Can tailored communications motivate environmental volunteers? A natural field experiment

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Volunteering is a significant component of economic activity. In 1990, volunteer labor accounted for almost 7% of US employment (Bruno Frey and Lorenz Goette 1999). Over 25% of the US population volunteers, of which over 2% are in environmental organizations (Bureau of Labor Statistics 2010).

Organizations that rely on volunteers communicate with their volunteers using a variety of media, including newsletters, emails, 'thank you' notes etc. This paper is a field experiment investigating whether tailoring the content of these communications to the stated motivations of a volunteer (Gil Clary et al. 1998) has a positive effect on the number of hours he/she volunteers.

To illustrate this, consider two volunteers, Alex and Robin, performing the same task for a humanitarian charity. Alex is motivated primarily by an altruistic desire to help others, while Robin is motivated primarily by a desire to acquire career-relevant skills. Both receive a monthly newsletter reporting the charity's latest activities. If, rather than sending them both the same newsletter, we send Alex a newsletter emphasizing the positive humanitarian consequences of volunteering, and we send Robin a newsletter emphasizing the career benefits of volunteering, will they both work harder?

For the organization with which we cooperated for this field experiment, we find that in general, this kind of tailoring has no effect on the hours volunteered. However we find that for new volunteers who are primarily motivated by career concerns, there is a substantial, positive

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effect on the number of hours volunteered of tailoring a newsletter to their stated preferences.

In light of potential contrast effects (Thomas Mussweiler and Fritz Strack 1999) we also investigated whether telling volunteers that the tailoring was going to occur interacted with the treatment effect of tailoring materials. We found no evidence of an interaction.

Our paper has several contributions. First, there is growing evidence that in the domain of philanthropy, extrinsic (financial) incentives can crowd out intrinsic incentives (Roland Benabou and Jean Tirole 2006). Our intervention is novel, does not suffer from this potential pitfall, and has the added advantage of being extremely inexpensive.

Second, we are able to reliably identify the causal effect by using randomized control. Third, the benefits of randomized control do not come at the expense of imposing an artificial environment. This is especially important given that we are investigating social preferences (see Steven Levitt and John List 2007).

I. Background

Within economics, the literature on operationalizeable techniques for motivating volunteers is small.[1] An emergent result is that financial incentives can have an adverse effect on philanthropy (Benabou and Tirole 2006). By directly studying what motivates volunteers, the study by Clary et al. (1998) yields policy recommendations. The authors classify volunteers according to the following motivations.

1. Values: expressing values related to altruistic and humanitarian concerns for others.

2. Understanding: seeking new learning experiences.

3. Social: being with friends and doing something of which friends approve.

4. Career: career-related benefits, e.g., signaling personality traits or improving contacts.

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[1] See, for example, Paul Menchik and Burton Weisbrod (1987), Richard Freeman (1997), and Frey and Goette (1999) and other studies cited in the review article Rene Bekkers and Pamala Wiepking (2007).

5. Protective: protecting one's ego from the negative features of one's self.

6. Enhancement: a way of maintaining and enhancing positive affect, including self esteem.

The authors devise a survey for identifying a volunteer's primary motivation. It comprises of 30 questions that are answered using a Likert scale (see the appendix). For example, *on a scale of 1-to-7, please indicate how important or accurate the following was for you in your decision to volunteer at the [organization]: "I am concerned about those less fortunate than myself."* Clary et al. (1998) validate the survey via several studies.

Let X denote the factor that is most important to a volunteer, and let X' denote the factor most strongly associated with a volunteer stimulus, e.g., a volunteering brochure. Finally let Y denote a volunteering outcome, e.g., hours worked by the volunteer.

Clary et al. (1998) study the causal effect of greater congruence between X and X' on Y.[2] For their studies on actual (rather than hypothetical) volunteer commitment and satisfaction, X' was not randomized; the congruence between X and X' was reported by the volunteers. Consequently the possibility of endogeneity is a concern. We employ randomized control.

As Clary et al. (1998) suggest, the practical application of their survey would be to manipulate volunteers' tasks to match their volunteering motives, e.g., a career type volunteer should be assigned tasks that help him/her build skills and generate contacts, while a social volunteer should be assigned tasks with their friends. This is typically unfeasible in practice.

The huge literatures on stereotyping and priming (Ap Dijksterhuis et al. 2000) offer an alternative: manipulate the communications materials that the volunteer receives as part of

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[2] It is clear that Clary et al. (1998) is a paper that is concerned with much more than estimating this causal effect. As policy-oriented economists, this is our primary interest. Thus we believe that our criticisms and refinement of their design stem primarily from our narrower emphasis than from their overlooking these possibilities when they conducted their study.

his/her association with the organization, e.g., newsletters, 'thank you' cards etc.[3]

If such an effect exists, then this would be of particular interest to the managers of organizations that depend upon volunteers. This is because manipulating communications is cheap and logistically straightforward.

II. Experimental design

The Center for Economic Progress (CEP) is a non-profit organization in Chicago, IL.[4] The volunteers receive communications from the CEP throughout the year. The opportunity to collaborate with us on a research project presented itself.

The main research question is: is it possible to have a substantial impact upon volunteer behavior by tailoring communications to volunteers by their type? Further, is any such impact robust to the volunteers being aware that the tailoring is going on?

The key advantage of running an experiment is the use of randomized control to sidestep endogeneity problems. Further, one of the advantages of working with the CEP is that we could test these hypotheses in a natural environment (Glenn Harrison and John List 2004), especially given the centrality of social preferences to the behavior under study (Levitt and List 2007).

This experiment has two stages: administering the survey (to assess volunteer types) and tailoring communications. We administered the surveys during the mandatory training sessions that took place in January.

Volunteer type was determined by the category in which the volunteer had the highest average stated importance. Of the six possible volunteer types, protective and social types were incredibly infrequent (less than 3%) in our sample. We reclassified these volunteers by their

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[3] This is essentially an extension of the Clary et al. (1998) study that used promotional materials except that we are holding the task constant and the materials that we are manipulating are the active, internal communications of the organization.

[4] http://www.economicprogress.org/

secondary type. Each volunteer was also assigned an opposite type, which was the category with the lowest average stated importance.

The literature on anchoring (e.g., Mussweiler and Strack 1999) demonstrates that when people are aware that their attention is being intentionally directed, this can generate contrast effects, i.e., a zero or possibly negative treatment effect. Even if the CEP never discloses its intentions, if it tailors year-upon-year, volunteers could plausibly infer the plan on their own.

To investigate this, we administered two versions of the survey. The control (non-disclosure) made no mention of the CEP's intention to tailor communications. The treatment (full-disclosure) added the following paragraph to the instructions:[5]

"These surveys help us understand what motivates each of you to volunteer for us, something that we are extremely grateful that you have decided to do. Knowing this, we can adapt our communications with you so that they stress the aspects of volunteering at the CEP that most appeal to you."

We manipulated two communications during volunteering season (Jan 1st – April 15th):

1. A mid-term update letter from the CEP's executive director delivered by email on March 10th
2. A mid-term newsletter delivered by mail on March 22nd

We produced four versions of each of these communications: a generic version, which was what the CEP would have produced without our intervention, and then four tailored versions (career, enhancement, understanding, values). Generic versions usually touched upon all of the four versions, but the emphasis was almost always on values-type volunteers.[6] We worked with the CEP to produce tailored versions that were still natural. We had three treatments.

1. Badly matched (control 1): volunteer receives version corresponding to his/her *opposite* type.

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[5] Volunteer type based on the survey was not affected by whether the survey was non- vs. full-disclosure.
[6] Creating a longer version that appealed to all types carries two risks: first, it will not be read due to length. Second, the message targeting each type will be diluted.

2. Generic (control 2): volunteer receives generic version.

3. Well matched (treatment): volunteer receives version corresponding to his/her type.

We included the generic treatment to ensure the availability of a good counterfactual to our study. We included the badly matched treatment to maximize power. The proportions were 40% badly matched, 20% generic and 40% well matched.

Prediction 1: Volunteers who receive well matched materials work more hours than those who receive generic materials and/or those who receive badly matched materials.

Prediction 2: The treatment effect of receiving well matched vs. badly matched (or generic) materials on hours worked will be larger for those unaware of the matching (<u>non-disclosure</u>) than those who are aware of it (<u>full-disclosure</u>).

Prediction 3: The accuracy of predictions 1 and 2 differs by volunteer type.

We expect the smallest treatment effect for values types. This is because the communications that the CEP typically employs, including the generic versions of our interventions and the communications prior to our interventions, are essentially geared towards values types. The starkest difference between our tailored materials and the generic versions are in the career and enhancement versions.

III. Results

Our sample size is 432 new CEP volunteers. The dependent variable in our models is the number of hours volunteered (using the logarithm of hours affects none of our results). Just over one third of the total hours volunteered occurred after our first intervention. The average hours volunteered were 25 per volunteer, with a standard deviation of 22 hours.

The treatment group are the volunteers who received well-matched materials. The control group is volunteers who received badly-matched materials combined with volunteers who

received generic materials. (Using either of the sub-groups alone does not affect our results.) We were able to administer the full-disclosure version of the survey to 40% of the volunteers.

Table 1: Regression results

Model	1	2a	2b	3
D_{pre}	0.016	0.051	-0.0067	0.046
	(0.019)	(0.030)	(0.025)	(0.043)
D_{email}	0.022	0.058	-0.0031	0.046
	(0.029)	(0.045)	(0.037)	(0.065)
$D_{newslettter}$	-0.0068	-0.0070	-0.0075	0.13*
	(0.025)	(0.039)	(0.032)	(0.057)
Full/non-disclosure	Both	Full	Non	Both
Volunteer type	All	All	All	Career
Observations	42768	17127	25641	7425
Clusters	432	173	259	75
R^2	0.079	0.11	0.067	0.084

Notes: The dependent variable in all models is hours worked. All regressions include a constant, time effects and random effects. Standard errors are in parentheses. * denotes statistical significance at the 5 percent level.

Result 1: The treatment effect of receiving well matched materials on hours worked is insignificantly different from zero. (This assumes a treatment effect that does not vary by type.)

We estimate the treatment effect using panel regressions. The results are in Table 1.

$$h_{it} = \alpha + \sum_{s=2}^{99} \theta_s T_s + \beta_{pre} D_{pre} + \beta_{email} D_{email} + \beta_{newsletter} D_{newsletter} + u_i + \varepsilon_{it}$$

i denotes volunteer and t denotes day. h denotes hours worked. T_t is a time effect. Let D be the treatment dummy variable, let $t = 67$ correspond to 3/11 (the date of the first intervention) and let $t = 80$ correspond to 3/25 (the date of the second intervention). Then $D_{pre} = D \; if \; t < 67; D_{pre} = 0 \; if \; t \geq 67,$ $\qquad D_{email} = D \; if \; 67 \leq t < 80; D_{email} = 0 \; if \; t <$

$67 \ or \ t \geq 80$, $D_{newsletter} = D \ if \ t \geq 80; D_{newsletter} = 0 \ if \ t < 80$. Finally u is a random effect and ε is pure white noise. Regressions produce a treatment effect that is trivial in size (less than 0.15 hours per week per volunteer; see Model 1) and a p-value well in excess of 10%.

Result 2: The treatment effect of receiving well matched materials on hours worked is not affected by the volunteers being aware of the matching (full-disclosure) vs. unaware of the matching (non-disclosure).

Again, the treatment effect for both groups is trivial in size (less than 3½ minutes a day or even negative; see Models 2a and 2b) with a p-value well in excess of 10%.

Result 3: The treatment effect of receiving well matched materials on hours worked is positive for career type volunteers.

For a sample of size 75, the estimated treatment effect is economically and statistically significant: 0.9 hours per week per volunteer over the 3 weeks of volunteering season that remain after the newsletter.[7] The treatment effect is insignificant for all other volunteer types.

IV. Conclusion

Our main research question was: is it possible to have a substantial impact upon volunteer behavior by tailoring communications to volunteers by their motivation type? Further, is any such impact robust to the volunteers being aware that the tailoring is going on? A positive answer to the first question would be of interest to environmental (and other) organizations that rely on volunteers, especially given how inexpensive it would be to exploit such an effect.

We collected data using randomized control in a natural setting. Within the confines of the organization with which we collaborated (the CEP), we find that in general, the answer to both research questions is negative. However for the subgroup that is new, career type

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[7] Further estimation reveals that the positive treatment effect is driven specifically by the tailoring of career-type materials to career-type volunteers rather than the receipt of career-type materials.

volunteers, we find a positive effect of tailoring.

To some extent, the general inefficacy of this method is not surprising. The mechanism relies on a mixture of stereotyping and priming. The schemata that underlie stereotyping effects are much more malleable early in their formation (analogously to Bayesian updating), while priming effects typically evaporate within minutes of the stimulus. If we were to somehow require a volunteering decision immediately after a volunteer read their assigned newsletter, we would have plausibly expected a stronger treatment effect. However even if that were feasible, it would not be representative of decision-making by volunteers.[8]

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[8] It would be more appropriate for decisions such as door-to-door solicitations for charitable contributions (see, e.g., Craig Landry et al. 2006).

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