

Research Article

Politicians as Persuaders: A Field Experiment on Messenger Effects and Social Program Uptake

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Abstract: Accessing public sector programs often involves onerous administrative processes for citizens, which can demotivate program applicants and discourage uptake. In this study, I worked with the Philadelphia Housing Development Corporation (PHDC) to test an intervention to boost uptake of PHDC’s Basic System Repair Program (BSRP) by using local political leaders to persuade citizens to complete the program’s onboarding process. Specifically, I randomly assigned 2,575 BSRP applicants to either receive a motivational mailer from PHDC directly, or from the office of that citizen’s representative on the Philadelphia City Council. While I find that the average response rate for individuals who received councilmember mailers was roughly 2 percentage points higher than for the PHDC mailers, this result is largely driven by the effectiveness of letters sent by one particular councilmember’s office. Furthermore, I find little evidence to suggest that councilmembers are more effective messengers for constituents who share their race or gender, relative to others in their districts. These findings suggest that “messenger effects” can vary significantly case-by-case, and underscore the importance of analyzing heterogeneity when evaluating behavioral interventions.

Keywords: Messenger effects, program uptake, nudges, public policy, behavioral science

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Many public sector programs involve onerous administrative processes for citizens seeking access to program goods and services. In many cases, applicants begin the process required for program enrollment but do not complete it because of the complexity of the onboarding process, delays leading to uncertainty about eligibility, or a lack of motivation (Bertrand, et al., 2006). These factors can even deter the initiation of applications; a 2004 review on welfare benefits in OECD countries concludes that “administration and information barriers are often the most important factors deterring eligible individuals from applying for welfare benefits” (Hernanz, 2004). These barriers, which are increasingly studied by public administration scholars (see recent work on “administrative burden” and program take up; e.g. Moynihan and Herd, 2018; Moynihan, et al., 2015; Fox, et al., 2020; Christensen, et al., 2020), limit the reach and efficacy of social programs and result in wasted time, effort, and money. Unfortunately, these administrative processes cannot always be streamlined easily, as they are often required for legal or other reasons. Thus, it is imperative to find cost-effective behavioral change tools to persuade citizens to complete complex administrative processes.

A growing branch of research in the social sciences focuses on “messenger effects,” which refers to the role that the source of information plays in influencing behavior change in response to the information (Dolan et al., 2012). The messenger effect has been investigated in a broad range of contexts, including responsiveness to messages highlighting the health risks of cigarettes (Maclean et al., 2019) and political attitudes (Maier et al., 2017). The messenger effect is a useful construct when tackling low take-up of public sector welfare programs, which are often administered by bureaucratic organizations that may not be well

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known to the potential beneficiary. In this context, it is plausible that messaging from the bureaucratic institution that administers the program might not be effective, and a specific, better known (or more trusted) messenger might motivate behavior change more effectively.

In this article, I report on a field experiment that tests several hypotheses related to using messenger effects to encourage citizens to overcome administrative burdens. First, I test whether messaging is more effective when the messenger is a specific local political figure (in this case, the message recipient's city councilmember) rather than a "faceless" bureaucratic entity. There is growing evidence supporting this hypothesis, consistent with a messenger effect linked to authority and prominence (Martin and Marks, 2019; Durantini et al., 2006; Karlan and List, 2020; Wilson and Sherrell, 1993) or trustworthiness and credibility (Pornpitakpan, 2004; McGinnies and Ward, 1980; Diamant, et al., 2022). Indeed, recent public opinion work suggests that citizens tend to be more trusting of their local officials and think of them as more competent than most other authorities they interact with (Pew Research, 2019; Hart, 2019), making them potentially effective messengers. Notably, even if participants do not recognize the name of their city councilmember in messaging, it is reasonable to posit that citizens are aware of the city council and therefore may respond more to messaging from that source than a bureaucratic agency unknown to them.

Second, I test the extent to which gender or racial concordance between the messenger and the message recipient might influence the efficacy of the messenger. This builds on a strand of literature that explores group identities and their potential effects on behavioral response in a variety of contexts. For example, recent work shows that health-related behaviors vary based on race/gender concordance between patients and doctors (Durantini et al., 2006; Alsan et al., 2019; Greenwood, et al., 2018) and educational outcomes vary based on race/gender concordance between instructor and student (Dee, 2004; Dee, 2005; Lusher et al., 2018). Importantly, much of the past work looking at race/gender concordance and behavior change focuses on situations where close personal interaction is inherent (doctor-patient, teacher-student). I extend this to the context of behavioral nudges using mailed letters, which are more about persuasion "from afar." In doing so, I draw on public administration research arguing that the race/gender of bureaucrats matters because this "symbolic representation" might improve outcomes by directly impacting citizen behaviors, even in "light-touch" messaging contexts (Ricucci and Ryzin, 2017; Ricucci, et al., 2016; Van Ryzin, et al., 2017; Sievert, 2021; Webeck and Lee, 2022).

I test these hypotheses using a field experiment with 2,575 citizens in Philadelphia, in the context of the Basic System Repair Program (BSRP), an initiative of the Philadelphia Housing Development Corporation (PHDC). The BSRP provides free home repairs for low-income residents and homeowners. However, the program has been hampered by difficulties in getting residents to proceed through the administrative processes of the BSRP, with many eligible individuals being unresponsive to PHDC's efforts to contact them. The experiment targeted persistently non-responsive individuals eligible for the program, and tested the impact of leveraging local political leaders as motivating influences on citizens. In collaboration with PHDC, I developed a simplified fourth mailer to send to those individuals who had not responded to the previous three letters. I then worked with PHDC and members of the Philadelphia City Council to randomize whether a given mailer recipient received this fourth letter from PHDC (the "PHDC Letter" condition) or from the office of the recipient's councilmember (the "Councilmember Letter" condition). With support from PHDC, I then tracked how many recipients responded to set up an appointment by condition.

There are three main findings from the experiment. First, I find that subjects receiving the Councilmember Letter had a roughly 2 percentage point higher response rate than those receiving the PHDC Letter. Second (and crucially), I find that most of this aggregate effect is driven by letters sent by one particular councilmember's office, which suggests that messenger effects were highly variable in this context. Indeed, most councilmembers had messenger effects that are statistically indistinguishable from zero. Third, I find no evidence of differential treatment effects of the Councilmember Letter by gender or racial concordance between the councilmember and the letter recipient. This is in contrast to some prior work, and suggests that behavioral change in response to light-touch nudges may not be strongly influenced by identity considerations.

Taken together, these findings support the idea that using local public leaders as messengers may have large potential for specific, individual messengers, but is likely to be ineffective for most messengers. Further

research is needed to better understand how and why certain local public officials are so persuasive, so they can be leveraged as messengers to change behavior for the good when in a position to do so. More broadly, this work speaks to the importance of a “heterogeneity revolution” (Bryan, et al., 2021) in the behavioral sciences, both for empirical methods and theory formation.

Experimental Design

Implementing Partners

This intervention was developed through a partnership with the Philadelphia Housing Development Corporation (PHDC), which was facilitated by the Philadelphia Mayor’s Office (through GovLabPHL). The project is part of the Philadelphia Behavioral Science Initiative, a broader effort to integrate behavioral science into public policy through collaborations between academic researchers and city policymakers.

Background

The experiment focused on uptake of the Basic Systems Repair Program in Philadelphia, which provides free home repairs for low-income residents and homeowners. The financial impact of the BSRP program for beneficiaries is very large; BSRP gives up to \$18,000 in grant dollars to pay for repairs to qualifying electrical, plumbing, heating, structural, and roofing emergencies (including collapsed roofs and outdated electrical wiring). In Philadelphia, a city with an abundance of row homes, these types of emergencies often impact multiple housing units. In light of this, deferred maintenance of these homes in emergency situations can be catastrophic to communities. It is therefore a priority to make sure that eligible residents proceed through the administrative processes of the BSRP quickly, to facilitate efficient repairs.

One persistent challenge with the BSRP program is a lack of funding, which created a long BSRP waitlist as of 2016. However, in late 2016 the Philadelphia City Council voted to make roughly \$60 million in bond funds available to support PHDC operations—and in particular, to reduce the waitlist for BSRP by funding repairs (PHDC, 2017). For this increased funding to have a meaningful impact, however, it was critical that waitlisted individuals were responsive to city efforts to initiate the administrative processes needed to start repairs. When someone reaches the front of the queue for BSRP services, they are prompted by mail to confirm interest in BSRP support and set up an initial, required appointment with a PHDC staff member. However, many eligible individuals do not respond to this messaging, which deprives them of the program benefits, wastes administrative resources, and causes further delays for those on the waitlist. Furthermore, to receive the aforementioned benefits, residents need to provide proof of income for all members in the household, plus any documents required to prove ownership. Of course, it is possible that non-response happens for reasons unrelated to administrative burden; they may have resolved the repair issue themselves, they may have moved, the address on file may have been incorrect, etc. However, these alternate explanations should be balanced across treatment groups in this sample, and therefore should not impact any inferences about treatment effects.

Subjects, Design, and Outcomes

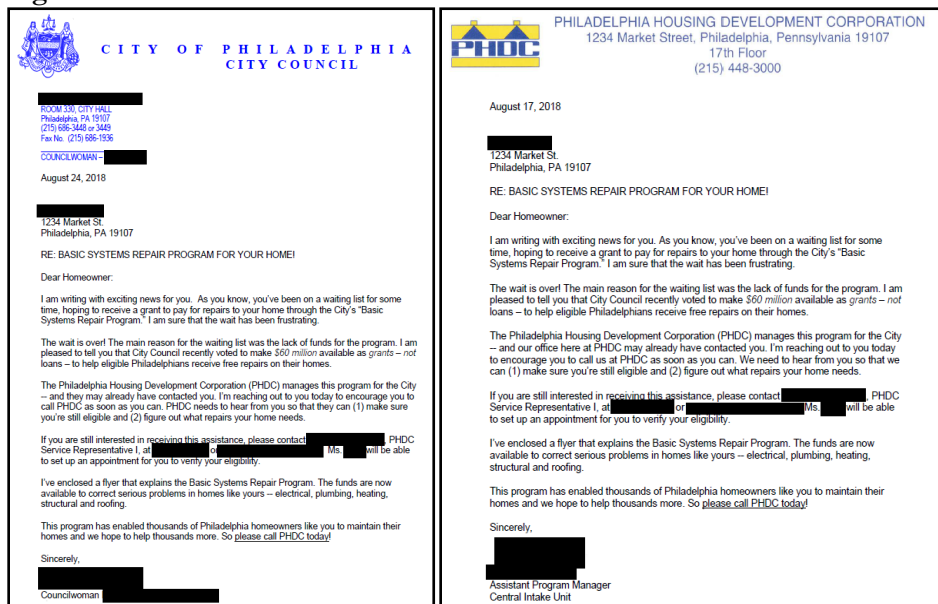
Prior to the intervention described here, PHDC had sent three mail requests to make an initial appointment to ~8,000 people eligible for BSRP services. Roughly 5,000 responded, leaving 2,870 individuals who were persistently unresponsive to messaging (unfortunately, the city did not track response rates by letter to the first three letters). These 2,870 individuals comprise this study’s (initial) sample. Note that individuals who previously sought services but were non-responsive to outreach were identified by PHDC leadership as a group of particular policy interest. Their previous interest in the BSRP indicates the opportunity for significant program impact, while their subsequent non-responsiveness suggests they may be struggling with the onerous administrative process or behavioral forces (like procrastination or inertia), creating an opportunity for a behavioral intervention to address the challenge.

The 2,870 individuals in the sample were then randomly assigned to receive either the PHDC Letter or the Councilmember Letter, with randomization stratified by councilmember. The final sample in the experiment comprises 2,575 individuals, however. This total is somewhat smaller than the initial sample pulled for randomization because administrative limitations necessitated a staggered rollout, which resulted in

some participants responding to PHDC after assignment but before mailers could be distributed. These individuals were removed from the sample. Importantly, there is no reason to suspect differential attrition, as attrition was pre-treatment and unrelated to treatment assignment. Note that this resulted in a slight difference in the numbers of people ultimately receiving the PHDC versus the Councilmember Letter.

The text content used in the PHDC and Councilmember Letters was identical. The only difference between the letters was who sent them - the recipient's councilmember or PHDC. Of course, the letterheads/envelopes' return addresses were therefore different by condition as well. Examples of the PHDC and Councilmember Letters are provided in Figure 1. It is important to note that the treatment letter does not rely exclusively on name recognition, but also emphasizes the councilmember's title and the institution of the Philadelphia City Council itself. This fact deemphasizes the importance of direct name recognition for successful treatment (though it does complicate inference regarding what specific element may cause any observed treatment effects).

Figure 1. Councilmember and PHDC Letters



To facilitate analysis of the experiment's impact, PHDC tracked and shared data in response to the letters. The primary outcome measure available was whether or not an individual contacted PHDC after receiving their letter to set up an appointment. The response data were de-identified and shared with the researcher in a way that facilitated linkage to treatment assignment.

Summary Statistics and Balance Check

A limited number of demographic variables were available for the sample and incorporated into the analysis; Table 1 provides a summary of the means for these demographic variables, by condition and overall. Notably, mean income in the sample is low (\$1,105.93/month) and disability rates are high (47.9%). This is consistent with the aims of the program, which seeks to provide urgent (and costly) home repairs to those with the least means.

To test for balance in the randomization, I regress treatment assignment on the demographic characteristics provided in Table 1. This enables me to test whether the demographics of those in the final sample predict treatment assignment, which they should not if randomization was successful. Note that this is particularly important given the modest attrition prior to the sending of letters. An F-test for the joint significance of the coefficients for the demographic characteristics in this regression suggests that randomization was successful, with no evidence of imbalance by treatment condition ($F=0.94$; $p=0.50$). For transparency, Table 1 also provides the p-values from tests on the equality of proportions/means for each of

the demographic variables by condition. Note that while the mean difference in age between conditions is statistically significant, none of the other differences are. While the regression approach described above is reassuring from a balance perspective, I report all experimental results with demographic controls in the next section.

Table 1: Summary Statistics

	Councilmember Letter	PHDC Letter	Full Sample	P-value of mean diff.
Age (years)	58.8	60.2	59.6	.016
Female (%)	21.8%	22.0%	21.9%	.897
Household Income (Monthly)	\$1,104.71	\$1,106.98	\$1,105.93	.917
Race: Black (%)	80.6%	80.4%	80.5%	.886
Race: White (%)	7.5%	7.1%	7.3%	.681
Race: Hispanic (%)	9.2%	9.9%	9.6%	.556
Race: Asian (%)	0.4%	0.7%	0.6%	.327
Race: Native American (%)	0.08%	0.0%	0.04%	.278
Race: Other (%)	2.1%	1.9%	2.0%	.652
Disabled (%)	46.9%	48.8%	47.9%	.325
Children in Household (%)	38.7%	35.5%	37.0%	.101
Multiple Adults in Household (%)	41.2%	42.2%	41.9%	.530
Observations	1,182	1,393	2,575	

Notes: The rightmost column reports the p-value from tests on the equality of means/proportions between the two letter conditions, for each demographic variable in the table.

Hypotheses

As discussed earlier, my hypotheses were based on the body of literature on the messenger effect and how it may be associated with prominence, authority, trust, and racial/gender concordance between messenger and recipient. Specifically, I hypothesized that the individuals receiving letters from their councilmember would have a higher response rate than those receiving the same letter from PHDC. As a secondary hypothesis, I posited that the Councilmember Letter would be more effective when the councilmember shared the race or gender of the recipient. One inherent limitation of this work is the inability to provide conclusive evidence on underlying mechanisms, a point I return to below.

Results

Overall, I find that the Councilmember Letter performed slightly better than the PHDC Letter, on average, on the key outcome variable of interest — responding to the letter by contacting PHDC to set up an onboarding appointment. Specifically, 16.6% of individuals responded to the PHDC Letter and 18.4% responded to the Councilmember Letter (Table 2, column 1). Note that the mean difference of 1.86 percentage points is not statistically significant at traditional thresholds ($p=0.22$). When demographic controls and councilmember fixed effects are added (Table 2, column 3), the effect size point estimate rises slightly and the p-value falls to 0.041. Notably, the effect size here is comparable to other similar nudge interventions. DellaVigna and Linos (2022), for example, finds that across 126 “nudge”-style trials in government, the average unweighted treatment effect was 1.4 percentage points.

Table 2: Average Treatment Effects

	(1)	(2)	(3)
Councilmember Letter	.0186 (.0151)	.0280 (.0154)	.0319* (.0156)
Constant	0.166*** (0.00997)	0.00318 (0.0779)	-0.00835 (0.0890)
Demographics	No	Yes	Yes
Councilmember FEs	No	No	Yes
R2	0.0006	0.0064	0.0134
Observations	2575	2473	2473

Notes: Demographic controls include age, race (categorical), gender, household income, disability status, and dummy variables indicating whether a household has children or multiple adults. Standard errors are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

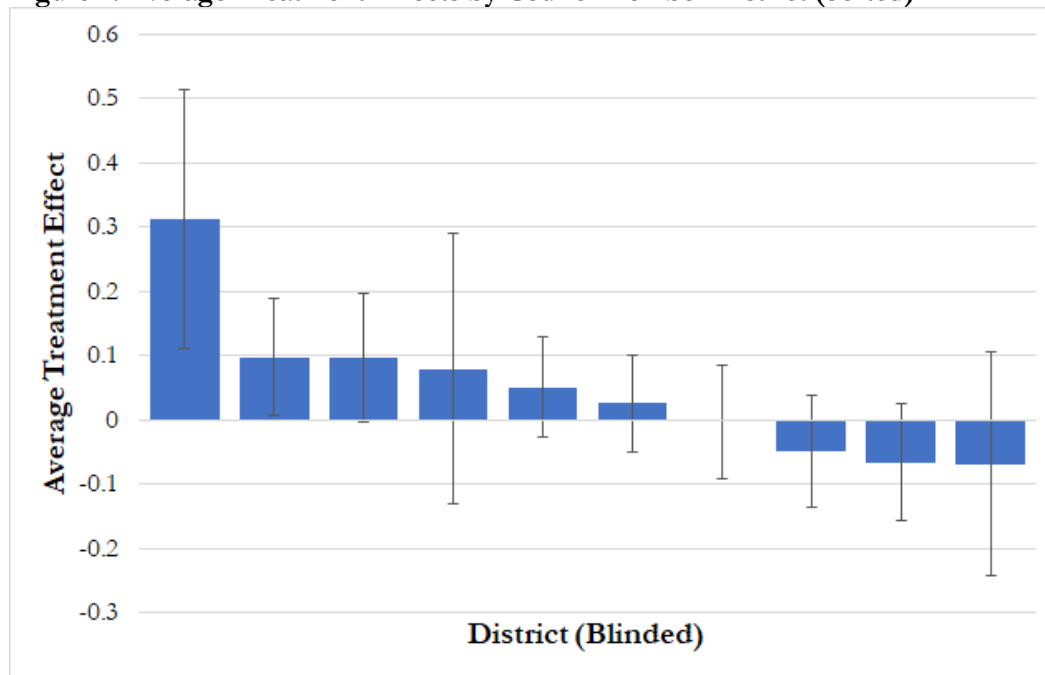
However, when evaluating the Councilmember Letter's effectiveness in more detail, a critical finding emerges. Specifically, recall that randomization was stratified by councilmember, allowing for a separate assessment of treatment effects for each councilmember (though admittedly with far smaller sample sizes for each estimate; this is more of an exploratory exercise). In Table 3, I report regressions estimating the treatment effects of the Councilmember Letter (relative to the PHDC letter) for each of the ten councilmembers separately. Figure 2 visualizes these treatment effects sorted from largest to smallest. Note that I have "blinded" both Table 3 and Figure 2 to shield the identities of specific councilmembers. It is apparent from Table 3 and Figure 2 that the aggregate efficacy of the Councilmember Letter is driven by the large and positive treatment effects of a small handful of councilmembers — with one councilmember in particular driving a great deal of the effect (Table 3, column 3; for this individual, the Councilmember Letter resulted in 47.4% response versus 17.6% response to the PHDC Letter; $p = 0.002$).

Table 3: Average Treatment Effects, by Councilmember

	Councilmember (Blinded, Numbered 1-10)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Council-member Letter	-0.0025 (0.045)	0.079 (0.099)	0.31** (0.10)	-0.049 (0.043)	-0.068 (0.087)	0.051 (0.040)	0.096 (0.051)	0.097* (0.046)	-0.066 (0.046)	0.026 (0.039)
Constant	0.21 (0.12)	0.26 (0.35)	-0.15 (0.33)	0.30 (0.30)	-0.042 (0.22)	0.012 (0.12)	-0.41** (0.16)	-0.0013 (0.10)	-0.25 (0.14)	0.28 (0.27)
Demo	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.024	0.235	0.168	0.028	0.062	0.028	0.076	0.034	0.058	0.0076
Obs	290	26	102	304	102	348	265	372	249	415

Notes: Demographic controls include age, race (categorical), gender, household income, disability status, and dummy variables indicating whether a household has children or multiple adults. Standard errors are shown in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2: Average Treatment Effects by Councilmember District (Sorted)

Notes: This figure plots disaggregated average treatment effects for each Councilmember (with 95% confidence intervals marked). Note that these regressions include controls for subjects' race, age, gender, household income, disability status, and whether or not there were children or multiple adults in the home.

These findings show that not all political messengers are alike in their ability to influence the decision-making of their constituents. The natural next question to ask is: what characteristics of politicians are associated with a greater ability to nudge behavior change? Unfortunately, because of agreements ensuring anonymity for the councilmembers in this study, I am limited in my ability to provide a detailed analysis of this question. However, I did explore correlations between vote share (in the last election) and treatment effect size ($r=0.155$) and between years in office and treatment effect size ($r=0.078$). As these correlation coefficients show, both vote share and time in office are only weakly correlated with treatment effect size. Furthermore, the vote share analysis is complicated by the fact that six of the ten councilmembers in the sample ran unopposed in the previous election (so had a vote share of 100%), and two others had vote shares over 90%. Therefore, this data set is not ideally suited to explore the question of what characteristics make a political messenger more or less effective. Further research is needed on this important question, a point I return to in the Discussion.

That said, I can investigate the possibility that councilmembers might be especially effective messengers for the subset of their constituents who share either their race or gender (note that for the latter, our data only provides a binary gender classification). To do this, I separate councilmembers into three racial categories (Black, White, and Hispanic) and two gender categories (Male and Female). I then restrict the analysis to the councilmembers in each of these five categories in turn, running three regressions per category. First, I estimate a simple average treatment effect without controls for councilmembers in each category, mirroring Table 2, column 1. Second, I run the same regression with added demographic controls and a dummy variable for whether or not a given individual in the data shared the racial/gender category of the councilmember. Third and most importantly, I then add an interaction term between the Councilmember Letter treatment and the dummy variable for whether individuals shared their councilmember's race/gender. In this regression, the interaction term allows me to assess whether or not the Councilmember Letter was more or less efficacious for the subset of constituents who shared the councilmember's gender or race (for each of the five racial/gender categories). The results of these analyses are provided in Tables 4 and 5.

Table 4: Differential Treatment Effects by Racial Concordance, by Councilmember Race

	<u>Councilmember Race</u>								
	Black Councilmember			White Councilmember			Hispanic Councilmember		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Councilmember Letter (CL)	0.0034 (0.017)	0.015 (0.017)	-0.033 (0.053)	0.097 (0.054)	0.12* (0.057)	0.074 (0.075)	0.077 (0.050)	0.093 (0.051)	0.088 (0.078)
Same Race		0.045 (0.028)	0.019 (0.042)		-0.007 (0.056)	-0.055 (0.069)		-0.053 (0.049)	-0.057 (0.061)
CL * Same Race (interaction)			0.052 (0.056)			0.11 (0.11)			0.009 (0.10)
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Councilmember Fes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
R2	0.000	0.008	0.008	0.014	0.064	0.069	0.009	0.062	0.063
Observations	2072	1978	1978	235	230	230	268	265	265

Notes: Demographic controls include age, gender, household income, disability status, and dummy variables indicating whether a household has children or multiple adults. Standard errors are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Overall, there is very little evidence to suggest that racial or gender identity similarity between a councilmember and a given constituent meaningfully changed the efficacy of the Councilmember Letter. In other words, the fact that the interaction coefficients in Tables 4 and 5 are consistently not statistically significant suggests that the Councilmember Letter is no more effective for constituents sharing a councilmember's racial or gender identity than for individuals not sharing that identity in that councilmember's District. One could plausibly argue that the positive point estimates on the interaction coefficients in Table 4 (and their non-trivial magnitudes for the black and white racial categories in particular) provide some suggestive evidence that racial concordance might increase the efficacy of the Councilmember Letter. However, given the small sample size and the resulting p-values for the interaction coefficients ($p = 0.35$, $p = 0.33$, and $p = 0.93$ for Table 4, columns 3, 6, and 9 respectively), I conclude that this is likely just statistical noise and not practically meaningful.

Table 5: Differential Treatment Effects by Gender Concordance, by Councilmember Gender

	<u>Councilmember Gender</u>					
	Male Councilmember			Female Councilmember		
	(1)	(2)	(3)	(4)	(5)	(6)
Councilmember Letter	0.0095 (0.022)	0.021 (0.022)	0.015 (0.026)	0.027 (0.021)	0.040 (0.022)	0.052 (0.045)
Same Gender		-0.026 (0.025)	-0.039 (0.034)		0.024 (0.025)	0.030 (0.032)
Councilmember Letter * Same Gender (interaction)			0.027 (0.051)			-0.015 (0.051)
Demographics	No	Yes	Yes	No	Yes	Yes
Councilmember FEs	No	Yes	Yes	No	Yes	Yes
R2	0.000	0.023	0.023	0.001	0.009	0.009
Observations	1164	1117	1117	1411	1356	1356

Notes: Demographic controls include age, race (categorical), household income, disability status, and dummy variables indicating whether a household has children or multiple adults. Standard errors are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Discussion

The results of this experiment support several broad conclusions. On first glance, there is some evidence that specific, local political messengers can matter (and potentially a great deal) in light-touch nudge interventions. Indeed, the most effective messenger in this experiment nearly tripled the response rate of their constituents relative to the PHDC letter. That said, the majority of local political leaders had little to no effect as messengers, relative to PHDC. The result here can be thought of as a null result, with a large and intriguing outlier.

This finding provokes several crucial questions for further exploration. First, there is the question of what makes a given politician an effective messenger. While I am constrained in my ability to explain why one messenger in this study was so effective, there are several possible channels through which a public official might be seen as persuasive. For example, certain political leaders might be trusted more by citizens, or be seen as more credible, competent or honest (Pornpitakpan, 2004; McGinnies and Ward, 1980; Diamant, et al., 2022). Alternatively, certain leaders might be more persuasive because they are viewed more as an authority figure to be obeyed, an explanation that might be more aligned with theories on social and political identity (Golos, et al., 2022; Bhanot and Hopkins, 2020). It is also possible that differential recognizability of councilmembers, perhaps driven by greater media or social media exposure, helps explain why certain politicians are more persuasive. Further work is needed to gather more detailed data in an experimental context to better understand if and why messages from certain leaders are more persuasive.

More generally, these results also shine a light on the need for additional work on the conceptual underpinnings of messenger effects for public programs. A few possibilities are worth considering. For example, might messengers have the potential to influence the stigma associated with participating in public programs (Besley & Coate, 1992)? Or is their potential efficacy more linked to trust and perceptions of honesty/competence in individual leaders, which then carries over to the program they are advocating for? Given the growing lack of trust in public institutions (Pew Research, 2022), getting a better sense of the mechanism at play here is vital, as it can drive better public messaging and social outcomes. Indeed, as the COVID-19 pandemic has shown us, which institutions citizens trust is an important and idiosyncratic factor in policy uptake, and one that is not well understood. Changing the perceived institutional origin of a policy

has important implications for perception. Therefore, better understanding whether specific political figures can positively influence citizen perceptions of broader public institutions is crucial.

The results here also provide little evidence that gender or racial concordance between messenger and recipient results in greater efficacy from messaging (as measured by behavior change in response to messaging). This contrasts with some prior work on racial and gender concordance effects on healthcare utilization and educational outcomes (Durantini et al., 2006; Garrick et al., 2019; Greenwood, et al., 2018; Dee, 2004; Dee, 2005; Lusher et al., 2018) and on symbolic representation in the public administration literature as well (Ricucci, et al., 2016). Of course, the context here is quite different from the former set of studies - this was a light-touch nudge intervention while those were sustained, personal interactions (doctor-patient, teacher-student). Therefore, a reasonable conclusion here is that with nudge interventions that use letters and other less personal means of communication and connection, some of the racial and gender concordance effects seen in other domains may not manifest in the same way. Furthermore, this article supports the argument that more conceptual replication work is needed to better understand the true behavioral impacts of symbolic representation and their underpinning mechanisms (Sievert, 2021; Van Ryzin, et al., 2017).

It is worth noting that the letters used in this intervention represented simplified versions of the letters normally sent by PHDC for this purpose. That is, I worked with PHDC to create streamlined letters that were more compact and simple (language-wise) than the previous three motivational letters used by PHDC with this sample (which were longer and more complex). The very positive response from PHDC leadership to the response rate in the sample studied here (roughly 17.5%) suggested that these letters were much more effective than the previous three (though this was not explicitly tested here — there was no “complex letter” condition). This finding is consistent with some past work finding that complexity can significantly diminish the efficacy of messaging intended to trigger behavior change in the domain of social program uptake (Bhargava and Manoli, 2015). Moving forward, the lesson for practitioners is that behaviorally-informed modifications to messaging can enhance the persuasive impact of information: simple and clear messaging is preferable to the complex, jargon-heavy communications that are common in much public sector messaging.

Finally, when assessing the generalizability of the results here, it is crucial to consider the context of this specific intervention. In this experiment, I studied low-income individuals in a large American city who were unpersuaded by several prior attempts to prompt them to take up a social program. How might similar interventions work in very different political contexts, outside of the U.S. for example? There is significant variability in trust in politicians and in bureaucracy around the world, and studying these topics in different contexts is important to determine which insights might generalize and which may be context specific. Furthermore, the mode of communication here was a physical letter and not an email, phone call, or text message. Increasingly, public sector officials have alternative means of connecting with (and thereby building trust with) citizens beyond traditional mail. It is important that messaging, and messenger effects, are tested in these contexts to inform the messaging campaigns of the future.

Finally, this work emphasizes the crucial importance of the ongoing discussion in applied behavioral science on heterogeneity in the study of nudge interventions (Bryan, et al., 2021; Hallsworth, 2022). There is a tension inherent in the results discussed here; while the messenger effects for most political messengers in this study were not distinguishable from zero, there was one politician who seemed highly persuasive. Rather than immediately dismissing this single case as an outlier, it is both practically and theoretically useful to think about whether the focus on average treatment effects in nudge interventions might mask heterogeneities of treatment effects that are both conceptually instructive and practically valuable. While the data limitations in this study make such a discussion challenging here, I hope this article contributes to and supports the importance of the ongoing heterogeneity discussion, which is crucial to maximize the impact of behavioral science in applied settings.

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