Supplementary Materials for

When contact changes minds: An experiment on transmission of support for gay equality

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When Contact Changes Minds: An Experiment on Transmission of Support for Gay Equality

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Materials

Terminology

Throughout, the text we use the terms gay and lesbian rather than a more comprehensive set of sexual orientations (LGBT — lesbian, gay, bisexual, transgender) to follow the wording of the outcome measures described below. When describing canvassers, we use "gay" to refer to those who self-identify as LGBT and "straight" to refer to those who do not.

Background of Supreme Court Rulings

1. ***United States v. Windsor:*** In June 2013, the Supreme Court's ruling in struck down section 3 of the Defense of Marriage Act and required the federal government to treat legally married same-sex couples and heterosexual couples on an equal basis. Before that ruling, some states extended full marriage rights to same-sex couples within their borders, but these marriages were not recognized by the federal government.

2. ***Hollingsworth v. Perry:*** On May 15, 2008, the Supreme Court of California issued a decision in which it effectively legalized same-sex marriage in California, holding that California's existing opposite-sex definition of marriage violated the constitutional rights of same-sex couples. Same-sex marriage opponents in California placed a state constitutional amendment known as Proposition 8 on the November 2008 ballot for the purpose of restoring an opposite-sex definition of marriage. Proposition 8 was passed on Election Day 2008, as were proposed marriage-limiting amendments in Florida and Arizona. On August 4, 2010, a decision by the U.S. District Court in *Hollingsworth v. Perry* ruled that Proposition 8 was unconstitutional. The decision in that case was upheld at appeal. Proponents of Proposition 8 appealed to the Supreme Court, which asked to be briefed for arguments concerning the appellants' standing and heard oral arguments on March 26, 2013. The Supreme Court dismissed the case for lack of standing in a decision issued June 26, 2013, after which same-sex marriage once again became legal in California. Same-sex marriages resumed on June 28, 2013.

Survey Instrument

The survey instrument, *California Opinion Study*, is a non-partisan confidential research survey measuring attitudes toward social and public policy issues, economic status, political events and media exposure. Wording and format followed the Cooperative Campaign Analysis Project 2012 (*12*). Topical modules were administered on a rotating basis. Modules focus on current topics such as attitudes toward social groups, immigration, abortion, death penalty, taxes, budget deficit, health care, global warming, affirmative action, trade, economic regulation. Within each module survey questions were presented in random order.

Survey Recruitment

From the 33,941 registered voters targeted for panel recruitment, 4,064 were successfully recruited to participate in the multi-wave survey panel. From this pool of 4,064 registered voters, 5,443 additional respondents were referred, resulting in a total of 9,507 survey panelists who completed a baseline survey prior to the launch of the canvassing experiment. In order to encourage participation in the baseline survey, respondents were paid $10 upon initial enrollment. In an effort to impanel multiple voters per household, individuals were offered $2 (per referral) to refer their friends and family to participate in the survey panel. In order to encourage participation in follow up surveys, respondents were paid $5 per follow-up survey.

Survey Outcome Measures

1. **Support For Same-Sex Marriage:** Wording and format followed the Cooperative Campaign Analysis Project 2012 (Jackman et al 2012): "Do you favor or oppose allowing gays and lesbians to marry legally?" Response options ranged from strongly oppose to strongly favor, forming a five-point scale.

2. **Ratings of Gays and Lesbians:** Wording and format followed the Cooperative Campaign Analysis Project 2012 (Jackman et al 2012): "We would like to get your feelings toward a series of demographic groups. We will display the name of a group, and we would like you to rate the group using a 'feeling thermometer.' Ratings between 50 degrees and 100 degrees indicate that you feel favorable and warm toward the group. Ratings between 0 degrees and 50 degrees mean that you don't feel favorable toward the group and that you don't care too much for that group. You would rate the group at the 50 degree mark if you don't feel particularly warm or cold toward the group. Your rating will appear at the end of the slider."

Canvasser Attributes Study

In order to obtain an objective assessment of canvassers' visible attributes, we invited Mechanical Turk coders to watch and evaluate a video of each canvasser delivering the placebo (recycling) script. In the videos, no canvassers revealed their sexual orientation, and no experimental subjects were filmed. Working independently, raters evaluated each canvasser along the following semantic differential scales: rich/poor, empathetic/not empathetic, likable/not likable, attractive/unattractive,

enthusiastic/unenthusiastic, masculine/feminine, and gay/straight. Table S2 presents average ratings for canvassers, broken down by whether each canvasser self-identifies as gay or straight. We find small and statistically insignificant differences on all measured dimensions other than perceived gay/straight and perceived masculine/feminine. Canvassers, in other words, did not differ on dimensions such as personal attractiveness enthusiasm, empathy, or affluence, which might have otherwise explained apparent differences in treatment effects.

Methods

Identification and Estimation of Causal Effects. Canvassing campaigns such as this one inevitably encounter noncompliance — some of the people who are assigned to the treatment group fail to receive the treatment. The causes of noncompliance may be systematic (some people no longer live at the listed address) or idiosyncratic (some people may be out shopping when canvassers visit). From the standpoint of identification and estimation, we want to remain agnostic about how door-answering behavior relates to outcomes of interest, such as attitudes toward same-sex marriage. Borrowing insights from the statistical literature on noncompliance (*13*, *14*) and the accompanying literature on placebo controls (*15*, *16*), in this section we explain the formal underpinnings of our experimental design.

Our identification strategy is rooted in a flexible, nonparametric potential outcomes framework that has been used to model noncompliance in a wide array of social science experiments (*9*, *13*). Potential outcomes are fixed attributes of each individual that indicate what he or she would do if exposed to a treatment or combination of treatments. Suppose that each person i harbors two potential outcomes: one indicates what her opinion on same-sex marriage would be if she were to converse with a canvasser about same-sex marriage; the other indicates what her opinion would be if she did not.[1] Let $Y_i(1)$ indicate what the expressed opinion would be if this person were canvassed with a same-sex marriage message, and let $Y_i(0)$ indicate what the expressed opinion would be if she were not canvassed with a same-sex marriage message.

The causal effect of the canvassing treatment may be defined as:

$$t_i = Y_i(1) - Y_i(0) \tag{1}$$

In other words, for this individual the treatment effect is defined as the difference between two potential states of the world, one in which this person is canvassed with a same-sex marriage message and another in which she receives no message of this kind from a canvasser. In an actual experiment, we observe people who have been treated or not, never both; hence, the causal effect for a given individual, expressed in equation (1), remains unobserved. When the administration of treatments encounters noncompliance, the average treatment effect in the subject pool cannot be estimated without invoking strong assumptions. However, under somewhat weaker assumptions an experiment that encounters noncompliance may identify average treatment effects among subgroups within the overall subject

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[1]By positing potential outcomes that respond solely to the treatment that a given subject receives, we invoke the Stable Unit Treatment Value Assumption (*17*), which implies that potential outcomes are unaffected by which set of subjects receives the treatment. Our model, however, does make some allowance for interpersonal spillovers within households; see below.

pool. We next describe those subgroups and assumptions.

Suppose we seek to estimate the average treatment effect among those who would answer the door if assigned to receive a visit from a canvasser. A placebo-controlled experimental design deploys canvassers with two messages, one of which is randomly administered to the person who answers the door (*15*). In the context of our study, the treatment message concerns same-sex marriage, and the placebo message concerns the importance of recycling waste. Define the assigned experimental condition of subject i as Z_i, where $Z_i = m$ denotes assignment to the treatment message concerning same-sex marriage, $Z_i = r$ denotes assignment to the recycling message, and $Z_i = c$ denotes assignment to a control group that receives no message whatsoever. Following (*13*), we model the receipt of treatment as a potential outcome. Let $D_i(m)$ be a binary potential outcome that indicates whether a person would answer the door if assigned to be visited by canvassers seeking to convey the treatment message; similarly, $D_i(r)$ indicates whether a person would open the door to a canvasser assigned to convey a recycling message. For any arbitrary treatment assignment $Z_i = z$, let $D_i(z) = 1$ if a person answers the door and 0 otherwise.

We make three assumptions about the implementation of the experiment. First, we assume that treatments are randomly assigned, which means that Z_i is statistically independent of potential outcomes. Second, we assume one-sided noncompliance: no one assigned to the untreated control group receives either the treatment or recycling message. In other words, $D_i(c) = 0$ for all i. Third, we assume that the assigned treatment is unrelated to compliance: a person's potential for contact by canvassers is the same regardless of which message the canvasser is assigned to deliver. Formally, this assumption may be expressed as $D_i(m) = D_i(r)$ for all i. Under these assumptions, a "door-answerer" is someone who would be contacted by a canvasser regardless of whether the assigned script is the treatment or placebo.

In practice, the one-sided noncompliance assumption is met by instructing canvassers to attempt to contact only those assigned to receive a message of some sort. In our experiment, as a precautionary measure, households randomly assigned to "no contact" were omitted from canvassers' walk lists. The third assumption is more delicate, requiring canvassers to maintain symmetry in the manner in which they attempt to contact households. In particular, as canvassers switch back and forth between scripts, they must exert the same effort when contacting subjects regardless of the message to be delivered.[2] Having each canvasser deliver both scripts provides greater leverage in isolating the message effect while controlling for canvasser characteristics.

The average causal effect of the same-sex marriage message among door-answerers may be expressed as

$$E\left[t_i | D_i(m) = 1\right] = E\left[Y_i(1) | D_i(m) = 1\right] - E\left[Y_i(0) | D_i(m) = 1\right], \qquad (2)$$

where $E\left[\cdot\right]$ indicates an expectation. This experimental design provides unbiased estimates of this average causal effect because average outcomes among door-answerers in the treatment group provide an unbiased estimate of $E\left[Y_i(1) | D_i(m) = 1\right]$, and average outcomes among door-answerers in the placebo group provide an unbiased estimate of $E\left[Y_i(0) | D_i(m) = 1\right]$ under the assumption that door-answering is unrelated to the canvasser's message. A properly implemented placebo design thus addresses the issue of non-compliance and identifies the average causal effect among door-answerers.

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[2]When voters answered the door, canvassers asked, verified, and recorded on their walk sheets the panelist they had contacted. (Canvassers were instructed only to speak with one person at a given address.) There were 11 instances where canvassers were told to "go away" by the panelist prior to the completion of the script. Otherwise, the treatments were administered in their entirety. In order to err on the conservative side, we coded these 11 instances as direct contacts.

The causal interpretation of the results generated by this placebo-controlled experiment depend on the assumption that the placebo treatment reveals the same potential outcome ($Y_i(0)$) as no contact whatsoever. This assumption would be jeopardized, for example, if canvassers assigned to discuss recycling defied instructions and instead discussed same-sex marriage. Fortunately, this assumption may be tested by comparing outcomes among those assigned to the untreated control condition to outcomes among those assigned to the placebo script, as in (15) and (16). Under the sharp null hypothesis of no placebo effect,

$$\mathrm{E}\left[Y_i(0)|Z_i = r\right] = \mathrm{E}\left[Y_i(0)|Z_i = c\right], \tag{3}$$

which is testable using sample estimates of these two quantities. Specifically, the researcher would perform a difference-in-means test comparing average outcomes in the assigned control group to average outcomes in the assigned placebo group.[3]

The estimation strategy proposed above is very simple: a comparison of means between those who are contacted by canvassers conveying different scripts. This estimator is unbiased, but it is not as precise as estimators that make use of the fact that a pre-test survey conducted prior to the launch of the experiment asked panelists about their attitudes toward same-sex marriage. This pre-intervention measure provides a valuable covariate that can be used to dampen sampling variability (see (18, 19)). For example, if we define our experimental outcome as change in baseline attitudes toward same-sex marriage (i.e., the post-intervention score minus the pre-intervention score), the difference-in-means estimator again generates unbiased estimates but with much smaller standard errors. In addition, the baseline survey includes questions that were taken directly from the 2012 Cooperative Campaign Analysis Project survey (12), such as party identification, age, religion, religiosity, level of education, race, media use, news interest, and prior contact with gay men and lesbians. When we control for baseline attitudes concerning same-sex marriage or gays and lesbians, inclusion of these covariates in a regression model predicting outcomes has little effect on the results. For ease of presentation, we focus on the raw outcomes or change scores without further covariate adjustment.

Building on research showing that attitude change hinges on the attributes of the messenger (20, 21), we experimentally varied the spokesperson for same-sex marriage. We reasoned that if gay canvassers deliver a message regarding same-sex marriage, the treatment effect might plausibly reflect the effects of conversing with a gay person; conversely, an apparent effect might be due to the script regardless of who delivers it. In order to tease apart the effects of the message and messenger, the design needs to include additional experimental groups. We therefore allocate four different combinations of messages and messengers: same-sex marriage scripts delivered by self-identified gay canvassers, same-sex marriage scripts delivered by self-identified straight canvassers, recycling scripts delivered by self-identified gay canvassers, and recycling scripts delivered by self-identified straight canvassers. This 2x2 design in effect reveals four potential outcomes among door-answerers, with each potential outcome corresponding to a messenger and message pairing. Under random assignment and the assumption that contact is independent of treatment, this design generates unbiased estimates of the average causal effects of each factor as well as their interaction.

This experiment may also be used to identify the average spillover effect — the secondhand effect that may be transmitted to housemates if the canvassing treatment prompts door-answerers to discuss

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[3]This comparison focuses solely on the assigned control and placebo groups, which, due to random assignment, have the same expected potential outcomes. We cannot compare those actually reached by canvassers to the control group because we do not know who in the control group would answer the door if canvassed.

same-sex marriage or gay equality with their housemates. Under random assignment and independence between door-answering and the canvassers' assigned message, the housemates of those who come to the door and receive a same-sex marriage message have potential outcomes that are identical in expectation to the housemates of those who come to the door and receive a recycling message. Let $S_i(j,k)$ be a potential outcome that indicates whether a person's housemate would answer the door if the experimental assignment of the household were a messenger of type j and message of type k; $S_i(j,k) = 1$ if a person's housemate answers the door and 0 otherwise. Similarly, let $Y_i^*(j,k)$ refer to a person's potential outcomes if treated secondhand by a canvasser of type j who delivers a script of type k.[4] The average secondhand effect among the housemates of door-answerers may be expressed as the expected difference between two potential outcomes, each associated with a set of messenger and script pairings, messenger j and script k versus messenger l and script m:

$$\mathrm{E}\left[t_{i,jk,lm}^* | S_i(j,k) = 1\right] = \mathrm{E}\left[Y_i^*(j,k) | S_i(j,k) = 1\right] - \mathrm{E}\left[Y_i^*(l,m) | S_i(j,k) = 1\right]. \tag{4}$$

This average causal effect is identified because among door-answering households, housemates' average observed outcome in the treatment group provides an unbiased estimate of $\mathrm{E}\left[Y_i^*(j,k) | S_i(j,k) = 1\right]$, and housemates' average observed outcome in the placebo group provides an unbiased estimate of $\mathrm{E}\left[Y_i^*(l,m) | S_i(j,k) = 1\right]$ under the assumption that $S_i(j,k) = S_i(l,m)$. For example, suppose we sought to estimate the average secondhand effect of the same-sex marriage message among those whose housemates spoke with straight canvassers. We would compare average outcomes among those whose housemates spoke with straight canvassers about same-sex marriage to average outcomes among those whose housemates spoke with straight canvassers about recycling.

Finally, we employ multiple outcome measures in order to assess the breadth and persistence of opinion change. One outcome measure, drawn from (*12*), is a direct question pertaining to same-sex marriage: "Do you favor or oppose allowing gays and lesbians to marry legally?" Response options ranged from strongly oppose to strongly favor, forming a five-point scale. A second outcome, also drawn from (*12*), is affect toward "gay men and lesbians," as gauged by a standard feeling thermometer that ranges from 0 to 100.[5]

In order to establish that messages and messengers influence opinion in the hypothesized fashion, we also look for a null effect on opinions in domains other than gay rights. For example, we consider feeling thermometer scores concerning an array of social groups beyond gay men and lesbians: Muslims, Jews, Christians, Latinos, Blacks, Asian-Americans, and Whites. We find null effects on these outcome measures as well as measures of policy opinions on issues such as abortion, immigration, taxes, health care, and global warming. The one policy domain in which we would not expect null effects is recycling, since support for recycling could be influenced by the placebo script.

Another important design feature is extensive over-time measurement of outcomes, which allows us to detect growth, stasis, or decay in average causal effects. Follow-up surveys were conducted

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[4]Potential outcomes associated with direct treatments could be expressed using analogous notation to reflect the four message and messenger combinations. In all, the experiment elicits nine types of potential outcomes, four from forms of direct treatments, four from forms of secondhand treatments, and one from a lack of any treatment.

[5]Wording and format followed the Cooperative Campaign Analysis Project 2012 (*12*): "We would like to get your feelings toward a series of demographic groups. We will display the name of a group, and we would like you to rate the group using a 'feeling thermometer.' Ratings between 50 degrees and 100 degrees indicate that you feel favorable and warm toward the group. Ratings between 0 degrees and 50 degrees mean that you don't feel favorable toward the group and that you don't care too much for that group. You would rate the group at the 50 degree mark if you don't feel particularly warm or cold toward the group. Your rating will appear at the end of the slider."

3, 12, 23, 27, 45 and 280 days after the conclusion of canvassing, in Study 1. The use of repeated outcome measures addresses an important critique of persuasion interventions (*22*, *23*), namely, that most experiments measure outcomes in the immediate aftermath of treatment despite evidence that mass communication effects often show signs of rapid decay (*24*, *25*). In our study, the use of repeated measures further enabled us to capture a fortuitous news event, the Supreme Court's decision on the Defense of Marriage Act and Proposition 8 in California – two decisions affirming the right to same-sex marriage. Our multi-wave panel survey gauges not only the persistence of treatment effects but also the interaction between our treatment and the affirmation of social norms by the Court.

To summarize, our experiment is designed to estimate the average treatment effect of the same-sex marriage script among those who potentially answer the door to canvassers and the average spillover effect among their housemates. Assuming that the experimental design is implemented as planned, those who answer the door to one script have the same expected potential outcomes as those who answer the door to another script, and assignment to the recycling script has no influence on opinions about same-sex marriage. These implementation assumptions have testable empirical implications, and both tests fail to reject the null hypothesis of proper implementation. In terms of estimation, access to pre-treatment baseline measures enables us to recode the outcome measure as a change score so as to dampen sampling variability while preserving unbiased estimation. In order to isolate the distinct effects of message and messenger, we use a 2x2 design to allocate all possible combinations of the two factors. Finally, multiple outcome measures and multi-wave follow-up surveys administered online by an ostensibly unrelated firm allow us to assess the domain specificity of treatment effects, the persistence of treatment effects over time, and the interaction between treatment effects and the announcement of two landmark Court decisions.

Verifying Core Design Assumptions. Before estimating the average treatment and spillover effects, we first verify four key assumptions implicit in our experimental design. The first assumption is that random assignment of treatment, placebo, and control groups was properly implemented, which implies that covariate imbalance is no greater than what one would expect by chance. Table S5 compares the covariate distribution for the five assigned groups. When multinomial logistic regression is used to regress treatment assignment on the covariates listed in Table S5, the likelihood ratio statistic, as expected, is non-significant. The p-value for this test is derived using randomization inference, which compares the test statistic of 0.456 to a reference distribution generated by simulating 1,000 random assignments. The second assumption is that the pattern of survey attrition that we observe over successive waves of the panel is unrelated to experimental assignment. Table S6 confirms that consistent with this assumption, rates of attrition are not significantly different across experimental groups. Furthermore, a regression of a dummy variable indicating that a panelist failed to participate in a survey wave on the covariates listed in Table S5 shows no significant interactions with experimental assignment in any survey wave (see Table S7). Regressing missingness on indicator variables for each assigned treatment produces non-significant F-statistics for every survey wave ($p > 0.05$). Regressing missingness on indicators for assigned treatment and interactions with all categories of the eleven covariates also produces F-statistics that are insignificant in every wave ($p > 0.05$). The third assumption is that rates of contact with targeted households do not vary by canvassing script. Table S4 confirms that contact rates do not vary significantly according to treatment assignment. In order to test the null hypothesis that the probability of contact is unaffected by treatment assignment, we regressed contact with a household on treatment assignment and obtained an F-statistic of 0.805, with a p-value of 0.491. We also verified that contact rates are not significantly predicted by the interaction between assigned treatment and the covariates listed in Table S5 and obtained an F-statistic of 0.985, with a

p-value of 0.547. By the same token, subjects in households that were not contacted by canvassers express opinions on the key outcome measures that do not vary significantly according to whether subjects were assigned to treatment or placebo conditions. These insignificant p-values suggest that canvassers did not make a special effort to contact subjects with one message or the other. The fourth assumption is that in survey waves after the intervention occurred, subjects assigned to either placebo group express views toward same-sex marriage and gays and lesbians that are identical in expectation to views expressed by those assigned to the control group. Consistent with this assumption, we find that among those assigned to discuss recycling with straight canvassers, the two-tailed p-values in wave 2 of the survey are 0.542 and 0.794, respectively, for the two outcome measures. Among those assigned to discuss recycling with gay canvassers, the corresponding p-values are 0.241 and 0.333. The lack of a placebo effect suggests that canvassers assigned to administer the recycling script did not broach the subject of same-sex marriage. The lack of a placebo effect among voters assigned to gay canvassers in particular has an interesting substantive implication: opinions on the issue of same-sex marriage are unaffected when voters converse with gay canvassers who neither reveal themselves to be gay nor raise the issue of gay rights.

Figures

Figure S1: **Canvassing Message Scripts**

Same-Sex Marriage Script

[Check walk list before knocking to confirm conversation type] Hi, are you [_____]? My name is [_____]. I'm a volunteer with the Leadership LAB and we're talking to voters in your neighborhood about marriage for gay and lesbian couples.

(if Voter is not on list, but wants to talk…) I'm sorry, I'm only supposed to talk to voters on my list. It will only take a few minutes, is _____ available?... *(if still no)* Ok, thank you for your time, we'll try back some other day.

<u>*Mar Start ID:*</u> **If you were to vote on this, would you vote in favor of or against gay marriage?**
- **In Favor**
- **Undecided**
- **Against**

[for all voters] **Can you say more about why you feel that way?**

1. Establish Shared Values about Marriage and Relationships

Are you married? / Have you ever been married?
- How has (your) marriage impacted your life and your family? What do you value most about (your) marriage?
- *[Not married]* Do you have any married couples in your life? What do you notice about their relationship?
- ***Share personal experiences*** with marriage/married couples or why marriage matters to you
- When you really love someone, there's just no substitute for marriage. Nothing else clearly says "we've made a lifetime commitment to each other" in the same way. What do you think about that?

2. Highlight Shared Humanity between People who are LGBT and Straight

Do you know anyone who is gay or lesbian? How do you know them? What's their name?
- Is ____ in a relationship? What have you noticed about their relationship? Does their relationship seem similar to the other couples you know? How so?
- Do you remember when you first learned about gay people? What do you remember? What did you think? How did that impact your view of gay and lesbian people?
- ***Share personal experiences*** as an LGBT person or ally and with LGBT people in your life
- Even if it's not your intention, excluding same-sex couples from marriage is part of a bigger message that gay and lesbian people are not as good as straight people. That message has a harmful impact on real gay people (like me / my friend / the person you mentioned). How do you feel about that?

<u>*Mar End ID:*</u> **If you were to vote on this, would you vote in favor of or against gay marriage?**
- **In Favor**
- **Undecided**
- **Against**

3. Crystallize Opinion Change
- *[If Changed Opinion]* Why did you change your mind?
- *[No Change]* Briefly remind voter of the importance of the issue to you
- *[All Voters]:* Why does this decision seem right to you?

BEST PHONE NUMBER: What is the best number to reach you at? (Record on Walk List)
CONFIRM ADDRESS: What is your current mailing address? (Double Check on Walk List, & Record if Different)

Thank you very much for taking the time to have a conversation with me. Have a great day!

Problems? Questions? – Call Field Director at 323-770-XXXX

Recycling Script

[Check walk list before knocking to confirm conversation type] Hi, are you [_____]? My name is [_____]. I'm a volunteer with the Leadership LAB and we're talking to voters in your neighborhood about recycling.

(if Voter is not on list, but wants to talk…) I'm sorry, I'm only supposed to talk to voters on my list. It will only take a few minutes, is _____ available?... *(if still no)* Ok, thank you for your time, we'll try back some other day.

<u>*Rec ID:*</u> We're working on ways to decrease environmental waste. **If an initiative requiring super markets to charge for plastic bags instead of giving them away for free were on the ballot tomorrow, would you vote in favor or against this measure?**
- **In Favor**
- **Undecided**
- **Against**

1. Brief Recycling Appeal

We'd like to remind you that recycling is only effective if everyone participates.

BEST PHONE NUMBER: What is the best number to reach you at? (Record on Walk List)
CONFIRM ADDRESS: What is your current mailing address? (Double Check on Walk List, & Record if Different)

Thank you very much for taking the time to have a conversation with me. Have a great day!

Problems? Questions? – Call Field Director at 323-770-XXXX

Note: For study 2, the following language was added to the introduction of the script, "As you may know, gay marriage is now legal in California but not in most other states. Some places are going to be voting on it in the future, so we're interested in knowing how people are feeling about it."

Tables

Table S1: **Self-Reported Canvasser Attributes in Study 1, by Canvasser Sexual Orientation**

		Canvasser Self-Reported Sexual Orientation	
Canvasser Attribute	*Response Value*	Straight (N=19)	Gay (N=22)
Age	Average	26	29
Race	Caucasian (%)	58	56
Gender	Female (%)	53	47
Canvassing Experience (1-5)	Average	2.6	2.8
How Often Perceived As LGBTQ	Always or Often (%)	24	90

Note: Table shows canvasser attributes by self-reported sexual orientation for the canvassers in Study 1. Data collected from canvasser survey administered at the canvassing training site.

Table S2: **Average Ratings for Canvassers in Study 1, by Self-Identification as Gay or Straight**

		Canvasser Self-Identification	
Canvasser Attribute	*Response Scale*	Straight	Gay
Attractive	Unattractive (1) to Attractive (10)	4.9	4.1
Likability	Unlikable (1) to Likable (10)	7.1	6.6
Empathy	Not Empathetic (1) to Empathetic (10)	2.9	3.4
Affluence	Poor (1) to Rich (10)	5.2	5.9
Sexual Orientation	Heterosexual (1) to Homosexual (10)	3.4	7.2
Overall Feeling Thermometer	Negative (0) to Positive (100)	68.4	62.7

Note: Table shows canvasser ratings by Mechanical Turk coders (N=27) of the 41 canvassers in Study 1.

Table S3: **Experimental Design for Study 1**

| | Messenger | | | |
Message	Gay Canvasser	Straight Canvasser	None	Row N
Same-Sex Marriage	423	385	–	808
Recycling	389	392	–	781
No Contact	–	–	1943	1943
Column N	812	777	1943	3532

Note: This table shows the distribution of household assignments for the experimental sample by message and messenger. The experimental design includes random assignment into one of five groups – a treatment, placebo and control message (same-sex marriage, recycling and no contact, respectively) delivered by a canvasser who either identifies as gay or straight. The last row of the third column shows the number of households assigned to receive no contact.

Table S4: **Treatment Assigned and Contact by Respondents in Study 1**

| | Form of Contact | | | |
Treatment Assigned	Direct	Secondhand	None	Row N
Same-Sex Marriage Script by Gay Canvasser	132 (11%)	240 (21%)	779 (68%)	1151
Same-Sex Marriage Script by Straight Canvasser	131 (13%)	234 (23%)	668 (65%)	1033
Recycling Script by Gay Canvasser	136 (13%)	234 (22%)	676 (65%)	1046
Recycling Script by Straight Canvasser	120 (12%)	195 (19%)	724 (70%)	1039
No Contact	–	–	5238	5238
Column N	519	903	8085	9507

Note: The table shows the distribution of the experimental sample by treatment assigned and the form of contact each subject received for the control, placebo, and treatment groups. The first column of the table conditions on direct contact; it shows the number and percentage of assigned panelists who received the treatment or placebo script face-to-face at the door. The second column of the table conditions on secondary contact; it shows the number and percentage of panelists who live in the same household as a panelist who was directly contacted and treated. The third column shows the number and percentage of individuals who received no contact.

Table S5: **Covariate Distribution in Study 1, by Assigned Treatment**

		Treatment Assignment			
	Control	*Same-Sex Marriage Message*		*Recycling Message*	
Covariate	No Contact	by Gay Canvasser	by Straight Canvasser	by Gay Canvasser	by Straight Canvasser
Education: What is the highest level of education you have completed?					
Did not graduate from high school	5	4	6	5	5
High school graduate	31	34	32	34	31
Some college, but no degree (yet)	25	24	24	23	25
2-year college degree	10	8	9	7	9
4-year college degree	19	19	20	20	21
Postgraduate degree	10	10	10	11	10
Income: "Thinking back over the last year, what was your family's annual income?"					
$10,000 - $14,999	5	4	3	4	5
$100,000 - $119,999	5	6	4	5	5
$120,000 - $149,999	4	5	4	4	4
$15,000 - $19,999	5	5	5	4	6
$150,000 or more	4	4	5	4	4
$20,000 - $24,999	5	6	6	7	6
$25,000 - $29,999	7	6	7	5	7
$30,000 - $39,999	10	11	11	11	10
$40,000 - $49,999	9	10	11	10	10
$50,000 - $59,999	9	10	9	8	8
$60,000 - $69,999	6	6	5	6	5
$70,000 - $79,999	6	6	7	6	6
$80,000 - $99,999	7	6	6	8	7
Less than $10,000	6	5	6	6	7
Prefer not to say	11	11	10	11	10
Skipped	0	0	0	0	0
Party Registration (From California Voter File)					
Democrat	49	50	52	52	50
Independent	24	26	24	23	24
Republican	27	25	24	25	26
Age (From California Voter File)					
18-30	25	24	25	25	25
31-42	20	22	20	20	21
43-55	23	22	23	20	22
53-67	18	19	19	20	18
68 and Older	14	13	13	14	13
Abortion, "Do you think abortion should be..."					
Illegal in all cases	15	12	13	13	14
Legal in all cases	30	28	32	30	29
Legal/Illegal in some cases	48	53	48	49	50
Not sure	7	7	8	7	7
Interest in news and public affairs, "Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs...?"					
Hardly at all	8	7	9	8	9
Most of the time	49	51	50	50	49
Only now and then	14	13	14	12	14
Some of the time	29	29	27	30	28
Interest in Politics, "How interested are you in politics and current affairs?"					
Not much interested	13	12	15	13	15
Not sure	3	2	3	3	2
Somewhat interested	35	33	33	32	34
Very much interested	49	52	50	51	49

Note: Table continued on next page.

			Treatment Assignment		
	Control	_Same-Sex Marriage Message_		_Recycling Message_	
Covariate	No Contact	by Gay Canvasser	by Straight Canvasser	by Gay Canvasser	by Straight Canvasser
Importance of Religion, "How important is religion in your life?"					
Not at all important	17	20	18	18	19
Not too important	13	15	14	14	13
Somewhat important	27	24	26	27	26
Very important	43	42	42	40	42
Born-Again Christian, "Would you describe yourself as a "born-again"; or evangelical Christian, or not?"					
No	71	70	71	72	73
Yes	29	30	29	28	27
Watch Local TV News, "How many days in a typical week do you watch the local evening news in your area?"					
none	38	37	35	36	40
one	16	17	17	15	18
two	9	10	8	9	8
three	7	5	8	8	7
four	3	3	3	4	3
five	4	3	4	5	3
six	2	3	2	2	1
everyday	21	22	22	21	20
Read Local Newspaper, "How many days in a typical week do you read a local newspaper available in your area?"					
none	38	37	35	36	40
one	16	17	17	15	18
two	9	10	8	9	8
three	7	5	8	8	7
four	3	3	3	4	3
five	4	3	4	5	3
six	2	3	2	2	1
everyday	21	22	22	21	20

Note: Table shows baseline covariates by treatment assigned (control, placebo, and treatment groups) for panelists in the experimental sample. Entries represent the percentage of each experimental group giving the stated response category. Party Registration was collected from the California Voter File. All other covariates were measured during the pre-treatment survey (wave 1). For the number of respondents in each entry see Table S6.

Table S6: **Number of Respondents who Answered Surveys in Each Wave, by Treatment Assigned, Contact and Study**

		Survey Wave						
Treatment Assignment	*Contact*	1	2	3	4	5	6	7
Panel A: Study 1								
Same-Sex Marriage Script by Gay Canvasser	Direct	132	119	119	126	116	120	89
Same-Sex Marriage Script by Straight Canvasser	Direct	131	119	118	118	107	126	97
Recycling Script by Gay Canvasser	Direct	136	123	124	121	120	128	95
Recycling Script by Straight Canvasser	Direct	120	106	106	114	98	112	85
Same-Sex Marriage Script by Gay Canvasser	Secondhand	240	222	213	212	216	227	165
Same-Sex Marriage Script by Straight Canvasser	Secondhand	234	199	206	217	208	226	153
Recycling Script by Gay Canvasser	Secondhand	234	215	217	209	202	223	159
Recycling Script by Straight Canvasser	Secondhand	195	172	175	176	175	183	129
Same-Sex Marriage Script by Gay Canvasser	None	779	677	710	712	683	746	542
Same-Sex Marriage Script by Straight Canvasser	None	668	584	604	607	594	632	469
Recycling Script by Gay Canvasser	None	676	602	619	613	589	643	466
Recycling Script by Straight Canvasser	None	724	657	664	662	629	689	503
No Contact	None	5238	4670	4776	4785	4602	4958	3608
Panel B: Study 2								
Same-Sex Marriage Script by Gay Canvasser	Direct	157	137	131	137	–	–	96
Same-Sex Marriage Script by Gay Canvasser	Secondhand	284	252	236	260	–	–	187
Same-Sex Marriage Script by Gay Canvasser	None	797	704	691	708	–	–	510
No Contact	None	1203	1039	1055	1066	–	–	735
Total N		11948	10597	10764	10843	8339	9013	8088

Note: Entries are the number of respondents who answered each survey wave. Treatment assignment refers to the assigned script and messenger. Contact refers to whether the respondent spoke directly with a canvasser, was the housemate of someone who spoke directly with canvassers, or neither. The six post-treatment surveys occurred 3, 12, 23, 27, 45 and 280 days after canvassing took place.

Table S7: **Regression Estimates Predicting Missingness as a Function of Both Treatment Assignment and Treatment-by-Covariate Interactions, by Survey Wave, in Study 1**

Model 1: Only Treatment Dummies

| | Survey Wave | | | | | |
	2	3	4	5	6	7
F-test p-value for joint significance of treatment dummies	0.256	0.558	0.770	0.815	0.931	0.994
Adjusted R^2	0.00	0.00	0.00	0.00	0.00	0.00
Degrees of Freedom	9502	9502	9502	9502	9502	9502

Model 2: Controlling for Covariates and Treatment by Covariate Interactions

F-test p-value for joint significance of treatment dummies and covariate interactions	0.702	0.250	0.329	0.126	0.773	0.235
Adjusted R^2	0.00	0.00	0.00	0.00	0.00	0.00
Degrees of Freedom	8289	8289	8289	8289	8289	8289
N	9507	9507	9507	9507	9507	9507

Note: Table shows estimates from OLS regression where dependent variable indicates whether the panelist took the follow-up Internet survey at each wave (1: panelist took survey; 0: panelist did not take the survey). The top panel shows estimates using only treatment dummies and the bottom panel shows estimates controlling for covariates shown in Table S5. Regressing missingness on indicator variables for each assigned treatment produces non-significant F-statistics for every survey wave ($p > 0.05$). Regressing missingness on indicators for assigned treatment and interactions with all categories of the fourteen covariates also produces F-statistics that are insignificant in every wave ($p > 0.05$).

Table S8: **Direct Effects of Canvassing.** Support for Same-Sex Marriage and Rating of Gays and Lesbians, by Survey Wave and Assigned Treatment, Among Those who Spoke Directly with Canvassers in Study 1

	Survey Wave						
Treatment Assignment	1	2	3	4	5	6	7
Panel A: Same-Sex Marriage Levels							
Same-Sex Marriage Script by Gay Canvasser	3.17 (0.15)	3.59 (0.14)	3.64 (0.14)	3.70 (0.13)	3.93 (0.11)	3.92 (0.12)	3.90 (0.14)
Same-Sex Marriage Script by Straight Canvasser	3.05 (0.14)	3.51 (0.13)	3.10 (0.13)	3.07 (0.13)	3.26 (0.14)	3.15 (0.15)	3.16 (0.17)
Recycling Script by Gay Canvasser	3.09 (0.15)	3.08 (0.14)	2.96 (0.15)	3.05 (0.14)	3.20 (0.14)	3.17 (0.15)	3.31 (0.17)
Recycling Script by Straight Canvasser	3.17 (0.15)	3.08 (0.15)	3.06 (0.15)	3.11 (0.15)	3.24 (0.16)	3.23 (0.15)	3.12 (0.18)
Panel B: Same-Sex Marriage Change							
Same-Sex Marriage Script by Gay Canvasser		0.46 (0.05)	0.49 (0.06)	0.48 (0.07)	0.83 (0.09)	0.75 (0.07)	0.91 (0.14)
Same-Sex Marriage Script by Straight Canvasser		0.50 (0.05)	0.14 (0.06)	0.03 (0.07)	0.24 (0.09)	0.07 (0.04)	0.23 (0.10)
Recycling Script by Gay Canvasser		0.06 (0.05)	-0.02 (0.04)	-0.03 (0.06)	0.15 (0.07)	0.04 (0.03)	0.14 (0.11)
Recycling Script by Straight Canvasser		-0.07 (0.07)	-0.08 (0.05)	-0.04 (0.05)	0.08 (0.07)	0.04 (0.03)	0.15 (0.11)
Panel C: Gay Feeling Thermometer Levels							
Same-Sex Marriage Script by Gay Canvasser	61.50 (2.42)	72.09 (2.31)					75.10 (2.75)
Same-Sex Marriage Script by Straight Canvasser	57.66 (1.95)	63.43 (2.05)					63.77 (2.19)
Recycling Script by Gay Canvasser	60.60 (2.66)	58.67 (2.78)					66.54 (2.95)
Recycling Script by Straight Canvasser	59.81 (2.57)	60.01 (2.75)					65.51 (2.50)
Panel D: Gay Feeling Thermometer Change							
Same-Sex Marriage Script by Gay Canvasser		10.60 (0.68)					15.10 (1.90)
Same-Sex Marriage Script by Straight Canvasser		5.96 (0.47)					6.14 (1.62)
Recycling Script by Gay Canvasser		-0.64 (0.41)					4.46 (2.24)
Recycling Script by Straight Canvasser		-0.47 (0.35)					5.36 (2.39)
N	519	467	467	479	441	486	366

Note: Panel A shows average responses to the same-sex marriage question (1: Strongly Opposed to 5: Strongly Support). Panel B shows average changes in support for same-sex marriage, where changes are the difference from pre-treatment survey (wave 1). Panel C shows average ratings of gays and lesbians on the feeling thermometer scale (0-100). Panel D shows changes from baseline ratings of gays of lesbians. Standard errors are shown in parenthesis. The Los Angeles LGBT Center canvassing intervention was administered between Internet survey waves 1 and 2. The U.S. Supreme Court decisions were announced between waves 4 and 5. Change scores may diverge from differences in wave-specific averages because some voters who respond to the baseline wave fail to respond in subsequent waves.

Table S9: **Secondhand Effects.** Support for Same-Sex Marriage and Rating of Gays and Lesbians, by Survey Wave and Assigned Treatment, Among Housemates of Those who Spoke Directly with Canvassers in Study 1

Treatment Assignment	Survey Wave						
	1	2	3	4	5	6	7
Panel A: Same-Sex Marriage Levels							
Same-Sex Marriage Script by Gay Canvasser	3.02 (0.12)	3.22 (0.12)	3.15 (0.13)	3.23 (0.12)	3.39 (0.11)	3.33 (0.11)	3.40 (0.12)
Same-Sex Marriage Script by Straight Canvasser	3.06 (0.11)	3.09 (0.12)	3.08 (0.12)	3.07 (0.11)	3.27 (0.11)	3.13 (0.11)	3.08 (0.12)
Recycling Script by Gay Canvasser	3.16 (0.12)	3.13 (0.13)	3.17 (0.12)	3.17 (0.13)	3.36 (0.12)	3.26 (0.12)	3.42 (0.14)
Recycling Script by Straight Canvasser	2.95 (0.13)	3.01 (0.13)	2.92 (0.14)	2.98 (0.14)	3.09 (0.13)	2.97 (0.13)	2.91 (0.15)
Panel B: Same-Sex Marriage Change							
Same-Sex Marriage Script by Gay Canvasser		0.21 (0.03)	0.08 (0.02)	0.11 (0.04)	0.40 (0.04)	0.35 (0.04)	0.52 (0.09)
Same-Sex Marriage Script by Straight Canvasser		0.03 (0.02)	-0.01 (0.02)	0.00 (0.02)	0.19 (0.03)	0.08 (0.02)	0.17 (0.08)
Recycling Script by Gay Canvasser		0.00 (0.02)	0.00 (0.02)	-0.01 (0.01)	0.17 (0.05)	0.06 (0.02)	0.10 (0.07)
Recycling Script by Straight Canvasser		-0.02 (0.02)	-0.05 (0.02)	-0.03 (0.02)	0.21 (0.05)	0.02 (0.02)	0.15 (0.09)
Panel C: Gay Feeling Thermometer Levels							
Same-Sex Marriage Script by Gay Canvasser	56.90 (1.71)	58.24 (1.74)					68.02 (1.90)
Same-Sex Marriage Script by Straight Canvasser	58.74 (1.89)	58.77 (2.00)					63.33 (2.00)
Recycling Script by Gay Canvasser	56.40 (1.98)	55.4 (2.17)					62.66 (2.26)
Recycling Script by Straight Canvasser	57.61 (2.48)	58.76 (2.45)					60.79 (2.48)
Panel D: Gay Feeling Thermometer Change							
Same-Sex Marriage Script by Gay Canvasser		2.04 (0.22)					10.25 (1.12)
Same-Sex Marriage Script by Straight Canvasser		-0.03 (0.12)					5.43 (1.47)
Recycling Script by Gay Canvasser		-0.15 (0.13)					3.23 (1.34)
Recycling Script by Straight Canvasser		-0.26 (0.11)					3.53 (1.43)
N	903	808	811	814	801	859	606

Note: Panel A shows average responses to the same-sex marriage question (1: Strongly Opposed to 5: Strongly Support). Panel B shows average within-subject changes in support for same-sex marriage, where changes are the difference from pre-treatment survey (wave 1). Panel C shows average ratings of gays and lesbians on the feeling thermometer scale (0-100). Panel D shows average within-subject changes from wave 1 ratings of gays of lesbians. Standard errors (shown in parenthesis) are clustered at the household level. The Los Angeles LGBT Center canvassing intervention was administered between Internet survey waves 1 and 2. The U.S. Supreme Court decisions were announced between waves 4 and 5. Change scores may diverge from differences in wave-specific averages because some voters who respond to the baseline wave fail to respond in subsequent waves.

Study 2: A Follow-Up Experiment to Verify Key Findings.

Given the unexpectedly strong direct and secondhand treatment effects produced by gay canvassers in Study 1, we sought to verify the key results through an additional round of data collection. A new subject pool of panel respondents was recruited in a different region of Los Angeles County using the same criteria as in Study 1. A baseline survey was conducted on August 2, 2013. Four days later, canvassers visited randomly selected addresses, and three waves of follow-up interviews were conducted 8, 20, 34 and 209 days after canvassing concluded. Unlike Study 1, which assigned households to treatment, placebo, or control groups, Study 2 only assigned households to treatment and control, as shown in Table S10. Because the most striking treatment effects in Study 1 were associated with gay canvassers, Study 2 fielded only gay canvassers. Canvassing scripts and survey questionnaires used the same wording as in Study 1. Again, as documented in Table S12, sample attrition is not significantly related to treatment assignment or to treatment-by-covariate interactions.

Table S10: **Experimental Design for Study 2**

	Messenger	
Message	Gay Canvasser	None
Same-Sex Marriage	437	–
No Contact	–	422

Note: This table shows the distribution of household assignments for the experimental sample by message and messenger. The experimental design includes random assignment to one of two groups – a treatment and control message (same-sex marriage and no message, respectively) delivered by either a canvasser who identifies as gay or no canvasser.

Table S11: **Treatment Assigned and Contact in Study 2**

	Form of Contact			Row *N*
Treatment Assignment	Direct	Secondhand	None	
Same-Sex Marriage Script by Gay Canvasser	157 (12.7%)	284 (22.9%)	797 (64.4%)	1238
No Contact	–	–	1203	1203
Column *N*	157	284	2000	2441

Note: This table shows the distribution of individual assignments in Study 2 by treatment assignment and form of contact received.

Table S12: **Number of Respondents who Answered Surveys in Each Wave of Study 2, by Treatment Assigned and Contact**

Treatment Assignment	Contact	Survey Wave				
		1	2	3	4	7
Same-Sex Marriage Script by Gay Canvasser	Direct	157	137	131	137	96
Same-Sex Marriage Script by Gay Canvasser	Secondhand	284	252	236	260	187
Same-Sex Marriage Script by Gay Canvasser	None	797	704	691	708	510
No Contact	None	1203	1039	1055	1066	735
N		2441	2132	2113	2171	1528

Note: Entries are the number of respondents who answered each survey wave. Treatment assignment refers to the assigned script and messenger. Contact refers to whether the respondent spoke directly with a canvasser, was the housemate of someone who spoke directly with canvassers (Secondhand), or neither.

Table S13: **Study 2 Results.** Support for Same-Sex Marriage & Rating of Gays & Lesbians, by Survey Wave, Assigned Treatment, & Form of Contact

		Survey Wave				
Treatment Assignment	*Contact*	1	2	3	4	7
Panel A: Same-Sex Marriage Levels						
Same-Sex Marriage Script by Gay Canvasser	Direct	2.88 (0.13)	3.54 (0.13)	3.60 (0.13)	3.63 (0.12)	3.76 (0.14)
Same-Sex Marriage Script by Gay Canvasser	Secondhand	2.91 (0.11)	3.13 (0.11)	3.18 (0.11)	3.04 (0.11)	3.05 (0.12)
Same-Sex Marriage Script by Gay Canvasser	None	3.01 (0.06)	3.03 (0.07)	2.98 (0.07)	3.01 (0.06)	3.08 (0.07)
No Contact	None	2.97 (0.05)	2.99 (0.05)	2.95 (0.05)	2.97 (0.05)	2.93 (0.06)
Panel B: Same-Sex Marriage Change						
Same-Sex Marriage Script by Gay Canvasser	Direct		0.67 (0.07)	0.75 (0.08)	0.78 (0.08)	0.91 (0.14)
Same-Sex Marriage Script by Gay Canvasser	Secondhand		0.21 (0.03)	0.20 (0.03)	0.14 (0.03)	0.51 (0.09)
Same-Sex Marriage Script by Gay Canvasser	None		0.04 (0.02)	0.01 (0.02)	0.01 (0.02)	0.10 (0.05)
No Contact	None		0.02 (0.02)	0.00 (0.01)	0.00 (0.02)	0.09 (0.04)
Panel C: Gay Feeling Thermometer Levels						
Same-Sex Marriage Script by Gay Canvasser	Direct	58.14 (2.42)	71.18 (2.43)	70.48 (2.32)	73.67 (2.17)	72.61 (2.61)
Same-Sex Marriage Script by Gay Canvasser	Secondhand	57.08 (1.71)	58.8 (1.86)	61.04 (1.87)	60.31 (1.71)	63.42 (1.95)
Same-Sex Marriage Script by Gay Canvasser	None	60.40 (0.92)	60.12 (0.98)	60.16 (1.00)	60.59 (0.95)	63.70 (1.04)
No Contact	None	57.89 (0.88)	57.64 (0.95)	57.80 (0.90)	57.28 (0.95)	60.40 (0.96)
Panel D: Gay Feeling Thermometer Change						
Same-Sex Marriage Script by Gay Canvasser	Direct		13.6 (0.88)	12.96 (1.02)	15.22 (1.13)	14.61 (2.09)
Same-Sex Marriage Script by Gay Canvasser	Secondhand		2.62 (0.60)	2.88 (0.48)	2.72 (0.49)	7.33 (1.17)
Same-Sex Marriage Script by Gay Canvasser	None		-0.22 (0.26)	0.34 (0.34)	0.32 (0.26)	3.02 (0.65)
No Contact	None		-0.25 (0.20)	-0.16 (0.24)	-0.39 (0.22)	3.12 (0.57)
N		2441	2132	2113	2171	1528

Note: Standard errors clustered at the household level for Secondhand, None, and No Contact categories. Because the design of Study 2 did not include a placebo group, we identify and estimate the CACE slightly differently and apply this approach to Study 1 for comparability. In contrast to the text, where compliers are those who speak directly to canvassers, here compliers are those who would answer the door themselves or whose housemate would answer the door if assigned to the treatment group, and the CACE is a weighted average of the direct and secondhand effects defined in the text. The CACE is estimated by comparing average outcomes in the assigned treatment and control groups and dividing by the rate of contact in the assigned treatment group (*13*). Using this estimator, we find that Studies 1 and 2 yield similar results. As applied to change in support for same-sex marriage in wave 2, the estimated CACE in Study 1 is $(0.10 - -0.01)/((119 + 222)/1018) = 0.33$. The corresponding estimate for Study 2 is $(0.16 - 0.015)/((137 + 252)/1093) = 0.41$. For thermometer ratings, the estimated CACEs are 5.4 and 6.8 for Study 1 and 2, respectively.

Study 3: A Follow-Up Experiment Testing The Effects of Passive Contact With An Openly Gay Person

In 2014, a randomized placebo-controlled experiment was conducted in Southern California to assess whether gay canvassers' conversations with voters about abortion (during which gay canvassers revealed their sexual orientation in passing) increased support for same-sex marriage or gay men and lesbians more generally. Subjects for this study were randomly sampled from the assigned control group in Study 1. Simple random assignment to experimental condition occurred at the household level to facilitate the analysis of within-household extended contact effects. The first group was assigned to receive the abortion script from gay canvassers who revealed their orientation in the course of the conversation. The second group was assigned to receive the placebo recycling script from the same set of gay canvassers, who did not reveal their orientation. Five days and thirty days after canvassing occurred, participants in the study were invited to complete a post-treatment on-line survey interview, largely dealing with issues other than gay rights. The results, shown in Table S14, demonstrate that *passive* contact with gay canvassers, who make their sexual orientation known but do not *actively* engage the issue of gay equality, produces neither increased support for same-sex marriage nor increased ratings of gay men and lesbians more generally.

Table S14: **Effect of Abortion Conversation with Openly Gay Canvassers on Support for Gay Marriage and Ratings of Gays and Lesbians**

Assigned Treatment	Ratings of Gays and Lesbians		Same-Sex Marriage	
	5 days	30 days	5 days	30 Days
Gay Canvassers				
Abortion	1.02 (1.56)	0.26 (2.22)	0.02 (0.04)	-0.02 (0.06)
Recycling	0.67 (1.49)	0.55 (2.18)	-0.01 (0.04)	0.03 (0.06)
Treatment Effect	**0.35 (2.16)**	**-0.29 (3.11)**	**-0.03 (0.06)**	**-0.05 (0.08)**
N	102	76	102	76

Note: Entries in plain type are the average changes in support for same-sex marriage, where changes are the difference from pre-treatment survey responses. Standard errors are in parentheses. During the course of conversations about abortion, which focused on dealing with difficult personal choices, the five canvassers revealed their gay sexual orientation to the voter. The same five gay canvassers did not reveal their sexual orientation in the course of discussing recycling. Entries in boldface indicate the estimated average treatment effect of message for gay messengers. The same-sex marriage question (SSM) drawn from (*12*) reads: "Do you favor or oppose allowing gays and lesbians to marry legally?" Response options ranged from strongly oppose (1) to strongly favor (5), forming a five-point scale. The gay and lesbian feeling thermometer forms a 100 point scale, with 0 indicating negative rating towards the group and ratings between 50 degrees and 100 degrees indicating a favorable rating. Treatment assigned refers to the randomly assigned message and messenger pairings. N indicates the the number of voters who answered each online survey, approximately 5 days and 30 days post-treatment.

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