

Getting Out the Vote With Evaluative Thinking

Yosef Bhatti¹, Jens Olav Dahlgaard²,
Jonas H. Hansen², and Kasper M. Hansen²

American Journal of Evaluation
2015, Vol. 36(3) 389-400
© The Author(s) 2015
Reprints and permission:
sagepub.com/journalsPermissions.nav
DOI: 10.1177/1098214015588183
aje.sagepub.com



Abstract

Democratic institutions often do not evaluate their instruments. By working closely with authorities, we developed a field experiment to examine an initiative to increase voter turnout among 18-year-olds that had not previously been evaluated. Particular attention was paid to developing an appropriate program theory and to designing the evaluation in a manner that was consistent with legal and ethical requirements. The program distributed different versions of mobilization letters to the newly enfranchised voters. The treatment effect was positive on turnout and diminished the gap in turnout across population groups, and the effects of the treatments were strongest for individuals with the lowest initial propensity to vote. Cost-effectiveness analysis indicated that the price of an additional vote was approximately USD\$136. Our findings influenced policy design and helped establish the principle of evaluative thinking as an integrated part of the future program.

Keywords

voter turnout, field experiment, get-out-the-vote, humor, nudging

Introduction

Voter turnout across established democracies has declined (International IDEA, 2015). Low turnout rates among young voters are considered a major challenge because voting habits are established during the early years of eligibility (Bhatti & Hansen, 2012; Dinas, 2012; Plutzer, 2002; Wolfinger & Rosenstone, 1980). Turnout mobilization experiments, often termed “Get-Out-The-Vote” (GOTV) studies, have primarily been conducted during U.S. elections (Green & Gerber, 2008; Green, McGrath, & Aronow, 2013). We applied insights from the U.S. context to a high saliency election with proportional representation and a strong voting norm, that is, municipal elections in Denmark. In 2009, turnout for the municipal elections was 65.8%, a comparatively high turnout rate. Nevertheless, the public was concerned about declining turnout, especially

¹ KORA—Danish Institute for Local and Regional Government Research, Copenhagen, Denmark

² Department of Political Science, University of Copenhagen, Copenhagen, Denmark

Corresponding Author:

Kasper M. Hansen, Department of Political Science, University of Copenhagen, Øster Farimagsgade 5, Copenhagen, 1353 K, Denmark.

Emails: kmh@ifs.ku.dk, www.kaspermhansen.eu

among young voters (Bhatti, Dahlgaard, Hansen, & Hansen, 2014; Bhatti & Hansen, 2010). Denmark has a tradition, albeit limited, of trying to engage voters during elections, but these programs were not evaluated. By cooperating with the Danish Parliament, we applied evaluative thinking to a government mobilization program. This project allowed us to evaluate the effects of the program via a field experiment without compromising the main principles or legal and ethical requirements of the program.

Since 2005, the Danish Parliament has mailed copies of the Danish constitution to all citizens when they turn 18 years old, along with a formal letter congratulating them on their 18th birthday and informing them that they are now entitled to vote. The motivation for this program has been to increase electoral participation among young citizens (The Government & Danish People's Party, 2004). However, no one has yet applied evaluative thinking to the program, which is not atypical. A program that is perceived as a national symbol, institutionally protected, and normatively good, such as sending the constitution to citizens, is seldom evaluated (Dahler-Larsen, 2011; Vedung, 2008). To compensate for the evaluative deficit, we cooperated with the Danish Parliament's administration (henceforth, the commissioner) to develop and evaluate the program.

The Two Challenges

When we first encountered the program, we identified two concerns. The first was the vaguely developed program theory. The theory suggested that when voters receive a letter from the Speaker of Parliament along with their copy of the Danish constitution, they learn about politics, become aware of the social norm of political participation and, consequently, are more likely to vote. This expectation is reasonable, and support can be found in the extensive literature in political science on GOTV campaigns and their effects (e.g., Green & Gerber, 2008). However, we observed that the Parliament had inadvertently stacked the deck against its own program by not specifying a causal link between receiving a copy of the constitution and participating in an election or an explicit program theory of how to maximize program impact. The program included a formal, vaguely worded letter that was delivered long before most voters had the chance to cast their first votes. The American GOTV literature tells us that nonpartisan, conventional mailers similar to the material in the program package typically produce limited effects (Green et al., 2013). Moreover, voters receive these packages on their birthdays, irrespective of when next election is scheduled. This timing of the treatment likely reduces its efficiency. Effects from GOTV campaigns tend to wane, and encouraging voters up to as many as 4 years before an election is very unlikely to have an effect on turnout (Cutts, Fieldhouse, & John, 2009; Gerber, Green, & Shachar, 2003). The original program was thus unlikely to produce large turnout effects.

The second concern was the feasibility of evaluation. All newly enfranchised voters are entitled, by law, to receive a copy of the constitution. In other words, we could not construct a control group of citizens who did not receive their copy of the constitution. This limitation seemed to impede evaluation of the program's effect. Thus, an alternative approach had to be developed.

Development of the Program Theory

Our response to the first challenge was to develop a program theory in cooperation with the commissioner. First, we believed it was necessary to postpone the distribution of the packages containing the constitution and a letter until just before an election. The commissioner accepted the suggestion to postpone the distribution to young voters who turned 18 from May 2013 to Election Day in November 2013. This agreement solved two interrelated shortcomings. First, as discussed previously, sending out packages long before an election was likely to reduce its effect on turnout. Second, the postponement allowed distribution of the packages to voters who were a little older than 18.

Turnout drops dramatically among Danish voters, and young voters in other countries, within the first years of eligibility (Bhatti, Hansen, & Wass, 2012). Therefore, the ability to target voters who were a little older than 18 allowed for inclusion of voters with lower propensities to vote. In a high-turnout setting, such as the Danish elections, mobilization campaigns are likely to have most success mobilizing such low-propensity voters (Arceneaux & Nickerson, 2009). Postponing the distribution alone allowed the program to target more voters when it was more likely to affect turnout. In addition, the voters most likely affected were those for whom we changed the timing. This step in itself increased the likelihood that the program promoted participation.

Adjustment of the Treatment Content

The next step in response to the first challenge was to adjust the content of the letter in accordance with the revisions to the program theory developed in dialogue with the commissioner. The distributed version of the constitution was a commented version with illustrations, which was not subject to change. However, the accompanying letter could be varied. The header of the letter was traditionally “Congratulations” and the signer was the Speaker of Parliament. The letter contained four paragraphs. The first stated that the voter now had acquired the right to vote in elections and thereby gain influence in politics. The second framed the need for citizens to support democracy by voting and engaging in politics. The third was a short description of the Constitution’s status and content. The final paragraph explained the motivation for distributing the constitution to all recently eligible voters.

The last three paragraphs were unchanged. The second paragraph corresponded to an argument about civic duty. Political scientists have long pointed to civic duty as a chief motivation for voting (Blais, Young, & Lapp, 2000; Riker & Ordeshook, 1968). Furthermore, GOTV campaigns found that highlighting the civic duty of voting can increase participation (Gerber & Green, 2000; Gerber, Green, & Larimer, 2008). In the first section, a small change was made, reminding the voter that she was eligible for the upcoming election rather than merely reminding her of a general eligibility to vote. Therefore, the revised conventional letter resembled the original letter.

With the emphasis on civic duty and reminder about the forthcoming election, it seemed likely that the letter itself would actually have affect turnout when mailed closer to the election. However, as noted previously, nonpartisan, conventional letters typically produce small effects. Research suggests that stronger effects are produced by unconventional letters (Green et al., 2013). Thus, an acceptable unconventional treatment might increase turnout more than the revised letter. Unconventional letters are a broad category, but most are framed as social pressure reminding voters of their own (or perhaps their neighbors’) past participation or abstention and promise that updated information will be provided after the upcoming election (Gerber et al., 2008, Panagopoulos, 2010).

Because the social pressure argument was not allowable by law, we urged the commissioner to think creatively and develop a nontraditional approach. They suggested an innovative, unconventional letter relying on humor. Advertisers use humor routinely, and public information programs have tried this tactic with promising results. For instance, a field experiment on melanoma prevention showed that a humorous leaflet increased knowledge compared to a control group and had a higher reading rate than a conventional information leaflet, though it did not lead to a stronger increase in knowledge than the conventional leaflet did (Richard et al., 1999). Furthermore, several studies of political comedy suggest there is a positive relationship between exposure to satire and political knowledge and further information acquisition, as well as a positive impact of watching political comedy on political self-confidence (Brewer, Young, & Morreale, 2013; Cao, 2010; Xenos & Becker, 2009; Young & Hoffman, 2012). Because we targeted young voters and had a special

interest in voters who might not read a conventional letter, it seemed credible to rely on humorous communication to engage and retain voters. The commissioner developed a humorous cartoon flowchart. In the flowchart, the voters had to answer one question at a time and with near certainty, they would end up in a scenario where the reasonable choice was for them to vote. Figure 1 (see p. 5) depicts the two treatments. They are also available from the authors in higher resolution.

With an appropriate program theory change, the commissioner changed the timing of the treatment, made changes to the traditional letter, and developed a new humorous cartoon to maximize the effects of the program.

Development of a Feasible Design

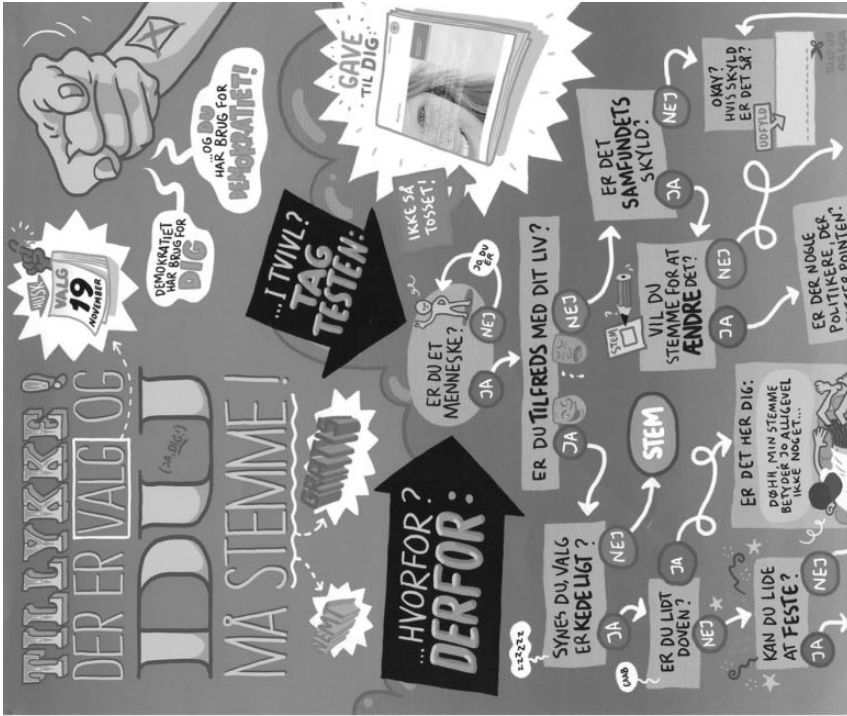
Our response to the second challenge (the legal requirement that all voters receive a copy of the constitution) was to utilize a variation of a randomized waitlist field experiment. In a randomized waitlist experiment, all individuals eventually receive the same treatment, but the timing is varied (Gerber & Green, 2012, pp. 276–281). We did not administer the same treatment to all voters, however. Instead, we randomly divided them into three groups. Two groups received the constitution along with either the modified conventional letter or the humorous letter. Both packages were dispatched five days before the election. The third group served as a control group and received the constitution along with the usual letter after the election. As treatment assignment was random, comparing outcomes among the three groups provides unbiased estimates of the overall treatment effects and the differences between the two treatments. This setup overcame the legal hurdle that everyone must receive the constitution, without damaging the prospects for causal inference. However, this design did create another tension because we excluded voters from a treatment during the time this treatment was expected to be most beneficial for their propensity to vote. Even so, the commissioner accepted this exclusion. In the discussion, we briefly discuss the implications of delaying treatment.

We needed to know the ages and addresses of all voters to select the sample and distribute the treatment according to the design. All Danes have a unique personal number that identifies them in the Civil Registration System (CRS; Pedersen, 2011). After the election, the treatment information was merged with validated turnout, which we collected in collaboration with all 98 Danish municipalities, and a very detailed set of sociodemographic variables from Statistics Denmark. These variables allowed us to assess the heterogeneous effects, which we address subsequently.

Our sample drawn from the CRS contained all young citizens between 18 and 18½ years on Election Day (33,520 individuals). Of these individuals, we excluded 5,657 for use in two other experiments, 413 individuals who lived in households with more than 1 individual in the experiment to avoid contamination through intra-household spillover effects, and 306 individuals with no voting records. This produced a sample of 27,144 relevant voters. Of these, 8,887 were randomly assigned to receive the revised letter and constitution (the traditional package), 8,920 received the cartoon and constitution (the vivid package), and 9,337 were assigned to the control group (received the traditional package after the election).

Empirical Analysis

Based on the program theory, we expected a positive effect of the treatments on turnout. Furthermore, we expected the humorous letter (the vivid package) to have a larger effect than the revised letter (the traditional package). Because individuals were randomly assigned to the two treatment groups and control group, we can straightforwardly evaluate the program's effect on turnout by comparing the turnout rates of the treatment groups to the rate of the control group, the latter of which did not receive a treatment until after the election.



FOLKETINGET



Folketingets formand
Christiansborg
DK-1240 København K
Tlf. +45 33 37 55 00
Faks +45 33 32 85 36
www.ft.dk
ft@ft.dk

Tillykke

Du har fået valgt, og du kan allerede stemme ved kommunal- og regionsvalget på tirsdag den 19. november. Ved at stemme får du indflydelse på, hvem der bestemmer i Danmark, og hvad det er for en politik, der skal føres. Er du dansk statsborger, kan du fremover også stemme til bl.a. folketingsvalg og folkeafstemninger.

For at være om det danske folkestyre er det vigtigt, at vi som borgere gør brug af muligheden for at stemme og af muligheden for selv at blive aktive politikere. Det var i 1849 – for mindre end 200 år siden – at Danmark afskaffede enevælden, hvor kongen bestemte, og fik den første grundlov. Grundloven er fundamentet for vores demokrati – den er grundlaget for det danske folkestyre.

Grundloven fastsætter først og fremmest de grundlæggende rammer for, hvordan landet skal styres. Det gælder f.eks. princippet om magtens tredeling i den lovgivende, den udøvende og den dømmende magt. Grundloven indeholder også det værbæret, som er bærende for det danske folkestyre, nemlig frihedsrettighederne.

Grundloven tilhører os alle sammen, og den er vigtig for os. Derfor har Folketinget besluttet, at alle, der fylder 18 år eller bliver dansk statsborger, skal modtage grundloven med forklaringer. Jeg håber, at du vil læse grundloven og bruge den som fundament for din opfattelse i den demokratiske debat.

Med venlig hilsen

Nogens Lykkehoff
Folketingets formand

Figure 1. Applied traditional and vivid treatments (partial).

Table 1. Turnout Rates Across Experimental Groups.

	Turnout Percentage	Increase in Turnout Compared to Control	<i>n</i>
Control (received package after election)	72.85		9,337
Combined (received a package)	73.92	1.07* (0.57)	17,807
Traditional package	73.43	0.58 (0.66)	8,887
Vivid package	74.41	1.56* (0.65)	8,920

Note. The differences from the control group are tested by a logit regression with individual participation as the dependent variable and treatment as an independent variable with the control group as the reference. Standard errors from average marginal effects are in parentheses.

* $p < .05$ (one-sided test). The difference between the two treatments is not statistically significant $p = .070$ (one-sided test).

The turnout rate for those who received a package was 73.9%, which represents a statistically significant 1.1% increase over the turnout rate of the control group (see table 1). This result is noteworthy given the high baseline (control group) turnout rate. The turnout rates for the packages were also higher than the control group, but the difference was only statistically significant for the vivid package (effect estimate = 1.6 percentage points). There is a noticeable 1.0% higher turnout rate among the recipients of the vivid package compared to the traditional package, which is consistent with the expectations. However, the difference is not statistically significant, $p = .07$ (one tailed). In another analysis (available from the authors upon request), we perform the analysis including pretreatment covariates that might lead to more precisely estimated effects (Gerber & Green, 2012, p. 121). However, this analysis does not change any of the conclusions.

Subgroup Effects

The commissioner was greatly interested in whether the program decreased or increased turnout inequalities among young voters. A strong normative ideal of political equality among the main political actors in Denmark exists. Therefore, it was considered normatively preferable that the program help reduce inequalities. To investigate whether we achieved this result, we followed a three-step procedure (see Enos, Fowler & Vavreck, 2014, for a similar approach).

First, in our control group, we regressed whether the person voted on a range of variables that we know predict participation in the Danish context (Bhatti & Hansen, 2013).¹ We restricted model fitting to the control group because we expected that the covariates conditioned the treatment effect. Therefore, including the treatment groups in the first step might bias the predictors. In the next step, we used the estimated regression coefficients to predict turnout across groups. We called this prediction “the propensity to turnout” for the individual. The final step was to regress individual turnout on the propensity to turnout interacted with the treatment to determine whether the effect of the treatments varied by participation propensity. Figure 2 illustrates the conditional average treatment effect (CATE) by turnout propensity. At the bottom, the figure includes a rug plot for each percentile of the distributions of the propensity to participate, including the maximum and the minimum values.

The figure displays a substantial effect for those with low propensities to vote and low effects for high-propensity voters. The CATE peaks at a propensity to vote of approximately 0.30. At this level, the CATE is well above 5 percentage points for both the vivid and the traditional packages. We can also see that the CATE is statistically significant for citizens with a propensity to turnout up to approximately 0.65. When the propensity to participate exceeds 0.7, the differences are statistically

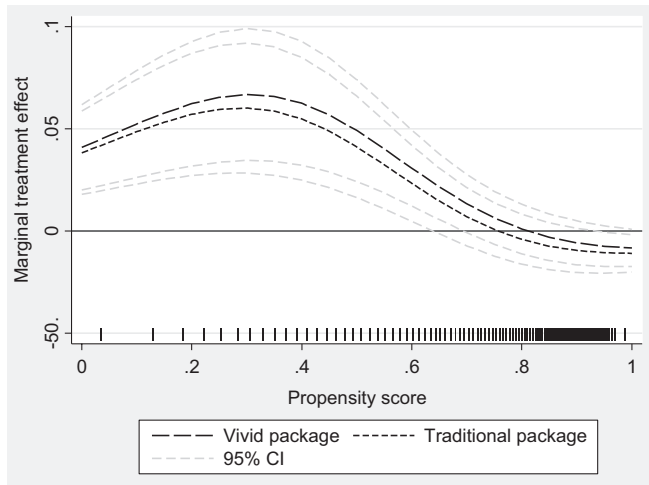


Figure 2. Turnout and propensity to turnout across experimental groups.

insignificant. This analysis supports the commissioner's hope of reducing the gap in turnout rates across subgroups through the program because those who were least likely to participate caught up somewhat to those who were most likely to participate. In a robustness analysis (available upon request), we restricted the sample to the 5th, 10th, 25th, and 50th percentiles to ascertain that high-propensity voters did not drive the results of the heterogeneity analysis. The substantial conclusions are robust to these restrictions.

Discussion

Democratic institutions often do not evaluate programs that are perceived as normatively good, for example, those aiming to increase turnout. This was true of the Danish Parliament first time voter packages, which had been in effect since 2005. We cooperated with the commissioner to develop a more explicit program theory to improve the program. Furthermore, we implemented a design that respected legal impediments to evaluation. We utilized a variant of a randomized waitlist field experiment, where the control group received a treatment after the point of measurement. The design thereby allowed testing the program's effect on voter turnout with strong internal and external validity, while ensuring that everybody received the most substantial element of the intervention (Boruch, 2005; Henry, Smith, Kershaw, & Zulli, 2013).

On average, the interventions increased turnout approximately 1.1 percentage point. With 18,075 persons treated, an estimated 199 extra voters were mobilized by the campaign. A reasonable estimate of the price of each mailing is \$1.50 and, consequentially, the price per extra vote was approximately \$136. Whether this is an acceptable price per vote to continue the program is up to the commissioner to decide. In general, putting a price on the value of democracy, which, among other things, depends on citizen participation, is a difficult task. However, note that the calculated number of extra votes represents only the direct effect. Substantial spillover effects from mobilization campaigns have been previously observed (e.g., Arceneaux & Nickerson, 2009; Sinclair, McConnell, & Green, 2012). The evidence suggests that when one voter is mobilized directly by a campaign, a substantial number of voters in the receiver's social network are mobilized indirectly. Second, research

on turnout has documented a habitual element of turnout behavior. When a citizen votes in one election, she is substantially more likely to vote in future elections (Cutts et al., 2009; Gerber et al., 2003). Encouraging citizens to vote in the first election for which they are eligible might be especially important because the probability of developing a habit of voting instead of nonvoting increases (e.g., Gerber et al., 2003; Plutzer, 2002). In other words, the effects reported in the evaluation are likely the lower bound effects of the total increase in participation among voter networks and across elections.

Did the Campaign Change Election Outcomes?

For the commissioner, it was important to maintain a party neutral position. When evaluating the potential impact *after* the campaign, it seems unlikely that the treatment influenced election outcomes with 199 mobilized voters across 98 municipalities. However, such an assessment could not be made before the election. Instead, before the election, we considered whether potentially mobilized voters were expected to vote differently than those who would vote in the absence of the campaign. According to previous research, young Danes' voting preferences do not differ markedly from the older generations (Stubager, Hansen, & Andersen, 2013). However, this research makes no claims about abstainers, which did not allow us to entirely rule out that mobilized voters behave differently. We estimated the number of voters we could mobilize. As we describe previously, existing research on nonpartisan mailings have found small mobilization effects. Gerber, Green, and Larimer (2008) find an effect of 8% with a uniquely powerful treatment. We found it extremely unlikely to produce a similar effect considering our comparatively high baseline turnout and weaker treatment. However, had we caused an 8% increase in participation among the treated, we would have mobilized approximately 1,400 voters, which would still be unlikely to change the outcomes of elections with over 3 million participating voters.

In sum, the finding that young voters vote similarly to the older generations and the limited expected effect left us assured that the campaign would not influence election results. In general, such considerations are important when conducting campaigns of this type, especially when representing a democratic institution mobilizing for elections.

Could We Withhold Treatment From Voters?

One reason public agencies are reluctant to exempt a random part of the population from an experiment is the concern that they will exclude citizens from a potentially beneficial treatment (Cotterill & Richardson, 2010). A central principle of public administration is the equal treatment of citizens and, at first, one could argue that a control group breaks with this principle. However, publicly funded or administered programs often exempt some citizens from a policy initiative, while others participate in the initiative. This selection is often necessary due to priorities and budget constraints (Banerjee & Duflo, 2014). One advantage of randomization is that assignment to the treatment occurs by chance rather than by some political criterion, which can be advantageous from an ethical perspective because everyone in the target population has equal probability of being targeted by the program (Gueron, 2002). This eliminates arbitrary criteria to which policy makers could otherwise resort. Furthermore, the randomization process is transparent and is arguably perceived as fair by organizations involved in field experiments as well as individuals placed in either treatment or control groups (Banerjee & Duflo, 2014, p. 101). Finally, the control group was not deprived of any rights or entitlements. They could vote just like the treatment groups could, and after the election, they received their copies of the constitution as required. In conclusion, the design of the study did not alleviate all potential concerns, but it ensured that the differences in treatments were determined by randomization and that the main difference between citizens was that a minority received the treatment at a potentially less beneficial time.

The Impact of the Evaluation

This field experiment has had three substantial consequences for the commissioner's practice of mailing copies of the constitution. First, the commissioner has stopped sending the constitution to citizens on their 18th birthday. Instead, they group young citizens and mail copies of the constitution with an encouragement to vote in the next local, national, or European elections prior to the elections. The commissioner will follow the timing employed in the field experiment going forward. In that way, the evaluation likely influenced the selection of a course of action (Henry, 2003). Second, the commissioner will apply variants of the vivid treatment in the upcoming election. Third, they will implement continuous evaluations of the program similar to the evaluation we presented here. Thereby this evaluation has helped establish evaluative thinking in the organization, where evaluations through field experiments will play a larger role in the years ahead.

Examining the existing practices in different policy areas, experiments are often performed as onetime events, and the results are then used (or not) to implement new programs (Banerjee & Duflo, 2014). However, continuous rigorous testing of programs is indeed important for at least two reasons. First, the effect of this experiment might be contextual: The effects might vary for other elections, years, weekdays, or contexts (Gerber, Green, & Kaplan, 2014). Second, implementing experiments as a part of an organization's program development practice can test different treatments. Experimental results, including the ones presented here, often fuel new questions and ideas that can and should be tested. What was it about the flow-chart treatment that mobilized young voters? Was it the cartoonish design, the quiz element, or the humorous content? Could other, more effective treatments be developed? To answer such questions, experiments must be part of ongoing program development, and for some organizations, it might be fruitful to establish long-term relationships with researchers who can provide theoretical and methodological insights. Perhaps the most successful aspect of this experiment was that it paved the way for continued evaluative thinking in the organization.

Appendix

Table A1. Logistic Regression for the Main Model, Model Controlling for Pretreatment Propensity to Participate, and Model With Treatment and Propensity Interactions.

	Model A	Model B	Model C
Traditional	0.03 (0.03)	0.05 (0.04)	0.49* (0.13)
Vivid	0.08* (0.03)	0.09* (0.04)	0.51* (0.13)
Propensity to participate	—	4.77* (0.07)	5.20* (0.13)
Traditional × Propensity to Participate	—	—	−0.63* (0.18)
Vivid × Propensity to Participate	—	—	−0.64* (0.18)
Constant	0.99* (0.02)	−2.32* (0.06)	−2.61* (0.09)
N	27,144	27,120	27,120
Pseudo R ²	.00	.17	.17
Log-likelihood	−15,677.90	−13,062.83	−13,054.43

Note. Coefficients are from a logistic regression of individual participation on the independent variables. Standard errors are given within parentheses. Additional descriptive statistics and robustness tests are available from the authors.

* $p < .05$ in one-tailed test.

Acknowledgments

An earlier version was presented at the Nordic Political Science Association (NOPSA) Conference 2014 in Gothenburg, Sweden. We thank the workshop participants for their comments. We have received valuable comments from Donald P. Green, Peter Dahler-Larsen, and numerous other colleagues. Finally, we are grateful for excellent research assistance from Mariann Malchau Olsen and Ane Reese Mikkelsen.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: The project is funded by the Danish Council for Independent Research (grant no. 12-124983) and the Danish Parliament.

Note

1. Specifically, we included gender, age in days, ongoing education, completed education, income, ethnicity, citizenship, residential stability, socioeconomic status, distance to the polling station, cohabitation with parents, parental turnout in the previous election interactions between cohabitation and parental turnout, parents' age, parents' education, parents' income, and municipality fixed effects in our model. We estimated a logit regression with robust standard errors. The logit regression has $N = 9,330$, log likelihood = $-4,387.9$, McFadden's $R^2 = .20$.

References

- Arceneaux, K., & Nickerson, D. W. (2009). Who is mobilized to vote? A re-analysis of 11 field experiments. *American Journal of Political Science*, 53, 1–16.
- Banerjee, A. V., & Duflo, E. (2014). The experimental approach to development economics. In D. L. Teele (Ed.), *Field experiments and their critics: Essays on the uses and abuses of experimentation in the social sciences* (pp. 78–114). New Haven, CT: Yale University Press.
- Bhatti, Y., Dahlgaard, J. O., Hansen, J. H., & Hansen, K. M. (2014). *Hvem stemte og hvem blev hjemme? Valgdeltagelsen ved kommunalvalget 19. November 2013. Beskrivende analyser af valgdeltagelsen baseret på registerdata* (CVAP Working paper, No. 2, Vol. 2014). Copenhagen, Denmark: Department of Political Science, University of Copenhagen.
- Bhatti, Y., & Hansen, K. M. (2010). *Valgdeltagelsen ved kommunalvalget 17. November 2009. Beskrivende analyser af valgdeltagelsen baseret på registerdata* (CVAP Working paper, No. 3, Vol. 2010). Copenhagen, Denmark: Department of Political Science, University of Copenhagen.
- Bhatti, Y., & Hansen, K. M. (2012). Leaving the nest and the social act of voting: Turnout among first-time voters. *Journal of Elections, Public Opinion & Parties*, 22, 380–406.
- Bhatti, Y., & Hansen, K. M. (2013). Nydanskernes valgdeltagelse: Effekten af familiestruktur og ghettoer. In J. Elklit & U. Kjær (Eds.), *KV09: Analyser af kommunalvalget 2009* (pp. 81–102). Odense, Denmark: Syddansk Universitetsforlag.
- Bhatti, Y., Hansen, K. M., & Wass, H. (2012). The relationship between age and turnout: A roller-coaster ride. *Electoral Studies*, 31, 588–593.
- Blais, A., Young, R., & Lapp, M. (2000). The calculus of voting: An empirical test. *European Journal of Political Research*, 37, 181–201.
- Boruch, R. (2005). Preface: Better evaluation for evidence-based policy: Place randomized trials in education, criminology, welfare, and health. *The Annals of the American Academy of Political and Social Science*, 599, 6–18.

- Brewer, P. R., Young, D. G., & Morreale, M. (2013). The impact of real news about “fake news”: Intertextual processes and political satire. *International Journal of Public Opinion Research, 25*, 323–343.
- Cao, X. (2010). Hearing it from Jon Stewart: The impact of *The Daily Show* on public attentiveness to politics. *International Journal of Public Opinion Research, 22*, 22–46.
- Cotterill, S., & Richardson, L. (2010). Expanding the use of experiments on civic behavior: Experiments with local government as a research partner. *The ANNALS of the American Academy of Political and Social Science, 628*, 148–164.
- Cutts, D., Fieldhouse, E., & John, P. (2009). Is voting habit forming? The longitudinal impact of a GOTV campaign in the UK. *Journal of Elections, Public Opinion and Parties, 19*, 251–263.
- Dahler-Larsen, P. (2011). *The evaluation society*. Stanford, CA: Stanford University Press.
- Dinas, E. (2012). The formation of voting habits. *Journal of Elections, Public Opinion & Parties, 22*, 431–456.
- Enos, R. D., Fowler, A., & Vavreck, L. (2014). Increasing inequality: The effect of GOTV mobilization on the composition of the electorate. *Journal of Politics, 76*, 273–288.
- Gerber, A. S., & Green, D. P. (2000). The effects of canvassing, telephone calls, and direct mail on voter turnout: A field experiment. *American Political Science Review, 94*, 653–663.
- Gerber, A. S., & Green, D. P. (2012). *Field experiments: Design, analysis, and interpretation*. New York, NY: W. W. Norton.
- Gerber, A. S., Green, D. P., & Kaplan, E. H. (2014). The illusion of learning from observational research. In D. L. Teele (Ed.), *Field experiments and their critics: Essays on the uses and abuses of experimentation in the social sciences* (pp. 9–32). New Haven, CT: Yale University Press.
- Gerber, A. S., Green, D. P., & Larimer, C. (2008). Social pressure and voter turnout: Evidence from a large-scale field experiment. *American Political Science Review, 102*, 33–48.
- Gerber, A. S., Green, D. P., & Shachar, R. (2003). Voting may be habit-forming: Evidence from a randomized field experiment. *American Journal of Political Science, 47*, 540–550.
- Green, D. P., & Gerber, A. S. (2008). *Get out the vote: How to increase voter turnout*. Washington, DC: Brookings Institution Press.
- Green, D. P., McGrath, M. C., & Aronow, P. M. (2013). Field experiments and the study of voter turnout. *Journal of Elections, Public Opinion & Parties, 23*, 27–48.
- Gueron, J. M. (2002). The politics of random assignment: Implementing studies and affecting policy. In R. Boruch & F. Mosteller (Eds.), *Evidence matters: Randomized trials in education research* (pp. 15–49). Washington, DC: The Brookings Institution.
- Henry, G. T. (2003). Influential evaluations. *American Journal of Evaluation, 24*, 515–524.
- Henry, G. T., Smith, A. A., Kershaw, D. C., & Zulli, R. A. (2013). Formative evaluation: Estimating preliminary outcomes and testing rival explanations. *American Journal of Evaluation, 34*, 465–485.
- International IDEA. (2015). *Voter turnout database*. Stockholm: The International Institute for Democracy and Electoral Assistance (IDEA).
- Panagopoulos, C. (2010). Affect, social pressure and prosocial motivation: Field experimental evidence of the mobilizing effects of pride, shame and publicizing voting behavior. *Political Behavior, 32*, 369–386.
- Pedersen, C. B. (2011). The Danish civil registration system. *Scandinavian Journal of Public Health, 39*, 22–25.
- Plutzer, E. (2002). Becoming a habitual voter: Inertia, resources, and growth in young adulthood. *American Political Science Review, 96*, 41–56.
- Richard, M. A., Martin, S., Gouvernet, J., Folchetti, G., Bonerandi, J. J., & Grob, J. J. (1999). Humour and alarmism in melanoma prevention: A randomized controlled study of three types of information leaflet. *British Journal of Dermatology, 140*, 909–914.
- Riker, W. H., & Ordeshook, P. C. (1968). A theory of the calculus of voting. *American Political Science Review, 62*, 25–42.

- Sinclair, B., McConnell, M., & Green, D. P. (2012). Detecting spillover effects: Design and analysis of multi-level experiments. *American Journal of Political Science*, 56, 1055–1069.
- Stubager, R., Hansen, K. M., & Andersen, G. J. (Eds.). (2013). *Krisevalg—Økonomien og folketingsvalget 2011*. København, Barque: DJØF-Forlag.
- The Government & Danish People's Party. (2004). *Aftaler om Finansloven for 2005*. Finansministeriet Copenhagen, Denmark: Schultz Information.
- Vedung, E. (2008). *Policy and program evaluation*. Piscataway, NJ: Transaction Publishers.
- Wolfinger, R. E., & Rosenstone, S. J. (1980). *Who votes*. New Haven, CT: Yale University Press.
- Xenos, M. A., & Becker, A. B. (2009). Moments of Zen: Effects of *The Daily Show* on information seeking and political learning. *Political Communication*, 26, 317–332.
- Young, D. G., & Hoffman, L. (2012). Acquisition of current-events knowledge from political satire programming: An experimental approach. *Atlantic Journal of Communication*, 20, 290–304.