judged. At the end of the conversation, if the voter expressed that they would now support legal protections for transgender individuals, the canvasser encouraged the voter to speak with social contacts about the conversation.

An ostensibly unrelated survey measured the effects of the canvassing. Registered voters were recruited to participate in the survey ($n = 68,378$) via mail. Respondents were enrolled in a panel ($n = 1825$) and assigned to either a treatment group ($n = 913$) or a control group

that would be targeted with a recycling message ($n = 912$). Multiple cohabitants ($n = 1295$) were enrolled, and all cohabitants were in the same treatment group. Canvassers then contacted potential voters ($n = 501$) at their doors. All of those who were in a household in which at least one individual answered the door and complied with the intervention (as described below) were then contacted to complete follow-up surveys that began three days, three weeks and six weeks after canvassing.

In households with two or three registered voters, all registered voters in the household were invited to participate in the surveys, with each individual receiving a separate code with which they could participate. In households with more than three registered voters, a random sample of three cohabitants were invited to participate. Among those invited, 1825 individuals enrolled via participation in the baseline survey, coming from 1295 households, for an average of 1.41 people per household completing the baseline survey. Cohabitants who were not directly contacted by the intervention, but who did respond to at least one follow-up survey ($n = 191$), are the focus of this study. I investigate the attitudes of cohabitants of those who were directly contacted in surveys conducted three-days ($n = 153$), three-weeks ($n = 152$), and six-weeks ($n = 149$) after the intervention.

Dependent variables

To measure the effects of the treatments, I use the same measures and indices as BK.

Support for transgender protection law one

Answer to the question,

Miami-Dade county recently passed a law that prohibits discrimination in housing, employment and public accommodations based on gender identity and expression, a category that includes transgender men and women [In the 6-week survey only, a clarifying phrase was added, that read, '(people who were designated one gender at birth, but now identify as a different gender.)']. Do you favor or oppose this new law?

Support for transgender protection law two

Answer to the question,

Some people say it's important to protect transgender people from discrimination in housing and employment. Other people have concerns about society becoming too accepting of transgender people, and do not want transgender people included in our non-discrimination law. What do you think? Do you agree or disagree that Miami law should protect transgender people [In the 6-week survey only, a clarifying phrase was added, that read, '(people who were designated one gender at birth, but now identify as a different gender.)'] from discrimination?

Feeling thermometer toward transgender people

Participant rating from 0 to 100. Ratings between 50° and 100° indicate feeling favorable or warm toward transgender people. Ratings between 0° and 50° indicate feeling unfavorable or cold toward transgender people.

Transgender psychologically abnormal

Participants' agreement with the statement, 'A man who identifies as a woman is psychologically abnormal.' Response options ranged from strongly agree (1) to strongly disagree (5).

Transgender morally wrong one

Participants' agreement with the statement, 'It is morally wrong for a man to present himself as a woman in public.' Response options ranged from strongly agree (1) to strongly disagree (5).

Transgender morally wrong two

Participants' agreement with the statement, 'Saying you are a gender that is different than the one you were born with is morally wrong.' Response options ranged from strongly agree (1) to strongly disagree (5).

Support friend's sex change

Participants' agreement with the statement, 'I would support a friend choosing to have a sex change.' Response options ranged from strongly disagree (1) to strongly agree (5).

Support transgender teacher

Participants' agreement with the statement, 'Transgender women (people who identify as women but were designated male at birth) should be allowed to serve as public school teachers.' Response options ranged from strongly disagree (1) to strongly agree (5).¹

Transgender bathroom use morally wrong

Participants' agreement with the statement, 'It would be wrong to allow a transgender woman (a person who identifies as a woman but was designated male at birth) to use the woman's restroom.' Response options ranged from strongly agree (1) to strongly disagree (5).

Gender norms about appearance

Participants' agreement with the statement, 'To keep children from being confused, it's better when men look and act like men, and women look and act like women.' Response options ranged from strongly agree (1) to strongly disagree (5).

Gender norms about rights

Participants' agreement with the statement, 'Men and women should have equal rights, but men and women are not the same; it's normal for men to act like men, and women to act like women.' Response options ranged from strongly agree (1) to strongly disagree (5).

Gender norms about dress

Participants' agreement with the statement, 'Men should dress like men and women should dress like women.' Response options ranged from strongly agree (1) to strongly disagree (5).

Outcome indices

Following the original analysis, I created four outcome indices. In all cases, the indices are constructed such that more positive scores indicate more tolerance and less prejudice toward transgender people. The measurement of attitudes toward transgender people is an area of research that has received little scholarly attention thus far. The measures above were adapted from measures of tolerance toward other minority groups. All of the scales below – except the law index, which is composed of only two items – are constructed using factor analysis.²

Omnibus index

A factor analysis was conducted on all of the main outcome variables: *Support for transgender protection law one*, *Support for transgender protection law two*, *Feeling thermometer toward transgender people*, *Transgender psychologically abnormal*, *Transgender morally wrong one*, and *Transgender morally wrong two*. The first rotated factor score was saved.

Transgender tolerance index

A factor analysis of the questions that concern tolerance and prejudice toward transgender people: *Feeling thermometer toward transgender people*, *Transgender psychologically abnormal*, *Transgender morally wrong one*, *Transgender morally wrong two*, *Support transgender teacher*, and *Transgender bathroom use morally wrong*. The first rotated factor score was saved.

Law index

The simple average of the two questions about support for the law: *Support for transgender protection law one* and *Support for transgender protection law two*.

Gender non-conformity index

A factor analysis of the questions that concern gender non-conformity (*Gender norms about appearance*, *Gender norms about rights*, and *Gender norms about dress*) was performed. The first rotated factor score was saved.

All indices were rescaled to have a mean of zero and standard deviation of one in the placebo group. By rescaling the index in this way, treatment effects can then be interpreted as changes in standard deviations from treatment.

Estimation of treatment effects

To evaluate the effect of treatment, I estimate complier average causal effects. 501 registered voters were successfully identified at the door. Of these, 246 registered voters were successfully identified. Individuals who refused to be identified are considered to not have been treated. Treatment was inadvertently delivered to 11 individuals who were assigned to the control group due to canvasser error. Note that these 11 individuals are included in the control group even though they were delivered the treatment. I consider a household treated if at least one member of the household began the conversation with the canvasser to the point of being identified. It is important to note that no member of the study is re-classified based on what happened at the door (treatment or placebo group status is not affected by mistakes by canvassers or by non-compliance with the treatment after being identified).

I use ordinary least squares (OLS) with cluster-robust standard errors, clustering on the household and residualizing using pre-treatment covariates from the baseline survey and voter list.³ I use the same set of pre-specified pre-treatment covariates that were used in the original analysis of the direct effects of the experiment from both the survey respondent whose behavior is being observed (i.e. the indirectly treated cohabitant) and the covariates from the individual who answered the door (i.e. the directly treated individual). These covariates are likely to be related to an individual's existing attitudes toward transgender people, and their inclusion should make the estimates of the treatment effect substantially more efficient. Covariates are included for two primary reasons. First, the inclusion of

the same covariates as in the original study makes the results between the studies more comparable. Second, the increase in efficiency of the estimates enables unbiased estimates of the treatment effect with significantly increased power (Gerber & Green, 2012). I note, however, that estimates which do not include covariates do not show statistical significance, as was the case in the analysis of the direct effects (Broockman & Kalla, 2016). The increase in efficiency by residualizing on pre-treatment covariates is necessary to distinguish effects from chance. Four cohabitants who were not directly contacted lived with two individuals who were contacted. For these individuals, I use the average values of the covariates among the contacted individuals (other specifications, including dropping these individuals, do not substantively change the results).

Identification of subsample

In order to identify a set of cohabitants in which social influence is most likely, I rely on the difference in age between the directly contacted individual and the cohabitant. More direct measures of the tie strength, such as frequency of interaction, trust between individuals, nature of the relationship, and so on, would be preferable. However, because the original researchers did not collect such data, I rely on a proxy measure that has been shown to be related to tie strength, the difference in age between connected individuals.

I split the sample between cohabitants who are at least 25 years in age different from the directly contacted individuals and those who are not.⁴ A difference of 25 years approximately corresponds with the length of a generation (Palchykov, Kaski, Kertész, Barabási, & Dunbar, 2012). By selecting cohabitants who are within 25 years in age different, there is a higher likelihood that the cohabitants share a close tie (Jones et al., 2013), which should increase the likelihood of the effect of the intervention being passed from the directly contacted individual to the cohabitant. By splitting the sample, I am able to maintain adequate statistical power while testing for a treatment effect in a subsample in which spillover of the treatment is more likely.

Of the 191 total cohabitants in the data, 125 (65%) had an age difference of 25 years or fewer from the directly contacted individual. I investigate the attitudes of this set of cohabitants of those who were directly contacted in surveys conducted three-days ($n = 105$), three-weeks ($n = 103$), and six-weeks ($n = 103$) after the intervention. While it is impossible to know with certainty, this set of relationships is more likely to be romantic partnerships, siblings, or close friends.

Results

I first tested for evidence of social influence in the full sample of cohabitants. The results were suggestive that effect of the treatment had been passed on to the cohabitants as the estimated effects indicated that the cohabitants of the treatment group had more positive views toward transgender people. However, in nearly all cases the estimates were too noisy to distinguish from chance (see supplementary information for the full set of estimates for all cohabitants). In the surveys conducted three weeks after the intervention, the cohabitants of the treatment group showed significantly higher measures on the transgender tolerance index than the cohabitants of the treatment group ($b = .27$, $t(102) = 2.14$, $p = .04$). A number

of other results are near statistical significance, but the evidence for social transmission among the full set of cohabitants is thin.

However, it is possible that the social transmission of attitudes among cohabitants occurs in closer relationships. I tested for this in the subsample of cohabitants who are within 25 years in age. After the intervention, initially (in the surveys conducted beginning three days after the intervention) the cohabitants of the treatment group showed suggestive evidence of more measures on the omnibus index when compared to the cohabitants of the control group ($b = .28$, $t(52) = 1.92$, $p = .06$). However, in the surveys conducted beginning three weeks after the intervention, the cohabitants of the treatment group had significantly higher scores on the omnibus index than were the cohabitants of the control group ($b = .45$, $t(50) = 3.26$, $p < .01$). This effect persisted ($b = .51$, $t(50) = 3.49$, $p < .01$) through the next wave of the follow-up surveys, which began six weeks after the intervention.

I next tested for social contagion of tolerance for transgender individuals using the transgender tolerance index. In the surveys conducted three days after the intervention, the cohabitants of the treatment group showed significantly more tolerance toward transgender people than the cohabitants of the control group ($b = .46$, $t(52) = 3.03$, $p < .01$). This effect persisted in both the survey conducted three weeks after the intervention ($b = .46$, $t(50) = 2.94$, $p < .01$) and the six-week survey ($b = .55$, $t(50) = 3.40$, $p < .01$). This is consistent with the strong direct effects of the intervention on tolerance toward transgender people.

Next, I investigated the effects of the intervention on cohabitants of the participants' scores on the law index from discrimination were socially transmitted. I found no evidence that such attitudes were socially transmitted in the 3-day, 3-week, or 6-week surveys (see supplementary materials for full statistical results). This is not particularly surprising given that in the original experiment, the researchers found that the intervention had little effect on support for the law.

I next tested for social transmission of the effects of the intervention on the gender non-conformity index. In the survey conducted three days after the intervention, cohabitants of the treatment group showed no difference when compared to cohabitants of the control group for attitudes toward gender non-conformity ($b = .35$, $t(52) = 1.47$, $p > .10$). However, in the surveys conducted three weeks after the intervention, cohabitants of the treatment group held significantly more positive attitudes about gender non-conformity than did cohabitants of the control group ($b = .49$, $t(50) = 2.34$, $p = .02$). This effect persisted in the survey conducted six weeks after the intervention ($b = .53$, $t(50) = 2.42$, $p = .02$).

Finally, I tested for social contagion of the feeling thermometer measure concerning feelings toward transgender individuals. The cohabitants of the treatment group report feeling thermometer scores significantly higher than the cohabitants of the control group ($b = 17.97$, $t(52) = 3.34$, $p < .01$) in the surveys conducted beginning three days after the intervention. This effect is marginally significant in the survey conducted beginning three weeks after the intervention ($b = 9.51$, $t(50) = 1.97$, $p = .05$), but is significant in the survey that began six weeks after the intervention ($b = 16.47$, $t(50) = 2.91$, $p = .01$).

Figure 1 graphically displays the treatment effects across the three survey waves in standard deviations. It is striking that while the estimates of the treatment effects vary to some degree, the confidence intervals across the estimates of the same effects over time (e.g. the effect for the omnibus index in the three-day, three-week, and six-week surveys) are all quite similar. This suggests that the intervention was broadly successful at changing the attitudes of not only those directly contacted, but also for cohabitants who are similar in

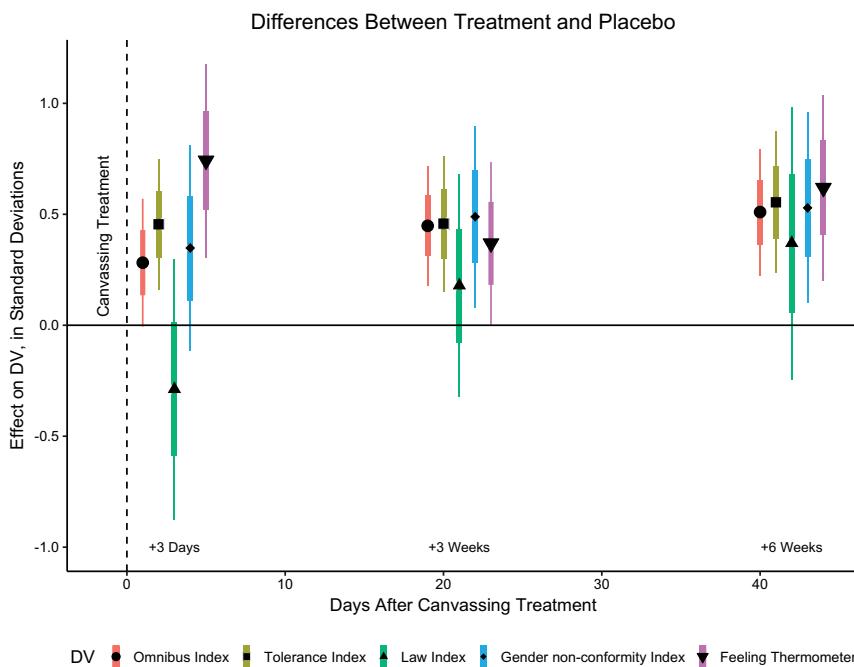


Figure 1. Complier average causal effects on the cohabitants (who were less than 25 years in age different from those directly contacted) of those exposed to the intervention.

Notes: Points indicate estimated treatment effect of the intervention in standard deviations. Thicker lines are standard errors of the estimate and thinner lines are 95% confidence intervals.

age. Further, similar to the direct effects of the intervention, the effects of the intervention on cohabitants who are similar in age appears to endure over time.

Discussion

Although social attitudes are typically thought to be stable and unlikely to be easily influenced (Alwin & Krosnick, 1991), this research shows that canvassing may affect the attitudes of not only the individual contacted, but also other cohabitants that person communicates with. This underscores the importance of social influence for eliciting attitude change around contentious issues. The results indicate around 90% of the effect of the conversation on survey respondents' feelings toward transgender people were socially transmitted to cohabitants within three weeks. However, it is important to note that the confidence intervals of these estimates are quite wide. It is possible that the true percentage of the effect that is passed on is smaller. Campaign efforts to elicit attitude change should understand that their messages are likely to be shared beyond the direct contact the campaign makes.

Previous work has shown that political behaviors and attitudes spread between people embedded in social networks (Bond et al., 2012; Bond & Messing, 2015; Nickerson, 2008). However, this work has not investigated the processes through which the spread of behaviors and attitudes takes place, particularly when the attitudes in questions concern attitudes toward a social group. The processes that account for the spread of the effect to cohabitants, likely conversations between the contacted individual and cohabitants, are

as-yet unobserved. However, the fact that treatment effects are found among those who are more likely to share a strong tie suggests that tie strength, perhaps through the mechanism of increased intimacy, helps to account for the spillover of treatment effects in this group.

This research has two particularly important limitations worthy of discussion. Both limitations owe to the original research design that was not intended to test spillover effects specifically. First, the research suffers from a statistical power problem due to the relatively small number of cohabitants who were included in the sample. The original researchers targeted households and attempted to maximize the number of directly treated individuals without regard to the number of cohabitants of those directly treated individuals. In the future, researchers who are interested in observing spillover effects may want to sample in order to increase the number of opportunities for observing spillovers, such as by selecting only households with multiple participants on the baseline surveys for treatment and control.

Second, it is possible that the cohabitants who responded to the survey were, in fact, not the cohabitants, but actually the individuals who were directly contacted at the door. In the particular case in which the person who was canvassed answers the surveys on behalf of cohabitants who were not canvassed, there is a possibility for a positive bias in the spillover effects. In the published, non-identified data there is no clear way to estimate the extent to which this may be a problem. However, inspection of the data does not show patterns that are demonstrably problematic. Cohabitants' answers to survey questions are significantly correlated, as would be expected. However, they are not so closely correlated that fraud is clear and evident. In the future, researchers who rely on survey responses to and are interested in spillover effects may devise research designs that better check for the identity of the person responding to the survey.

A further limitation of this work is that the process through which interpersonal influence takes place is not observed. The most likely scenario is that those who had conversations with canvassers and were convinced to change their attitudes then shared that conversation with those they live with, or at least in some way described their changed attitude. However, there may be other mechanisms that could account for the change as well. It is plausible that those who were contacted at the door in some way changed their behavior, such as through changes in media consumption and cohabitants were in turn also exposed to different media. Such a subtle shift seems unlikely to cause the degree of attitudinal change observed in cohabitants, and conversation seems much more likely to do so, but without observation of the mechanism the circumstances in which spillovers are likely to occur is unknown.

Conclusion

This research shows that campaigns which contact individuals at their doors in order to affect attitudinal change may impact not only those they contact, but others as well. This research also highlights the importance of understanding how social relationships impact attitudes through social influence. Interpersonal influence impacts the attitudes that people hold. This research also demonstrates a methodology through which the causal effect of interpersonal conversations on attitudes may be observed. Through careful research designs, future research may better ascertain how campaigns create the circumstances through which interpersonal influence propagates further behavior and attitude change.

Notes

1. Support transgender teacher, transgender bathroom use morally wrong, and gender norms about dress were asked on the 3-week and 6-week surveys only.
2. The use of factor analysis should decrease measurement error from these measures, which is particularly important given that they have not been validated. For the purposes of this study, the factor analysis creates index measures related to attitudes toward transgender people while reducing measurement error, but the factor variable may or may not represent a latent attitude toward transgender people.
3. The covariates from the voter file are age, an indicator for whether the participant is black, an indicator for whether the participant is Hispanic, an indicator for whether the participant is a registered Democrat, and gender. The covariates from the baseline survey are Support for transgender protection law one, Support for transgender protection law two, Feeling thermometer toward transgender people, Transgender morally wrong one, Transgender morally wrong two, Support friend's sex change, a feeling thermometer score for Barack Obama, a feeling thermometer score for gay people, the participant's response to a 7-point political ideology question, the participant's response to a 7-point political party identification question, whether the participant had met a gay or lesbian person, whether the participant had met a transgender person, frequency of attending religious services, support for same sex marriage, social dominance orientation scale score, and the language in which the participant chose to take the survey (English or Spanish).
4. By splitting the sample, I am investigating whether or not a treatment effect exists within a subsample rather than whether the treatment effect varies across groups. Previous work has similarly split samples based on dyad characteristics to investigate peer influence (Bond et al., 2012; Jones et al., 2017; Fowler & Christakis, 2009; Shakya et al., 2016). I provide a direct test of the difference in treatment effect in the supplementary materials in which I treat age difference as a moderator.

Disclosure statement

No potential conflict of interest was reported by the author.

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