Straight to Action: 
The Effect of Individual Empowerment on Democratic Consolidation

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Work in progress.

Abstract
What prompts citizens to undertake costly civic actions in unconsolidated democracies — their sense of individual agency or their sense of community? Despite the critical role civic engagement plays in democratic consolidation, few experimental studies have examined the underlying psychological motivations behind individual civic actions. Using an original experiment in Southeast Ukraine (N=1,381), I demonstrate that individual empowerment constitutes a sufficient condition for civic engagement. Moreover, contrary to most theoretical expectations, both individual and collective empowerment cause a direct behavioral change in civic involvement irrespective of the change in attitudes and intentions. These results explain why recent experimental interventions failed to produce an attitudinal change in communities exposed to pro-democratic information. This paper contributes to our understanding of the underpinnings of civic culture by demonstrating that individual and collective empowerment form two distinct pillars of civic action. It also advances the theory of democratic learning by showing that democracies consolidate by providing opportunities for people to engage in various civic activities rather than inculcating democratic values through civic education and top-down democracy promotion.

Keywords: field experiments, efficacy, democratic behavior, democratic consolidation, democratic learning, Eastern Europe

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# Contents

1 **Introduction** 4

2 **Theoretical Framework** 7
   2.1 Civic Engagement in Weak Democracies 7
   2.2 Hypotheses 9

3 **Research Design** 10
   3.1 Experimental Context 10
   3.2 Experimental Intervention
      3.2.1 Self-Efficacy 13
      3.2.2 Collective Efficacy 15
      3.2.3 Combined 17
      3.2.4 Control 17
   3.3 Outcome Measures
      3.3.1 Intentions 17
      3.3.2 Behavior 19
   3.4 Treatment Assignment 20
   3.5 Experimental Validity 21

4 **Empirical Results** 22
   4.1 Model Estimation 22
   4.2 Results
      4.2.1 The Change in Intentions 22
      4.2.2 The Change in Behavior 23
      4.2.3 Intention-Behavior Inconsistency 25
      4.2.4 Heterogeneous Treatment Effects 27

5 **Discussion** 29

6 **Conclusion** 31

7 **Appendix** 33
   7.1 Randomization
      7.1.1 Subjects Recruitment 33
      7.1.2 Organization 34
      7.1.3 Randomization Procedure 35
      7.1.4 Covariate Balance 36
      7.1.5 Block Random Assignment 39
   7.2 Experimental Intervention
      7.2.1 Self-Efficacy 40
      7.2.2 Collective Efficacy 44
      7.2.3 Combined Efficacy 47
      7.2.4 Instrument: Attitudinal Responses 48
      7.2.5 Instrument: Behavioral Responses 49
      7.2.6 Manipulation Check 50
   7.3 Treatment Outcomes 51
      7.3.1 Outcomes Statistics 51
7.3.2 Tabular Forms of Main Results ................................. 54
7.3.3 Intention-Behavior Inconsistency ............................... 55
7.3.4 Summary of Randomization Inference Trials .................. 56
7.3.5 Added Variable Plots ........................................... 61
7.3.6 Heterogeneous Effects: Generalized Random Forests .......... 64
1 Introduction

What prompts citizens to undertake costly civic actions in unconsolidated democracies — their sense of individual agency or their sense of community? On the one hand, individuals are expected to believe in their ability to achieve an intended goal before engaging in costly civic action (Bandura 1982, Solhaug 2006, McNatt & Judge 2008, Caprara et al. 2009, Vecchione & Caprara 2009, Sanborn 2015). Efficacious, educated, economically secure individuals are more likely to join civic associations, monitor elections, and control bureaucrats. On the other hand, democracy is a collective endeavor, and strong sense of community might be more relevant for solving community-related problems (Sampson et al. 1997, Bandura 2000, Morenoff et al. 2001, Stajkovic et al. 2009, de Rooij et al. 2009, Lee 2010, Kim 2015). It is not clear, however, how sense of community affects civic behavior in contexts where beliefs in collective capabilities are weak (Croke et al. 2016).

Surprisingly, no experimental study has attempted to empirically manipulate individual and collective motivations to clarify their effects on civic participation during democratic consolidation. Existing research in comparative politics has traditionally emphasized institutional parameters of democratic consolidation, leaving the interplay between individual and collective motivations largely unaddressed (Almond & Verba 1963, Putnam et al. 1993, Skocpol & Fiorina 1999, Galston 2001, Theiss-Morse & Hibbing 2005, Nabatchi et al. 2012, Pancer 2015). Yet, good institutions – such as fair elections and efficient bureaucracies – emerge from unsteady and poorly coordinated civic actions and concessions (Diamond 1999, Mann & Patrick 2000, Mishler & Rose 2001, Theiss-Morse & Hibbing 2005, Domínguez & Jones 2007, Svolik 2013, Berman 2019, Pisano Forthcoming). The examination of practices and skills on the ground is a way of dissecting macro-institutional changes that democratization brings (Thelen 2004). By focusing on clearly identifiable micro-level motivations behind individual civic actions, we advance our understanding of the mechanisms through which individual attitudes and actions make democracy the only game in town (Linz & Stepan 1996).

This paper presents one of the first experimental tests of the effects of individual and collective motivations on civic engagement in weak democracies. I design a lab-in-the-field experiment that randomly manipulates these two motivations and identifies which one of
them is more conducive to civic engagement: seeing oneself as an achiever (self-efficacy), or
belonging to a potent and powerful community (collective efficacy). I expose my respondents to cognitive interventions, encouraging them to reflect on their personal experience of problem-solving and community successes in civic actions. Targeting respondents’ beliefs about their efficacy as both individuals and members of local communities, I am able to causally identify the effect of self- and collective efficacy on civic behavior and intentions.

This experiment takes place in seven locations in Southeast Ukraine, a region with high electoral abstention rates and relatively passive civic engagement (Bilousov 2002, Madin 2008, Pietrzyk-Reeves 2008, Gatskova & Gatskov 2015, Giuliano 2015, 2018). Southeast Ukraine’s long history of authoritarianism makes it a particularly stark example for the study of civic involvement (Hinckley 2009, Gatskova & Gatskov 2015, Ekman et al. 2016, Stewart & Dollbaum 2017, Kvartiuk & Curtiss 2019). To account for potential preference falsification anticipated in these settings, I measure both intentions and actions (Kuran 1995). The behavioral response records respondents’ actual interest in learning about new community development projects and sharing this information with their friends and family. The attitudinal response measures respondents’ interest in joining an electoral commission, campaigning for recycling, establishing a civic council to improve bureaucratic accountability, and leading a homeowners’ association.

The results of the experiment show that individual and collective empowerment form two distinct pillars of civic action. The induction of beliefs about one’s capacity to achieve desired goals without any hint of civic education is sufficient to motivate higher levels of civic involvement. Against expectations derived from observational data (Morenoff et al. 2001, Stajkovic et al. 2009, Omoto et al. 2010, Velasquez & LaRose 2014, Chen 2015, Doherty & Webler 2016), high individual self-efficacy and awareness of collective civic successes are equally important for civic mobilization.

Moreover, contrary to most theoretical expectations, the experiment shows that the improvement in efficacy causes a direct behavioral change in civic involvement irrespective of the change in attitudes and intentions. This finding stands in contrast to the widespread assumption that attitudes are the exclusive driver of individual political behavior. Behavioral change does not require a shift in intention as long as individuals believe in their
ability to achieve desired civic outcomes.

This research contributes to the literature on democratic consolidation, democratic learning, and political accountability. This paper is one of the first experimental studies to bring psychological motivations to the fore of democratic consolidation. It provides micro-level evidence of the transformative effect of individual and collective empowerment on civic engagement after democratization. It also sheds light on the relative importance of individual and collective pillars of civic engagement, demonstrating that confident citizens and capable communities are equally relevant to civic engagement. This perspective explains the connection between constitutional, attitudinal, and behavioral dimensions of democratic consolidation: democracy becomes the only game in town when constitutional changes allow for regular and spontaneous civic actions, which in turn result in attitudinal consolidation (Linz & Stepan 1996).

Moreover, this paper supplements the relative lack of research on micro-level democratic learning, showing that civic intentions and attitudes might change more slowly than practices, and that democracy consolidates through action. In other words, democratic culture is built by providing opportunities for people to engage in various civic activities rather than inculcating democratic values through civic education and top-down democracy promotion. These findings provide strong empirical support to the literature on democratic learning and suggest that it might be even more behaviorally driven than the research has implied so far (Peffley & Rohrschneider 2003, Meirowitz & Tucker 2013). This way, civic skill formation becomes a harbinger of the evolution of democratic institutions. This is consistent with recent observational studies that demonstrate how the experience of living in a democracy itself endogenously forms pro-democratic preferences (Quintelier & Deth 2014, Fuchs-Schündeln & Schündeln 2015, Kostelka & Blais 2018). These findings also add some optimism in the perspectives of democratic learning in post-Soviet states (Hinckley 2009).

Finally, this perspective ties up some loose ends in the extant experimental research in political accountability, explaining why field experiments often result in an inconsistency between intention and behavior in treatment outcomes (Boas et al. 2018, Dunning et al. 2019). Field experiments on civic engagement have traditionally focused on attitudes and
assumed their downstream effect on behavior. This paper demonstrates that there might be multiple paths to action, one of them being perceived individual and collective capacity to achieve civic goals. During the initial stages of exposure to democratic principles, individuals might be adapting new civic habits without an immediate change in their declared intentions. While post-authoritarian attitudes are hard to change, voters might be more amenable and agile in their behavioral practices.

2 Theoretical Framework

2.1 Civic Engagement in Weak Democracies


Existing micro-level explanations of civic action in unstable settings focus largely on electoral behavior (Schedler 2001, Brender & Drazen 2009, Svolik 2013), mostly because a ‘disenchanted population’ can ruin a newly born democratic regime as effectively as paramilitary groups and authoritarian leaders (Schedler 1998, 96). This is problematic for two reasons. First, the emphasis on voting behavior and public opinion ignores alternative, and potentially more meaningful, ways in which political power is exercised. Ethnographic evidence demonstrates that people might view electoral participation as a performative act rather than a meaningful way of expressing their political or social will (Pisano Forth-
coming). Second, a ‘disenchanted population’ can nevertheless take advantage of existing
democratic opportunities, and few studies have managed to explain how the fresh op-
portunities that emerge in a newly democratized regime lead to civic involvement at the
micro-level. Although the literature acknowledges the importance of collective efforts to
consolidate democracy by investing in institutions of civic action and bureaucratic control
(Berman 2019), there is scarce, mixed evidence that studies the individual drivers of civic
engagement in unconsolidated settings (Sanborn 2015, Larreguy & Marshall 2017, Kerr
2017).

Thus, to understand how democracies consolidate at the micro-level, an explanation is
needed for the individual motivation to join civic activities that safeguard the survival of
democratic institutions: such as groups that monitor the quality of elections, keep civil
servants accountable and governance transparent, and ensure a public discussion of issues
of shared importance.

As any intentional process, civic involvement can be interpreted with the framework
of the theory of planned behavior (TPB) (Ajzen 1985, Armitage & Conner 2001, Ozkan
Gifford 2019). The theory assumes that any behavior stems from the attitude toward the
action, subjective norms about the action, and the degree of perceived behavioral control
or self-efficacy (Figure 1). The degree of perceived behavioral control is considered to have
a direct effect on behavior when it coincides with actual behavioral control, and all three
components indirectly drive individual behavior by modifying the intention to participate.

For example, through the lens of the TPB, civic engagement stems from the attitudes
toward civic engagement (arrow A1), subjective norms about it (arrow A2), the perception
of individual efficacy (arrows A3 and A4), and the intention to engage in a civic activity
(arrow A5). In the short run, democratic empowerment decreases the costs of civic engage-
ment and increases personal efficacy because it reduces the perceived costs of collective
action, establishes freedom of association, encourages citizens to take part in electoral ob-
ervation, and allows for the establishment and open competition of independent political
parties. As a result, citizens in democratizing states experience an increase in their efficacy,
both perceived and actual. Thus, democratic empowerment improves the link between per-
ceived behavioral control and behavior (arrow A4), as well as the link between perceived behavioral control and intention (arrow A3). In the long run, democratic empowerment transforms subjective attitudes and norms about civic engagement and thereby facilitates the path from norms to intentions (arrows A1, A2, and A5).

2.2 Hypotheses

This research, however, neglects the role of the social context in which individuals resort to civic action. The abundant evidence linking social capital and trust to the formation of civic engagement comes predominantly from developed democratic regimes (Almond & Verba 1989, Sampson et al. 1997, Newton 2001, Stajkovic et al. 2009, Lindström 2010, Ansari 2013, Ebert & Okamoto 2013, Kim 2015, Capurro & Contreras 2015). Evidence from electoral authoritarian regimes such as Zimbabwe, on the contrary, shows that education — an important proxy of self-efficacy — does not predict individual involvement in broken political institutions (Croke et al. 2016). Other studies suggested that the experience of living in a democracy that contributes to the development of self-expression values rather than the other way around (Dahlum & Knutsen 2015).

One way to address this puzzle is to experimentally manipulate various facets of efficacy and measure their effect on civic participation (Table 1). If the social context matters, high self-efficacy (and its particular forms, such as internal or external political efficacy) is a necessary but insufficient condition for civic involvement in collective actions. Civic intentions and actions will be caused only by a simultaneous increase in both self- and collective efficacy (Hypothesis 1). If the alternative hypotheses are true, then self- and collective efficacies independently influence civic engagement, and their combination does not produce a compound effect (Hypotheses 2a and 2b).

Two additional hypotheses can be derived from this theoretical framework. Based on the TPB, efficacy might affect civic engagement directly and through the change in intentions. I reserve two hypotheses to test this supposition. One assumes that the increased sense of efficacy transforms civic engagement via the cognitive update in intentions (Hypothesis 3a). The other assumes that the cognitive change might not be required to cause a behavioral response (Hypothesis 3b).

**Hypotheses**

*Hypothesis 1:* The combined induction of self- and collective efficacy improves civic engagement.

*Hypothesis 2a:* High self-efficacy is a sufficient condition for civic engagement.

*Hypothesis 2b:* High collective efficacy is a sufficient condition for civic engagement.
Table 1: The Summary of Theoretical Expectations

<table>
<thead>
<tr>
<th>Engagement Driver</th>
<th>Hypotheses</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Self-Efficacy</td>
<td>Empowerment → behavioral change</td>
</tr>
<tr>
<td>2 Collective Efficacy</td>
<td>Empowerment → change in intentions → behavioral change</td>
</tr>
<tr>
<td></td>
<td>Empowerment → behavioral change</td>
</tr>
<tr>
<td>3 Self- and Collective Efficacy</td>
<td>Empowerment → change in intentions → behavioral change</td>
</tr>
<tr>
<td></td>
<td>Empowerment → behavioral change</td>
</tr>
</tbody>
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*Hypothesis 3a:* The induction of any individual efficacy improves civic engagement directly.

*Hypothesis 3b:* The induction of any individual efficacy improves civic engagement only for those whose intentions have changed.

### 3 Research Design

#### 3.1 Experimental Context

The experiment is organized in Southeast Ukraine in 2018. The choice of the research site takes into account my theoretical interest in the effect of democratic empowerment on the development of civic engagement and builds on the insights derived from previous field-based interventions.

First, some field experiments that manipulate political and civic participation in hybrid regimes are performed precariously. More specifically, they put citizens at risk by pushing them towards closer interaction with the state. Unlike other lucky ‘survivors in favorable circumstances,’ Ukraine is an emerging democracy with a strong societal commitment to consolidation\(^1\) (Svolik 2008). Exposing Ukrainians to random democratic empowerment

\(^1\)Ukrainian democracy has not consolidated yet. Judging by a standard proposed by Scott Mainwaring (Mainwaring 1989), it performs relatively well on the dimension of competitive elections as the only principal route to political office (with an exception of the career paths of revolutionary leaderships in two Maidan revolutions of 2004-2005 and 2013-2014). The adult citizenship is broad, with the exception of those internally displaced people from the Crimea peninsula and ATO zone in Donbas who had troubles
does not put them under immediate identifiable risk, since the Ukrainian political context encourages civic activities (Onuch et al. 2018, Onuch & Sasse 2016).

Second, the novelty of experimental interventions is an important condition to ensure experimental success (Rao et al. 2017, Lieberman et al. 2014). Ukraine is undergoing rapid political transformation, and most people remain unaware that many new features and institutional opportunities have been made available to them. As a result, an experimental intervention that improves their efficacy by providing them with civic information is perceived as novel. In addition, this information was also helpful because in 2018, most Ukrainians were dissatisfied with the pace and intensity of democratic transformation (Matsiyevsky 2018).

Third, organizing the experiment in Southeast Ukraine offers a unique window to observe the micro-level dynamics of civic engagement in the making. Analysts describe the average Ukrainian’s civic life as passive because of “disappointment with the post-Soviet transformation and low subjective social status” (Gatskova & Gatskov 2015, 673) and underscore the limited role of civil society organizations in disseminating cooperative and democratic values (Stewart & Dollbaum 2017). The experiment identifies the specific sequence of the update in attitudes and behavior that happens when democratic transformation emancipates individuals in hybrid regimes. By doing so, the experiment provides a micro-level explanation of the variation in survival of incomplete democratic regimes.

Finally, for democracy to be sustained, rapid formal democratization must be transformed into a rich civic engagement. Incomplete democracies “must enjoy sufficient support to survive and sustain a democratic course for the duration of the transition” (Mishler & Rose 2001, 304). The experiment in Ukraine creates a range of social benefits by identifying the effects of democratic empowerment on emerging civic engagement.

The Southeast of Ukraine is an especially challenging test for the experiment. The study is set in Kherson oblast (region) (Figure 2). This region is a typical province in Southeast Ukraine with low levels of civic engagement and political turnout (Bilousov 2002, Bely 2016, Central Electoral Commission (Tsentral’na Vyborcha Komisiya) 2015). This oblast is more economically deprived and politically passive than the regions in western and central registering to vote. Petro Poroshenko presidency (at the time of experimental fieldwork) was relatively harsh on minority rights and traditional civil liberties were not guaranteed for all.
Ukraine. The region has never been a part of the East-West cleavage that took place after the Orange Revolution (Madin 2008, Susak 2011). In the presidential elections of 2004, Kherson featured the narrowest margin between Viktor Yanukovych and Viktor Yushchenko (52% to 48% in the second round), making it the least electorally polarized oblast of Ukraine (Central Electoral Commission (Tsentral’na Vyborcha Komisiya) 2004). In addition, civic engagement in the region shows some signs of post-Soviet nostalgia (Giuliano 2015). At the same time, in the post-Soviet history of the country, this region had no clear dominance of either post-Soviet political parties with access to administrative resources (‘political machines’ (Frye et al. 2014)) or oppositional parties without such access (Berezyns’kyy 2005).

Figure 2: Kherson Oblast on the map of Ukraine

3.2 Experimental Intervention

To recreate democratic empowerment in a laboratory setting, I randomly increase subjects’ perception of their efficacy as individuals and as members of a collective. This intervention is not supposed to cause a long-term change, but it approximates democratic empowerment well enough for the experiment’s purposes, and it allows me to measure an average behavioral and intentional change associated with similar real-life political changes.
Subjects in the first experimental condition received a cognitive intervention which manipulated their self-efficacy by priming their awareness of personal successes. Importantly, the treatment did not feature any information related to civic engagement. All examples were private (family relationships, personal or professional development). The only mechanism that the treatment is expected to work through is the induction of self-efficacy. Subjects in the second experimental condition were educated about recent community successes in recycling, infrastructure innovation, property management, and anti-corruption school budget reform. The improvement of collective efficacy is achieved through learning from the success stories and developing a plan to adopt these successes in their own neighborhood during a subsequent brainstorming session. The combined treatment causes the simultaneous induction of self- and collective efficacy. The difference between the treatments decomposes the effect of self-efficacy per se (the one manipulated in the first treatment) and the effect of collective efficacy. Appendix 7.2 provides a sample text used in experimental interventions, and the expectations are summarized in Table 1.

3.2.1 Self-Efficacy

In an experiment like this, there are few alternative ways of manipulating civic efficacy. The first way, which is more straightforward, would be to remind people of their rights and opportunities as citizens of an independent democratic state. This intervention, however, would probably fail because civic education per se is not a sufficient tool for improving civic engagement (Chatterjee 2018, Rao et al. 2017, Dunning et al. 2018). An alternative way to manipulate civic efficacy would be to activate the mechanisms through which democracy works – that is, randomly improve individual efficacy via cognitive intervention. The intervention would combine the manipulation of two interrelated aspects of self-efficacy: self-efficacy itself and internal locus of control.

The calibration of individual locus of control is a safety check before the manipulation of self-efficacy. First, and most importantly for the subsequent manipulation of self-efficacy, it reduces frustration by balancing unrealistic expectations. The concept of locus of control refers to the way that individuals attribute the outcomes of their performance to themselves or to external factors. Internal locus of control puts an emphasis on what one can change,
that which is “within one’s locus of control.” Civic engagement programs seem to boost citizens’ self-esteem without providing them with tools to calibrate their locus of control. As a result, the citizens develop unrealistic expectations for democratic practices (Galvin et al. 2018). Second, the concept of internal locus of control correlates with higher political and civic engagement (Anisfeld 1981, Fast 1973) and higher political trust (Lindström 2010). Third, it activates a sense of agency (Sullivan 1993, Launius & Lindquist 1988) and overcomes learned helplessness in politics, since it is associated with less of a need for strong leadership and authoritarianism and with higher demand for self-governance and independence (Hiroto 1974, Jarmakowski 2009). Finally, the concept of internal locus of control orients citizens towards their immediate surroundings, reducing the pressure of polarized national politics and priming them to care about their community (McCarty & Shrum 2001, Mostafa 2016).

With this experiment, respondents began by completing a quick introductory session that acquainted them with the ideas of internal locus of control and self-efficacy. The information was presented in a way that people with no psychological education could understand. At the end of this session, subjects were asked to consider several mundane life situations and decide which of the presented solutions were within the locus of control of the characters involved.

At this point, the treatment turned to the induction of self-efficacy. Respondents were asked to write short reflections (on a provided printed form) about some life concern of
their choice. Most respondents chose topics related to health improvement, a job change, and family relationships. The form prompted them to outline some realistic solutions to their concern, providing nudges such as “What can you feasibly change in this situation?” and “How can you improve your control over this situation?” Research assistants checked the reflections for completion and accuracy to make sure that the respondents identified the solutions correctly. After that, respondents were asked to reflect upon their past successes and to identify personal strengths that helped them to achieve their goals in similar life situations. The treatment ended with respondents listing the traits of their personality that they are proud of and that help them to reach desirable outcomes in life. The intervention is discussed in detail in Appendix 7.2.1.

To be sure, it is unlikely that this intervention generates a long-lasting cognitive benefit. At the same time, most respondents confessed that they had never thought about their lives through the lens of internal locus of control and high self-efficacy, and they stated that they enjoyed this new perspective.

In this condition, subjects were isolated from each other and did not have a chance to discuss their handouts with anyone but research assistants.

### 3.2.2 Collective Efficacy

The second treatment primed the subjects regarding collective efficacy. The design of the treatment repeated the structure of the first one, prompting subjects to receive some information on the topic and then reflect on their efficacy in achieving desirable outcomes. At the same time, it focused on supporting the sense of belonging to an active community, sharing the skills for social action, engaging in deliberation, and pursuing public goals in collaboration with peers (Beaumont 2011, Bruton et al. 2016). Appendix 7.2.2 features the description of the intervention.

Respondents began by watching four videos made specifically for the experiment by local TV presenters. The videos describe four original and successful real-life initiatives led by local people: a new, transparent system of school budgeting, an innovation in electricity infrastructure, the establishment of a homeowners’ association, and a recycling campaign. (see Figure 4a and Figure 4b in this section and other examples in Appendix 7.2.2). After
the videos, all respondents filled out a short questionnaire with comprehension questions. The results showed that most of respondents correctly understood the content of the videos. Specifically, the comprehension rates (the number of correctly answered questions) were 76.8% and 75.3% in the groups that received this treatment. Up to this point, subjects were isolated from each other.

Following the questionnaire, subjects were invited to a civic brainstorming session, guided by moderators (See Figure 14). These moderators were recruited from local universities’ psychology departments and were trained to lead the brainstorming group discussion. During the session, subjects were prompted to discuss the videos, share their opinion on other local initiatives, and think of potential tools of engagement that they could use to enhance the quality of their lives. The discussions used deliberative practices to improve collective efficacy (Bowler & Donovan 2002, Jaske 2018), and it induced a growth mindset to prevent respondents from leaning toward negative thinking about past failures (Claro et al. 2016, Ng 2018). For example, when one of the respondents shared a depressing story of a failed civic initiative, the moderators helped respondents to critically evaluate the reasons behind the failure and to identify ways to overcome similar obstacles in the future. Similar to the reflection about personal strengths and successes in the Self-Efficacy treatment, the Collective Efficacy treatment incorporated a reflection on community achievements and highlighted the potential for success.
3.2.3 Combined

In the third group, cognitive intervention and a civic brainstorming session were combined. The intervention preceded the civic treatment because it was essential to prime the respondents’ efficacy before they were exposed to the information on community efficacy. An alternative design (democratic education before cognitive intervention) would also be appropriate if the researcher is interested in the effect of community potency on individual self-evaluation, but this dynamic is beyond the scope of this study. For this study, this design allowed me to decompose the effects of political information on civic engagement and to detect whether cognitive training provides a mediating mechanism through which democratic education works.

3.2.4 Control

The control group had no cognitive or information intervention. I restrained from administering a placebo treatment because, in this case, it would be difficult to develop a single placebo for both intervention and education treatments (as they were administered in different formats and ways).

3.3 Outcome Measures

3.3.1 Intentions

To differentiate between intended and actual civic engagement, I collect verbal (self-reported) and non-verbal (observed by experimenters) responses. From a psychological standpoint, all of these activities are behavioral (Ajzen 2005). However, I comply with political science conventions by referring to verbal responses as to ‘attitudinal’ or ‘intentional’ (as they measure declared attitudes towards the action) and to non-verbal responses as to ‘behavioral’.

Based on previous research on civic engagement, I focused on the following civic activities (Zukin et al. 2006, Bermeo 2003, Nabatchi et al. 2012): (1) regular volunteering for a pro-recycling organization; (2) working with others to exert civic control over rigged elections by signing up as an electoral observer; (3) running for a leadership position in a homeowners’ organization; and (4) establishing a civic council (a quasi-formal civic entity
that allows for tighter control over public expenditures in municipal organizations).

These four activities were chosen to appeal to various audiences; they range from one-time commitments to regularly scheduled, costly commitments and from politically or socially desirable activities to slightly contentious ones. Appendix 7.2.4 contains specific survey questions used to capture these outcomes.

For a socially desirable one-time activity, I chose the intention to campaign for a recycling program in the region. Pilot work showed that the majority of the local population supports the recycling of plastic, glass, and other reusable materials, and recycling generates virtually no backlash. This non-political and socially important activity was chosen as the easiest test for experimental manipulation.

For a socially contentious one-time activity, I chose the intention to join an electoral commission. Electoral observation is a crucial civic activity for the Southeast of Ukraine. The region is embedded in a wider clientelist network that Ukrainian politicians have actively used to manipulate elections. Ensuring the transparency of elections is critical for the survival of democracies in the former Soviet states (Bunce & Wolchik 2010, Svolik 2013, Frye et al. 2014), and civic engagement is essential for preventing electoral fraud and manipulation. Electoral observation is a relatively rare form of civic activism, and it is not yet universally widespread in the region. It requires a commitment to stay on electoral polls and confront the manipulations, but it is a one-time activity because the elections are a unique rare event.

For a socially desirable regular activity, I chose the intention to lead an “organization of apartment building property co-owners” (Ukr: OSBB, Ob’ednannya Spivolasnykiv Bakhovartryznoho Budyntu). This organization is a legal public body that can be formed to manage public property and make effective decisions on its exploitation. This activity requires substantial time commitment, but it does not entail any direct confrontation with authorities or implicit political significance.

Finally, for a socially contentious time-consuming activity, I chose the intention to establish a civic council (Ukr: Hromads’ka Rada). Civic councils are permanent collegial elective public advisory bodies that can be set up to facilitate public participation in public policy. They are legally empowered to exercise public control over the activities
of executive authorities; and by law, they “promote public opinion in the formation and implementation of public policy” (Cabinet of Ministers of Ukraine 2010). Civic councils can be assigned to any public body to control budgetary expenses, advise civil servants, and resolve contentious matters around publicly funded projects such as the purchase of school supplies, infrastructure repair, and waste management.

This set of activities works well for the experiment for several reasons. They represent opportunities for engagement in the region that are non-military (do not allude to the military conflict in Donbas), and they are non-polarizing. The activities are also common, and the information on their implementation is publicly available. The willingness to join these activities is measured with a simple four-point Likert scale from “definitely not” to “definitely yes,” with the direction of the scale randomized across individuals.²

Immediately following the survey, and before the discussion, the respondents’ attitudinal outcomes were measured. For the collective efficacy and combined groups, I recorded the behavioral outcome after the group discussion to specifically detect the difference between the treatment effect induced by cognitive intervention, videos, and the combination of the two. At the same time, it should be noted that additional caution is required when interpreting my behavioral measure since it is not possible to distinguish between the separate effect of group discussion and educational videos.

3.3.2 Behavior

To record a behavioral response that would be discrete enough not to be perceived as a response, I developed an informational leaflet in collaboration with local civic activists. (Detailed information about the measurement strategy for behavioral responses can be found in Appendix 7.2.5.) This leaflet featured a set of activities potentially available in the region (such as enrolling in an energy-saving program for one’s residential building from a local NGO or participating in a historical conservation program). The description was followed by an easily accessible action plan with steps the respondents would need to undertake to benefit from these programs. The leaflets were left on a separate table,

²I measure the intentions to run for political office and discuss the effect of self- and collective efficacy on political participation in a separate paper (Soboleva 2019a).
and subjects could take the leaflets on their way out from the site of the discussion. We specifically told them that “the leaflets contain useful information about the opportunities for civic activism” in their city and that “you might consider circulating them with your friends and family.” The respondents were not asked about the exact number of leaflets they took, but my research assistants recorded it discretely.³

This behavioral response is meaningful in several ways. First, subjects were explicitly informed of the content of the leaflets and had a chance to examine the leaflets closely before committing to taking them. Some did just that, finding no use in taking the leaflets home. Most respondents, however, found the provided information on engagement opportunities valuable enough to circulate it to others. The act of taking a leaflet is thus a useful indication of initial interest in civic engagement programs. Second, taking more than one leaflet implies a stronger commitment to spreading useful information among friends and neighbors – and this social initiative is a crucial proxy for successful civic engagement. Third, the response was completely spontaneous. The subjects knew that the experiment was over, and they were no longer expected to “comply” with the experiment’s rules. Thus, this act of civic commitment is a sensible proxy for a sincere intention to participate in a civic activity.

3.4 Treatment Assignment

In 2018, I recruited 1,381 respondents for the experiment after piloting the questionnaire without tracking the outcome measures. The subjects were recruited through three recruitment channels: (1) random demographically weighted recruitment on streets; (2) personal invitations distributed through mailing lists provided by local social activists; (3) and targeted online recruitment by way of social media. The resulting sample is more female than the general population (57.8% compared to 52.1%) and close to the population median of age (40.3 compared to 40.4). Linguistically, the sample is representative of the Ukrainian

³Although some respondents, being especially excited about what they learned during the experiment, became interested in sharing this information beyond their inner circle, and therefore requested a specific number of leaflets (usually some round number such as 40 or 50). Also, many respondents were seen or heard counting the number of leaflets they needed (e.g., “one for me, one for my colleague, three for my neighbors...”), so sometimes we could learn the exact number from them directly.
East and South (16.4% of respondents speak Ukrainian as their first language). The sample is diverse enough to detect heterogeneous treatment effects (Appendix 7.1.1).

A blocked randomization protocol was used on-the-spot to assign respondents to one of four blocks. These blocks were formed based on respondents’ age and gender, reducing sampling variability and increasing the precision of ATE estimates (Appendix 7.1.5). Chart 10 summarizes the flow of the subjects from recruitment to treatment assignment. In addition, the treatment is balanced along other covariates (Appendix 7.1.4).

As to the logistics of the experiment, the study was administered in seven locations – five in rural or suburban areas and two in the center of Kherson. All respondents received monetary compensation at the end of the survey. The experiment was set in a lab-in-the-field, and the treatment assignment resulted in the reception of the treatment by all subjects. Subjects had personal ID cards with a masked group identifier during the experiment, ruling out the possibilities of mistakenly treating a subject from the control condition or failing to administer the treatment to those in treated conditions.

### 3.5 Experimental Validity

Overall, the experimental manipulation of efficacy might appear quite far from real-world processes. I claim, however, that external validity of such intervention is sufficiently high. In fact, young democracies such as Ukraine experience a rapid change in the individual efficacy of their citizens every time they democratize (Letki & Evans 2005). The costs of civic action are defined externally by legal restrictions and informal contracts (i.e., the possibility to arrange a non-violent rally without being detained by the police or the possibility to establish a civic association without preliminary permissive negotiations with government officials). These costs might change within a short period of time. For instance, they could change when a new democratic government comes to power and abolishes restrictive regulations of the Ancien Régime. Subsequent democratic empowerment is exogeneous for most people in the sense that they have no control over the time or the exact way that the new government abolishes restrictive rules.\footnote{For instance, democratization has an exogeneous one-off effect on social outcomes such as infant mortality (Kudamatsu 2012).} Civic engagement, on the contrary, is
endogenous and idiosyncratic. By definition, it is impossible to ‘civically engage’ people without their explicit consent.

This experiment does not cover all possible types of costly civic actions. For example, I do not focus on violent protests or any types of contentious mobilization. At the same time, I assume that the role of individual and collective motivations in such actions would be similar to the actions discussed in the study.

4 Empirical Results

4.1 Model Estimation

I estimate intent-to-treat effects with a linear model:

\[ Y_{i,j} = \gamma_1 \ast \text{Self}_i + \gamma_2 \ast \text{Collective}_i + \gamma_3 \ast \text{Combined}_i + X_i \lambda + \mu_j + \epsilon_{i,j} \]

where \( Y_{i,j} \) is an outcome measure, measured with a linear regression; the Self, Collective and Combined variables are treatment indicators of Self-Efficacy, Collective Efficacy, and Combined treatments respectively; \( X_i \) is a vector of covariates which includes gender, language, education, age, and socioeconomic status; \( \mu_j \) is a block randomization dummy; and \( \epsilon_{i,j} \) is a disturbance term. The \( i \) subscript stands for individual subjects, the \( j \) subscript stands for blocks. The received treatments are the fact of the receipt of self-, collective, or both self- and collective efficacy intervention. Since the de-facto probability of assignment to treatment varies by block, I use inverse probability weights in all model specifications (Gerber & Green 2012, 76). The standard errors are robust in all specifications (Lin 2013).

4.2 Results

4.2.1 The Change in Intentions

I start by examining the direct effects of three experimental conditions as they pertain to the respondents’ declared intention to take part in four civic activities. Figure ?? shows the effect of experimental induction of efficacy on the reported intention to become involved in at least one of four civic activities. The upper X-axis shows the outcomes as absolute
values and additionally denotes the average outcome of the control group. For instance, on a Likert scale from 1 to 4, where ‘1’ is a definite refusal to join a civic activity and ‘4’ is a definite intention to join a civic activity, the mean outcome for the intention to campaign for recycling in the control group is a 3 out of 4. The control population was already very sympathetic towards pro-recycling civic campaigning. The least desirable activity in the control group was to lead a homeowners’ organization (2.1 out of 4). Complete summary statistics for the mean outcomes in the control group can be found in Table 4.

The lower X-axis measures the estimated average treatment effect on a four-digit Likert scale. As we see, the treatment does not cause any substantial changes. In all cases, the Bell-McCaffrey confidence intervals contain zero (Table 5) (Lin et al. 2016). In the case of electoral observation, the average effects are slightly negative for the self-efficacy treatment when measured by a simple linear regression (Table 8). This result, however, does not hold true when tested against the sharp null hypothesis of no effect on any unit (see Appendix 7.3.4).

The null effects are robust and confirmed in other model specifications (Appendix 7.3.5). Ultimately, these treatments yielded only insignificant changes to the respondents’ intentions to participate in civic activism.

Figure 5: The Change in Intentions to Engage in Civic Activities
4.2.2 The Change in Behavior

The behavioral outcomes, however, deliver a strikingly different result. Figure 6 shows the effect of treatment assignment on the number of leaflets with community projects information that respondents took following the experiment. In the control group, the mean number of taken leaflets was 2.81 per person.

When subjected to the self-efficacy treatment, respondents took an average of 3.58 leaflets per person, 40 being the maximum number taken. A covariate-adjusted average treatment effect is 0.774 leaflets. Randomization inferences performed for the test statistics (in both covariate adjusted and unadjusted models) show that the results are verifiably robust (Figure 18). Further, the studentized permutation test shows that the p-value of the simulated t-test is 0.01, while the sharp null hypothesis of no effect on any unit is rejected at the 0.01 level (see Appendix 7.3.4). When exposed to an induction of collective efficacy, the subjects took, on average, 1.06 leaflets more than the control group. This result is significant at the 0.001 level (Appendix 7.3.4). Finally, subjects in the combined treatment group took, on average, 1.03 leaflets more per person than their peers in the control group. The effect is significant at the 0.01 level (Appendix 7.3.4).

The confidence intervals for three treatment effect estimates overlap, revealing that there was no statistical difference detected between the size of the three effects (Table 5 and Table 6). At the same time, the collective efficacy treatment yields results that are both robust in various specifications and confirmed at a higher level of significance than the self-efficacy and the combined treatments.

4.2.3 Intention-Behavior Inconsistency

The simultaneous absence of change in respondents’ declared intentions and a strong positive change in actions is puzzling. At first glance, these results seemingly contradict much of what is known about the role of efficacy in political engagement and the connection between intention and behavior. Whereas verbal and non-verbal measures frequently travel in different directions (Boas et al. 2018) — thus making it essential to obtain both outcomes whenever possible — a positive change in a behavioral measure, combined with an absent change in attitudinal measures, is a rare phenomenon.
There are several alternative explanations of this phenomenon.

First, these results could possibly be explained through the lens of the theory of planned behavior. The exogenous empowerment increased subjects’ perceived behavioral control and thus directly improved their behavior (arrow A4 on Figure 1). A change in intention (arrow A3), however, did not happen — the null results are shown to be consistent across all civic measures, thereby proving that there was no experimental change in intention when compared to the control group. The only paths which could, therefore, explain these changes in behavior are: the direct effect of perceived behavioral control (arrow A4), and the indirect effect that such a change in perceived behavioral control could have caused via a change in the attitudes and subjective norms surrounding civic engagement (arrows A1 and A2).

The experimental design separates the effects in these cases. Self-efficacy induction features no civically infused content (Appendix 7.2.1), thus holding attitudes and norms around civic engagement constant. The only possible change in intention for this group would necessarily have had to happen through a change in perceived behavioral control (and, as we know, the change in intention did not happen). Therefore, the only remaining causal path is A4. Conversely, collective and combined groups did receive civic education, and, moreover, the treatment activated their attitudes and subjective norms. However, this did not cause a change in their average civic intentions either.
Second, it might be that the intention questions featured costly intentions and respondents were serious and honest about their commitment to such actions. If we assume that any action is costlier than any declaration, then this explanation does not hold. If, however, we assume that some actions might be easier to make than to declare, then this would mean that the treatments only affected cheaper civic actions (i.e., taking a leaflet) and did not change costlier actions (i.e., declaring a commitment to join a homeowners’ association). In this case, the outcomes might not have meaningful consequences for more costly actions in the real world.

Finally, questions about intentions were framed as hypotheticals (“If you had some free time, would you consider joining X”). The conditional clause was added to make the question relatable even for those respondents who do not have time for such actions. Respondents, however, might have skipped the hypothetical part and focused on the time commitment entailed by the activities instead of thinking about their general attitudes about them.

Thus, these results do not provide clear-cut evidence either for or against intention-behavior inconsistency. For instance, the dispersion of treatment outcomes shows that while there was an average increase in the number of leaflets taken in all experimental conditions, it does not seem like either the control group or any treatment group displays a visible connection between the number of projects they intended to pursue and the number of leaflets taken (Figure 7). Future work might feature specific dyads of intentions and actions (one specific activity for both survey answers and behavioral responses) and suggest a wider array of questions of intention measures. These changes would allow testing if respondents indeed perceived that the questions about intentions require a higher commitment than taking a leaflet.

4.2.4 Heterogeneous Treatment Effects

A substantial variation in the expressed intentional outcomes, shown in Figure 7, suggests that experimental interventions might have caused a heterogeneous effect on respondents’ intentions to join in civic activity. To examine the heterogeneity of these effects, I have run a predictive model based on generalized random forests (Athey et al. 2018, Wager &
Athey 2018). Causal forests provide individual-level predicted estimates of treatment effects and further demonstrate whether the null effect on intentions is caused by a dispersion of treatment effects or rather a lack of effect altogether. I used all available pre-test covariates to build a causal forest (n=149) and subsequently train each model on a randomly drawn percentage of each sample (train models use 60% and test models use 40% of initial samples for each experimental group).

Figure 26 shows the spread of individual-level predicted treatment effects, measured as a change in the number of leaflets taken. Most of these treatments caused relatively homogeneous effects on actual engagement. The highest observed variability came following a combined efficacy induction, while the lowest observed variability presented in the collective efficacy condition. This difference is explained by the presence of outliers in the combined group (few people took a large number of leaflets, and their predicted effects are visible in wide confidence intervals) and the complexity of the treatment. Note that the results confirm alternative estimations of treatment effects – that combined and collective treatments are more robust than self-efficacy treatment and also have a potentially stronger effect if we were to compare the upper bounds of their 95% confidence intervals.

Figure 9 uses the same causal forest models to predict individual-level heterogeneous treatment effects for the number of declared intentions (please consult Appendix 7.3.6 to access the results for each of the outcome variables). Here, three insights are especially intriguing. First, both collective and self-efficacy treatments present relatively homogeneous results, whereas the combined treatment group demonstrates a higher degree of heterogene-
ity. This pattern is apparently visible in both verbal and non-verbal outcomes. Second, while the predicted outcomes for collective and self-efficacy groups are consistently spread around zero, some predicted effects for combined group subjects ended up in the negative zone. For some subjects in this group, the treatment might have actually had a discouraging effect on their expression of preferences. The estimated average treatment effect based on the forest estimates is -0.12 for the combined group, suggesting that the intense double treatment might have actually discouraged subjects from expressing an intention to participate in civic activities. Finally, combined efficacy induction proves to be the treatment inducing the highest heterogeneity, mostly because it was designed to interact with a wider set of potential covariates (7.3.5).

These results suggest the following preliminary conclusions. First, the change in efficacy does not unilaterally affect intention and behavior towards civic engagement. Contrary to widespread stereotypes concerning the potential difficulties in affecting changes in behavior, a change in behavior, given an experimental setting with a well-controlled manipulation of varied planned human behaviors, might actually be attainable.

Second, all types of efficacy matter for individual civic engagement. Against expecta-
Predicted Treatment Effects

Figure 9: Generalized Random Forests: Heterogeneous Effects on Civic Intentions

tions, self-efficacy and collective efficacy do not dramatically differ in their effect on civic engagement. The difference between average treatment effects resulting from the induction of self-efficacy, collective efficacy, and their combined version is negligible.

Finally, given a substantial difference in treatment induction, it makes sense to reconsider the literature regarding the facets of individual efficacy. The boundaries between collective and self-efficacy in the context of civic activism might be more blurred than we used to think.

5 Discussion

One limitation of the experiment is the degree to which we can rely on the declared verbal responses in a political context with a long recent history of authoritarian rule. Even if the respondents experienced a change in their self-evaluation, they may have refrained from disclosing these changes altogether (Kuran 1995). It is important to note that respondents were not aware of the recording of the results of the behavioral response, as they were never directly asked about the number of leaflets they took, while they expected the results of
the attitudinal survey to be “stored somewhere”. There is a chance that they might have suspected these results to be shared with political parties or local bosses (a widely shared concern that I tried to address while implementing the experiment). In this context, the behavioral measure might be the only measure that was not internally falsified.

Another limitation considers the so-called Hawthorne effect. The only authorities present at the experimental sites were experimenters themselves. Respondents might have felt obligated to take more leaflets the more time they spent with the experimenters. If this was true, then the control group subjects would have taken more leaflets the longer they stayed on experimental locations. In fact, though, the effect that time spent with experimenters may have had on the respondents was revealed to be the opposite. Rather, those who spent a longer time with the experimenters took less leaflets at the end of the survey (Table 10). If time correlated positively with more leaflets taken, we would also observe a sharper difference in treatment effects between the combined group and the other two groups because the respondents in the combined condition took a longer time to complete their tasks. This difference is not observed either.

Finally, while research assistants did their best to clearly communicate the content of the leaflets, it is possible that subjects did not intend to link the number of leaflets taken to participation in civic engagement projects. Instead, they might have merely expressed a determination to share the leaflets with their friends and family. That would mean that while they did not experience a change in their own interest in civic engagement projects, they nevertheless did experience a boost of confidence in influencing the behavior of others. While this shift might not be ideal from the hypothesis testing perspective, the spread of information about social projects might be considered a civic activity on its own. In fact, these actions imply a social dimension of civic engagement, because sharing the leaflet with friends and neighbors is a social activity while signing up on for something on their own is not.

In the real world, individuals in democratizing states are exposed to new civic opportunities on a daily basis. The results of the experiment suggest that this exposure results in long-term civic commitments if available opportunities for engagement are easily accessible and diverse. Citizens seem to be attracted by an easy, quick opportunity to engage, and
distracted by abstract activities that entail longer commitment of energy and time.

The experiment exhibits several methodological features. It is the first randomized controlled trial that targets civic culture in a post-Soviet country. The experiment takes place in seven locations in Southeast Ukraine, a region with high abstention rates and relatively passive civic culture (Bilousov 2002, Madin 2008, Pietrzyk-Reeves 2008, Gatskova & Gatskov 2015, Giuliano 2015, 2018). Southeast Ukraine’s long history of authoritarianism makes it a particularly stark example for the study of civic involvement (Hinckley 2009, Gatskova & Gatskov 2015, Ekman et al. 2016, Stewart & Dollbaum 2017, Kvartiuk & Curtiss 2019). To account for potential preference falsification, I measure both declared intentions and expressed actions. The behavioral response records respondents’ actual interest in learning about new community development projects and sharing this information with their friends and family. The attitudinal response measures respondents’ interest in joining an electoral commission, campaigning for recycling, establishing a civic council to improve bureaucratic accountability, and leading a homeowners’ association. The test is performed on a large and demographically diverse sample of adults (with an average age of 40.3 years and substantial variability in socio-economic status and education), thus allowing for the detection of various heterogeneous effects. Finally, I induce the sense of self- and collective efficacy by implementing a series of cognitive and behavioral interventions that involve personal or collective empowerment through self-reflection, civic education, and deliberation.

At the same time, future research might shed light on these findings by including a wider range of behavioral measures and providing a deeper qualitative assessment of the respondents’ perception of the questions about intentions. Also, it would be helpful to specifically target those individuals who would like to self-enroll in the experiment to improve their civic involvement or overcome activist burnout. The current intervention is unlikely to last long because subjects did not know what exactly they were recruited for (they were aware that the study was an academic experiment in political psychology and that they might be asked to complete certain cognitive tasks and exercises). Moreover, future studies might additionally identify the conditions under which respondents tend to overact (without declaring intentions) rather than overpromise (without delivering politi-
cally or civically valuable actions such as anti-corruption punishment (Boas et al. 2018)). Finally, it might be the case that the relative role of self- and collective efficacy vary across political domains and national cultures (Finkel 2003, 140).

6 Conclusion

This paper identifies the psychological drivers of civic engagement during democratic consolidation based on a cognitive experiment which randomly targets self- and collective efficacy of 1,381 subjects in Southeast Ukraine. The experiment was designed to improve subjects’ intention to participate in civic activities such as electoral observation, activism in a homeowners’ association, a pro-recycling waste management campaign, establishment of a civic council, and their actual engagement in spreading information about these civic opportunities.

The experiment delivers counterintuitive results that go against some theoretical expectations. The induction of efficacy does not unilaterally affect intentions and behavior towards civic engagement. While most subjects did not diverge from the control group in their declared intention to participate in civic activism, they _de facto_ expressed a stronger behavioral response. Similarly, against expectations, self-efficacy and collective efficacy did not dramatically differ in terms of their effect on civic engagement. The boundaries between self-efficacy and collective efficacy in the context of civic activism might be more blurred than we usually think.

These results provide a strong causal test of the theory of democratic learning and challenge those scholarly approaches that overemphasize the importance of declared principles and attitudes. The direct way to improve civic engagement (or prevent its erosion) is to ensure behavioral freedoms and allow people to exercise various civic opportunities rather than targeting beliefs and intentions about actions.

Ultimately, the main takeaway of this study is that self- and collective efficacies are interchangeable in the development of civic engagement. Collective efficacy is not the only way to boost civic involvement in weak democracies. Moreover, cognitive changes might not be happening directly after democratization, and individuals might respond to changes in a democratic environment with the update in actions rather than beliefs. The focus on
changing behavioral habits might be a better way to detect subtle institutional changes that are not yet visible in explicitly declared political preferences.
7 Appendix

7.1 Randomization

7.1.1 Subjects Recruitment

I recruited subjects in three ways. The first group, 733 subjects, was invited through random sampling on the streets. Those younger than 18 years were disqualified from participation. The second group, 334 subjects, contacted us after we circulated an announcement of the experiment in a listserv of local activists. The third group, 314 subjects, was recruited through Facebook ads targeted at adult citizens of Khersonska oblast. Table 2 shows the differences in pre-treatment civic engagement levels among the groups, confirming an oversample of activists and electoral observers in the second group. Active Facebook users are engaged less than activists but more than those sampled on streets. The wide variety of pre-treatment civic engagement confirms that the experiment does not test the hypotheses on civic engagement only on those who are already sufficiently engaged. Subjects from each group never met on the site of the experiment. Research assistants invited them to experimental labs with those recruited in the same way.
Table 2: Pre-Treatment Civic Engagement per Recruitment Group

<table>
<thead>
<tr>
<th>Statistic</th>
<th>N</th>
<th>Mean</th>
<th>St. Dev.</th>
<th>Min</th>
<th>Pctl(25)</th>
<th>Pctl(75)</th>
<th>Max</th>
</tr>
</thead>
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<td>Run for Political Office</td>
<td>1,381</td>
<td>0.161</td>
<td>0.367</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Random Sample</td>
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<td>0.156</td>
<td>0.363</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
</tr>
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<td>0.178</td>
<td>0.382</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Facebook Users</td>
<td>314</td>
<td>0.157</td>
<td>0.363</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Joined Electoral Commission</td>
<td>1,381</td>
<td>0.307</td>
<td>0.460</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
<td>1.000</td>
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<tr>
<td>Random Sample</td>
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<td>1.000</td>
<td>1.000</td>
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<tr>
<td>Activists Listserv</td>
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<td>0.000</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>Joined/Started Homeowners Association</td>
<td>1,381</td>
<td>0.150</td>
<td>0.356</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
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<tr>
<td>Joined Civil/Social Movement</td>
<td>1,381</td>
<td>0.449</td>
<td>0.497</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
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</tr>
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<td>1.000</td>
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<td>0.000</td>
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<td>Launched Civil/Social Movement</td>
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<td>1.000</td>
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</tr>
</tbody>
</table>

7.1.2 Organization

The experiment is approved by the Columbia University Institutional Review Board (IRB-AAAR8352). The research assistants were hired from two sources. Moderators were local psychologists or social workers, and research assistants were hired from the student pool of local psychology departments. I organized training sessions with moderators and research assistants before the experiment to strengthen their knowledge of local civic engagement initiatives and practice the delivery of the cognitive intervention.

The experimental laboratories were developed on the basis of computer classes in local schools and universities. Respondents were free to choose whichever language they preferred when working with questionnaires, paper handouts, and group discussions, and the design of the experiment accommodated their needs with the highest possible flexibility. Computer assistance on the spot was available for all respondents who had additional accommodation requests.
7.1.3 Randomization Procedure

Figure 10 describes the stages of the experiment. After subjects showed up at experimental locations, research assistants confirmed their age (18-34 or older than 35), detected their block, and assigned them to experimental conditions according to a block-specific randomization protocol. The protocols were designed to slightly oversample the subjects in the control group because I wanted to ensure an adequately powered statistical comparison between control and treatment conditions. After subjects were assigned to experimental groups, they were only staying in rooms with people with the same experimental condition.

No attrition happened in the experiment. The treatment assignment resulted in the reception of the treatment by all subjects.

All subjects are treated as compliers. There was no variation in the treatment ‘dose’ received by subjects. Subjects had personal ID cards with a masked group identifier during the experiment, ruling out the possibility of mistakenly treating a subject from the control condition or not administering the treatment on those in treated conditions.
7.1.4 Covariate Balance

Table 3 displays the summary statistics for five main covariates and the significance of F-statistics from covariate balance checks. Age and gender were used for block randomization. For these variables, the table shows simple means and standard deviations. Weighted means and weighted standard deviations are calculated for language (Ukrainian — 1, Russian — 0), education, and socio-economic status. Education is measured as an ordered categorical variable where 1 stands for primary school or below and 5 stands for a doctoral degree and postgraduate education. Socio-economic status is measured as an ordered categorical variable were 1 stands for being very poor (responded positively to “We have troubles buying food and basic household items”) and 4 stands for being wealthy (responded positively to “We can afford to buy whatever we want”).
The last column has the p-values of F-statistics which were extracted from regressions of treatment assignment on covariates. For each variable, I test if different levels of this covariate predict the likelihood of treatment assignment. The first model (Control) tests if the covariate predicts the assignment to any treatment condition as compared to the control group. For instance, the p-value of the test statistics from a linear model that regresses the assignment to any treatment on the fact of being a Ukrainian speaker is 0.871. This means that respondents’ language does not predict their assignment to either of the treatment groups compared to the control group. The remaining models (Self-Efficacy, Collective Efficacy, and Combined) regress the assignment to the corresponding treatment group as compared to control on the covariate of interest. For instance, the p-value of the test statistics from a linear model that regresses the assignment to Collective Efficacy group on the respondent’s educational attainment the 0.287. This means that respondents’ education does not predict their assignment to Collective Efficacy condition compared to the control group.

The covariate balance tests show that all p-values are higher than the 0.001 threshold recommended for covariate balance checks (Gerber & Green 2012, 432). The randomization did not result in covariate imbalance and the results of the experimental analysis are not jeopardized.
Table 3: Covariate Balance Check

<table>
<thead>
<tr>
<th>Statistic</th>
<th>N</th>
<th>Mean</th>
<th>S.D.</th>
<th>Min</th>
<th>Max</th>
<th>P &gt;</th>
<th>z</th>
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<td>40.584</td>
<td>15.628</td>
<td>18</td>
<td>81</td>
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<td>0.643</td>
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<td>16.365</td>
<td>18</td>
<td>81</td>
<td>0.583</td>
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</tr>
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<td>73</td>
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<table>
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<th>Weighted S.D.</th>
<th>Min</th>
<th>Max</th>
<th>P &gt;</th>
<th>z</th>
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</table>
7.1.5 Block Random Assignment

Since we assigned subjects to groups based on randomization protocols which were designed before each randomization occurred, we had to follow the procedure even when by chance some groups were ending up having fewer people from certain blocks. De-facto probability of assignment to treatment varies by block (from 0.199 to 0.295), and I account for this variation by using inverse probability weights in all model specifications (Gerber & Green 2012).
7.2 Experimental Intervention

7.2.1 Self-Efficacy

The treatment is delivered individually, on screen and on paper. Respondents receive paper handouts before the experiment begins. They read educational information on the screen and respond either electronically or by writing on the handouts. The excerpts of the intervention are given below.

Part 1. Internalizing locus of control

The part explains the concept of internal locus of control. It calibrates respondents' causal attribution to focus their mental energy on the things that they can change. First, respondents make themselves acquainted with a very simple explanation of the concept of locus of control. To better memorize the concept, they are prompted to read through a series of ubiquitous life situations and complete an interactive assignment.

Assignment excerpt:

Now, let’s practice a little bit. Imagine a young man who cannot find a job. Which of his circumstances are external? Which of his circumstances are internal and can be changed?

- Low demand for his profession
- Lack of education required for work
- Positions of interest are available only by an acquaintance and he has no powerful connections
- He does not have enough money to start his own business
- He has no work experience
- He is not happy with his choice of profession
- His family does not approve of his professional choice

Press “next” when you are ready to proceed.

Respondents independently reflect on the internal and external circumstances and classify the circumstances on the screen. Then they proceed to the next page and compare their answers to suggested responses.
The man cannot change external circumstances:
1. The behavior of other people (*all good places are available only by acquaintance*)
2. Attitudes of other people (*the family does not approve his professional choice*)
3. Objective constraints (*low demand for his profession*)

Instead, he can change internal circumstances:
1. *Lack of education required for work:* Professional retraining is often provided for free by large companies; there are also free online options available at websites such as Coursera (provides official certificates of completion) and on the YouTube channels of the world’s best universities.
2. *He does not have enough money to start his own business:* There are grant programs to support small and medium-sized businesses (run by the Ukrainian government, the European Union, or the local government program).
3. *He has no experience:* he can acquire it by signing up for an internship or taking advantage of the skillset that he already has.
4. *He does not like the profession he received in university:* he can work with a psychologist or by himself on the art of prioritization and self-discipline or work through the list of professions that appeal to him.

Respondents repeat the assignment with other examples to practice before proceeding to the next part.

**Part 2. Sorting out internal and external circumstances**

In this part, respondents identify the life concerns that they want to address and work on selecting feasible solutions within their locus of control.

Assignment excerpt:

Now, let’s classify external and internal circumstances for your own life goal that you specified above. In your handout, formulate the circumstances that affect the achievement of the goal. Write down all the possible solutions you can think of. Now, let’s circle out those solutions that you have control over. Which of them are based on external
circumstances? Which of them are internally controlled? The moderator can help you if you are not sure.

**Part 3. Feasible solutions and growth mindset**

In this part, respondents continue working with their handouts. They write down specific, feasible action plans to overcome the concerns and find feasible solutions.

Assignment excerpt:

Look at the list of internal circumstances. It is within your power to change them. On your handout, spell out at least one specific action you can undertake to change the internal circumstances.

Respondents work with their handouts with the assistance of moderators.

Assignment excerpt:

So, we realized that the key to solving any problem is focusing on internal processes that can be changed in the future. However, the future is unpredictable! What if some of our actions will not bring fruits? It is fine. Progress happens to those who make mistakes and learn from them. Errors are out of our control. Learning from them is within our control. In your handouts, refer to Table 3. Write down the challenges that you anticipate to encounter when performing your action plan. In the next column, specify how you will face those challenges and overcome them.

Respondents work with their handouts, envision potential obstacles and reflect on their response actions.

**Part 4. Self-efficacy induction**

In the final part, respondents think of the examples of successful problem-solving in their past and identify those character traits that helped them to overcome these problems. This mood induction exercise makes their self-efficacy more salient and restates their ability to
solve problems and face challenges. The textual analysis of the traits that the respondents used to boost their sense of self-efficacy is available in a separate paper (Soboleva 2019b).

Complete scripts are available for replication upon request.
7.2.2 Collective Efficacy

The treatment is delivered individually on-screen and collectively (via group discussion). First, respondents watch videos and respond to clarifying questions on the screen. Then, they proceed to group discussion. The excerpts of the intervention are given below.

**Part 1. Civic education**

In the first part, respondents watch educational videos about civic projects in their city. Each video states a civic problem, identifies feasible solutions, and demonstrates the example of collective efficacy.

The first video discusses the problem of waste management in Khersonska oblast (Figure 4a) and explains how civic activists are solving the problem by launching an innovative waste management system for recycling plastic and glass (Figure 4b).

The second video discusses the problem of energy efficiency (Figure 11a). Old buildings do not store heat well enough because of the lack of exterior thermal protection. The video shows how activists are solving the problem by making civil servants work on proper building maintenance (Figure 11b).

The third video discusses the transparency of public expenses, using the example of school budgeting. Parents have few tools of actual control over school expenses and funding priorities. Civic counseling of school budgets is a feasible way of exerting financial control and ensuring fiduciary transparency. The video shows how civic councils formed by the parents of school students solve the problem by creating a system of financial auditing available to all parents (Figure 12a).
The fourth video discusses the problem of dilapidated common property in residential buildings. In most post-Soviet countries, the public space ownership structures (the ownership of spaces such as basements, halls, and elevators) vary, and often building owners do not exert actual control over the way this space is used. As a result, public space is often not taken care of (Figure 13a). The video shows how homeowners organization formed by apartment building co-owners solve the problem by establishing OSBB association, initiating through the repair of public space, and – in line with a public decision – equipping a newly repaired basement with fitness gear(Figure 13b).

At the end of this part of the experiment, respondents respond to a number of questions that check on their comprehension of video materials.

Sample question:
What is this video about?

a. The role of local media in ecological campaigns
b. The energy saving program run by OSBB activists
c. Family mortgage available at discounted rate
d. The role of civil servants in crediting local business

Part 2. Spelling out feasible solutions

In the second part, respondents proceed to separate rooms to discuss the solutions presented in the videos and explore the potential ways of replicating these practices in their communities (Figure 14). The discussions are moderated and follow a script.

Figure 14: Brainstorming Sessions

Group Discussion Script

1. You have just watched videos about successful civic initiatives that were enacted by local people like you. Let’s discuss these success stories. How did they make you feel?
2. Which of these stories did you like most?
3. What made these initiatives so successful?
4. Which of the topics best suits the needs of your community?
5. What distinguishes these civic activists? How do they address their failures?
6. Suppose you have some time and energy to get involved in a civil project. What would it be?
7. If you need to choose one of the projects as a group, what would it be?
8. Together, you picked up a PROJECT. What could you do to make it work?
9. What kind of challenges you might encounter? How could you overcome them?
10. Whom would you identify as potential allies in the PROJECT? Where would you solicit the resources from?
11. What will help you to success? What are the strong sides of your community?

7.2.3 Combined Efficacy

The interventions were merged to induce a sense of self- and collective efficacy. First, the respondents went through self-efficacy induction. Then, they proceeded to collective efficacy induction.

Complete scripts of all interventions are available for replication upon request.
7.2.4 Instrument: Attitudinal Responses

The intentions to participate were measured in the following way:

1. If you had some free time, would you consider joining the activists who demand the improvement of waste management and recycling in Kherson?
   - 1 -- definitely not
   - 4 -- definitely yes

2. If you had some free time, would you consider joining electoral observers or becoming a member of electoral commission?
   - 1 -- definitely not
   - 4 -- definitely yes

3. If you had some free time, would you like to establish a civic council (Ukr.: *hromads'ku radu*)?
   - 1 -- definitely not
   - 4 -- definitely yes

4. Would you like to run for office as an OSBB (homeowners organization) leader?
   - 1 -- definitely not
   - 4 -- definitely yes

The direction of the scale randomized across individuals.
7.2.5 Instrument: Behavioral Responses

After the experiment was over, research assistants read the following script to each respondent: “Thank you so much for your participation. By the way, there are some leaflets on the table at the exit, they contain valuable information on civic engagement opportunities in Kherson. Please feel free to grab some for yourself and share them with your family and friends if you are interested. Have a nice day.”. The number of taken leaflets was discretely recorded by research assistants after the respondent left the laboratory. The leaflets looked as colored A4-sized dense paper sheets (Figure 15). The title page featured a bright word “Act” (Ukr: Diy) and invited subjects to improve their environment with civic action. The leaflets contained detailed information on specific community projects and featured a local contact number (hosted by one of the research assistants) for those who needed more information. The contact details are blurred for privacy concerns.

![Figure 15: Leaflets](image)

I do not directly ask respondents if they felt an increase in efficacy post-treatment. First, I would have to ask subjects from each experimental group about both self- and collective efficacy to keep the comparison of experimental outcomes consistent, and this would lead to an unnecessary contamination of the types of efficacy in first and second experimental groups. Second, asking people about their m not interested in the reported. Instead, the outcome I check the increase of efficacy based on the responses to cognitive intervention and based on individual’s activity during brainstorming sessions. This provides me with a good measure on the factual receipt of the treatment. At the same time, it does not give me any estimate of partial compliance to the treatment.
7.2.6 Manipulation Check

I restrain from asking direct questions about perceived self- and collective efficacy after administering the treatments for three reasons. First, an array of questions that directly asks about the individual perception of self- or collective efficacy right before measuring the outcomes would make the intervention too obvious for respondents. For cognitive treatments of this kind, manipulation check might be interpreted as just another form of treatment induction. Thus, it would make it harder to separate the effect of administering the treatments and the effect of administering subsequent manipulation check questionnaires.

Second, a difference-in-difference comparison would require administering self-efficacy questionnaires before and after the treatment induction. The pre-treatment assessment would have programmed the respondents, especially in the self-efficacy condition, to think about their efficacy before reflecting on their locus of control. This would jeopardize the administration of the treatment. Finally, for a sophisticated personalized treatment of this type, qualitative responses contain much more realistic and accurate assessments of self- and collective efficacy as compared to a generic self-efficacy questionnaire.

Thus, I assess if the treatments induced the sense of self- and collective efficacy based on qualitative responses that subjects provided at the end of treatment induction. For example, in the self-efficacy condition, respondents focused on the available ways to improve energy management in their neighborhood. While reflecting on these ways, they considered civil servants, bureaucrats, and energy providers as external circumstances beyond their control. In the collective efficacy condition, however, these actors were perceived as something that respondents are capable of controlling, because the induction of collective efficacy targets individual capabilities as members of communities, and thus improves their control over officials. Complete text-as-data analysis of these data is provided in a separate paper.
7.3 Treatment Outcomes

7.3.1 Outcomes Statistics

Table 4 contains the summary statistics of all experimental outcomes (simple mean, standard deviation, minimum, maximum, and the 25th and 75th percentiles).

<table>
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<th>Statistic</th>
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<th>St. Dev.</th>
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<td>3.580</td>
<td>5.024</td>
<td>0</td>
<td>1</td>
<td>3.6</td>
<td>40</td>
</tr>
<tr>
<td>Collective Efficacy</td>
<td>308</td>
<td>3.806</td>
<td>5.022</td>
<td>0</td>
<td>1</td>
<td>4</td>
<td>50</td>
</tr>
<tr>
<td>Combined Efficacy</td>
<td>331</td>
<td>3.834</td>
<td>6.662</td>
<td>0</td>
<td>1</td>
<td>4</td>
<td>50</td>
</tr>
</tbody>
</table>

Table 5 contains the confidence intervals for unadjusted models (with treatment assignment as the only predictor).

Table 6 contains the confidence intervals for adjusted models.

The power analysis shows that all intentional measures have a maximumly possible power (Table 7). The likelihood of receiving a false negative outcome is extremely low for these variables. The behavioral measures in the self- and collective efficacy groups are also powered enough. At the same time, the likelihood of making a Type II error is higher in the combined efficacy group (there is a 31% chance of not rejecting the null hypothesis when there is a significant effect). The reason behind the relatively underpowered result is an
Table 5: Bell-McCaffrey Confidence Intervals (Unadjusted Estimates)

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Self-Efficacy</th>
<th>Collective Efficacy</th>
<th>Combined Efficacy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leaflets Taken</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>Estimated ATE</em></td>
<td>0.757</td>
<td>1.066</td>
<td>1.049</td>
</tr>
<tr>
<td><em>Robust Standard Error</em></td>
<td>0.321</td>
<td>0.352</td>
<td>0.415</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>0.127</td>
<td>0.367</td>
<td>0.235</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>1.387</td>
<td>1.75</td>
<td>1.863</td>
</tr>
<tr>
<td>Establishing a Civic Council</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>Estimated ATE</em></td>
<td>0.01</td>
<td>0.024</td>
<td>0.012</td>
</tr>
<tr>
<td><em>Robust Standard Error</em></td>
<td>0.067</td>
<td>0.069</td>
<td>0.07</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.121</td>
<td>-0.111</td>
<td>-0.125</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.142</td>
<td>0.158</td>
<td>0.149</td>
</tr>
<tr>
<td>Joining an Electoral Commission</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>Estimated ATE</em></td>
<td>-0.14</td>
<td>-0.042</td>
<td>-0.109</td>
</tr>
<tr>
<td><em>Robust Standard Error</em></td>
<td>0.073</td>
<td>0.074</td>
<td>0.075</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.283</td>
<td>-0.187</td>
<td>-0.255</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.003</td>
<td>0.104</td>
<td>0.037</td>
</tr>
<tr>
<td>Leading a Homeowners Organization</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>Estimated ATE</em></td>
<td>-0.01</td>
<td>-0.042</td>
<td>-0.071</td>
</tr>
<tr>
<td><em>Robust Standard Error</em></td>
<td>0.082</td>
<td>0.085</td>
<td>0.082</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.171</td>
<td>-0.209</td>
<td>-0.233</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.15</td>
<td>0.125</td>
<td>0.09</td>
</tr>
<tr>
<td>Campaigning for Recycling</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><em>Estimated ATE</em></td>
<td>-0.031</td>
<td>-0.07</td>
<td>-0.078</td>
</tr>
<tr>
<td><em>Robust Standard Error</em></td>
<td>0.06</td>
<td>0.061</td>
<td>0.061</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.15</td>
<td>-0.189</td>
<td>-0.197</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.087</td>
<td>0.049</td>
<td>0.041</td>
</tr>
</tbody>
</table>

unexpectedly high spread of treatment outcomes in this group that I have not accounted for in my pre-treatment power analysis. However, given that the treatment effect was found in all other model specifications, the false-negative alternative is not an issue.
Table 6: Bell-McCaffrey Confidence Intervals (Covariate Adjustment)

<table>
<thead>
<tr>
<th>Statistic</th>
<th>Self-Efficacy</th>
<th>Collective Efficacy</th>
<th>Combined Efficacy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leaflets Taken</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimated ATE</td>
<td>0.774</td>
<td>0.992</td>
<td>1.056</td>
</tr>
<tr>
<td>Robust Standard Error</td>
<td>0.318</td>
<td>0.349</td>
<td>0.405</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>0.15</td>
<td>0.307</td>
<td>0.26</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>1.399</td>
<td>1.676</td>
<td>1.851</td>
</tr>
<tr>
<td>Establishing a Civic Council</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimated ATE</td>
<td>0.015</td>
<td>0.026</td>
<td>0.027</td>
</tr>
<tr>
<td>Robust Standard Error</td>
<td>0.066</td>
<td>0.068</td>
<td>0.069</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.115</td>
<td>-0.108</td>
<td>-0.109</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.145</td>
<td>0.16</td>
<td>0.163</td>
</tr>
<tr>
<td>Joining an Electoral Commission</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimated ATE</td>
<td>-0.137</td>
<td>-0.039</td>
<td>-0.109</td>
</tr>
<tr>
<td>Robust Standard Error</td>
<td>0.072</td>
<td>0.073</td>
<td>0.073</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.278</td>
<td>-0.183</td>
<td>-0.253</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.065</td>
<td>0.104</td>
<td>0.035</td>
</tr>
<tr>
<td>Leading a Homeowners Organization</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimated ATE</td>
<td>-0.007</td>
<td>-0.033</td>
<td>-0.056</td>
</tr>
<tr>
<td>Robust Standard Error</td>
<td>0.081</td>
<td>0.085</td>
<td>0.082</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.167</td>
<td>-0.2</td>
<td>-0.218</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.152</td>
<td>0.134</td>
<td>0.106</td>
</tr>
<tr>
<td>Campaigning for Recycling</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimated ATE</td>
<td>-0.031</td>
<td>-0.07</td>
<td>-0.081</td>
</tr>
<tr>
<td>Robust Standard Error</td>
<td>0.06</td>
<td>0.06</td>
<td>0.06</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (lower)</td>
<td>-0.148</td>
<td>-0.187</td>
<td>-0.199</td>
</tr>
<tr>
<td>Bell-McCaffrey confidence interval (upper)</td>
<td>0.087</td>
<td>0.047</td>
<td>0.037</td>
</tr>
</tbody>
</table>

Table 7: Power Calculations

<table>
<thead>
<tr>
<th>Comparison</th>
<th>Recycling Campaign</th>
<th>Civic Council</th>
<th>Electoral Observation</th>
<th>Homeowners Organization</th>
<th>Leaflets Taken</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control vs. Self-Efficacy</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>0.867</td>
</tr>
<tr>
<td>Control vs. Collective Efficacy</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>0.855</td>
</tr>
<tr>
<td>Control vs. Combined Efficacy</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>1.000</td>
<td>0.684</td>
</tr>
</tbody>
</table>

55
## 7.3.2 Tabular Forms of Main Results

The tables below show the results of the linear regression models which were used to produce the graphs in the main texts. Table 9 shows the results for unadjusted models (without covariates). Table 8 shows the outcomes of covariate adjusted models. Covariates include gender, language, education, age, and socioeconomic status.

### Table 8: Average Treatment Effects

<table>
<thead>
<tr>
<th></th>
<th>Leaflets Taken</th>
<th>Electoral Observation</th>
<th>Civic Council</th>
<th>Recycling Campaign</th>
<th>Homeowners Organization</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Self-Efficacy</strong></td>
<td>0.774**</td>
<td>-0.137**</td>
<td>0.015</td>
<td>-0.031</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.318)</td>
<td>(0.072)</td>
<td>(0.066)</td>
<td>(0.060)</td>
<td>(0.081)</td>
</tr>
<tr>
<td><strong>Collective Efficacy</strong></td>
<td>0.992***</td>
<td>-0.039</td>
<td>0.026</td>
<td>-0.070</td>
<td>-0.033</td>
</tr>
<tr>
<td></td>
<td>(0.349)</td>
<td>(0.073)</td>
<td>(0.068)</td>
<td>(0.060)</td>
<td>(0.085)</td>
</tr>
<tr>
<td><strong>Combined</strong></td>
<td>1.056***</td>
<td>-0.109</td>
<td>0.027</td>
<td>-0.081</td>
<td>-0.056</td>
</tr>
<tr>
<td></td>
<td>(0.405)</td>
<td>(0.073)</td>
<td>(0.069)</td>
<td>(0.060)</td>
<td>(0.082)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>2.812***</td>
<td>2.767***</td>
<td>2.430***</td>
<td>2.990***</td>
<td>2.116***</td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.050)</td>
<td>(0.046)</td>
<td>(0.041)</td>
<td>(0.056)</td>
</tr>
<tr>
<td><strong>Covariates</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>2.809</td>
<td>2.774</td>
<td>2.437</td>
<td>2.992</td>
<td>2.121</td>
</tr>
<tr>
<td><strong>Outcome Range</strong></td>
<td>[0.50]</td>
<td>[1.4]</td>
<td>[1.4]</td>
<td>[1.4]</td>
<td>[1.4]</td>
</tr>
</tbody>
</table>

*\( p < .1; \) **\( p < .05; \) ***\( p < .01 \)

Robust standard errors are in parentheses. Covariates include gender, language, education, age, and socioeconomic status.

### Table 9: Average Treatment Effects

<table>
<thead>
<tr>
<th></th>
<th>Leaflets Taken</th>
<th>Electoral Observation</th>
<th>Civic Council</th>
<th>Recycling Campaign</th>
<th>Homeowners Organization</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Self-Efficacy</strong></td>
<td>0.757**</td>
<td>-0.140*</td>
<td>0.010</td>
<td>-0.031</td>
<td>-0.010</td>
</tr>
<tr>
<td></td>
<td>(0.321)</td>
<td>(0.073)</td>
<td>(0.067)</td>
<td>(0.060)</td>
<td>(0.082)</td>
</tr>
<tr>
<td><strong>Collective Efficacy</strong></td>
<td>1.058***</td>
<td>-0.042</td>
<td>0.024</td>
<td>-0.070</td>
<td>-0.042</td>
</tr>
<tr>
<td></td>
<td>(0.352)</td>
<td>(0.074)</td>
<td>(0.069)</td>
<td>(0.061)</td>
<td>(0.085)</td>
</tr>
<tr>
<td><strong>Combined</strong></td>
<td>1.049**</td>
<td>-0.109</td>
<td>0.012</td>
<td>-0.078</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td>(0.415)</td>
<td>(0.074)</td>
<td>(0.070)</td>
<td>(0.061)</td>
<td>(0.082)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>2.804***</td>
<td>2.768***</td>
<td>2.435***</td>
<td>2.989***</td>
<td>2.123***</td>
</tr>
<tr>
<td></td>
<td>(0.183)</td>
<td>(0.051)</td>
<td>(0.047)</td>
<td>(0.041)</td>
<td>(0.053)</td>
</tr>
<tr>
<td><strong>Covariates</strong></td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>2.809</td>
<td>2.774</td>
<td>2.437</td>
<td>2.992</td>
<td>2.121</td>
</tr>
<tr>
<td><strong>Outcome Range</strong></td>
<td>[0.50]</td>
<td>[1.4]</td>
<td>[1.4]</td>
<td>[1.4]</td>
<td>[1.4]</td>
</tr>
</tbody>
</table>

*\( p < .1; \) **\( p < .05; \) ***\( p < .01 \)

Robust standard errors are in parentheses. No covariates included.
Table 10: Time as Predictor of Behavioral Response (Control Group)

<table>
<thead>
<tr>
<th></th>
<th>Leaflets Taken</th>
</tr>
</thead>
<tbody>
<tr>
<td>Second Quartile</td>
<td>$-1.149^{**}$</td>
</tr>
<tr>
<td></td>
<td>(0.460)</td>
</tr>
<tr>
<td>Third Quartile</td>
<td>$-1.402^{**}$</td>
</tr>
<tr>
<td></td>
<td>(0.574)</td>
</tr>
<tr>
<td>Fourth Quartile</td>
<td>$-1.476^{***}$</td>
</tr>
<tr>
<td></td>
<td>(0.526)</td>
</tr>
<tr>
<td>Constant</td>
<td>$3.470^{***}$</td>
</tr>
<tr>
<td></td>
<td>(0.259)</td>
</tr>
<tr>
<td>N</td>
<td>389</td>
</tr>
<tr>
<td>F Statistic</td>
<td>4.475^{***}</td>
</tr>
<tr>
<td></td>
<td>(df = 3; 385)</td>
</tr>
</tbody>
</table>

$^{*}p < .1;^{**}p < .05;^{***}p < .01$

7.3.3 Intention-Behavior Inconsistency

To uncover the reasons behind the inconsistency, I explore the general pattern of intention-behavior inconsistency at the level of experimental groups. In a strict sense, an intention-consistent behavior would be to take any amount of leaflets if declared any interest in any civic project. Table 11 explores the patterns of intention-behavior consistency defined in this way. Most subjects demonstrate intention-behavioral consistency: they expressed an interest in at least one project and took at least one leaflet (88.92%). The second group, however, shows behavior-intention inconsistency: they expressed no interest in any civic project but took at least one leaflet (9.34%). The rest are minor groups of cheap-talkers who expressed an intention to participate in at least one project but took no leaflet (1.38%) and quitters who expressed no interest in civic projects either verbally or non-verbally (0.36%).

Table 11: Intention-Behavior Distribution

<table>
<thead>
<tr>
<th>Action</th>
<th>No Action</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intention</td>
<td>Activists (Consistent): 88.92%</td>
</tr>
<tr>
<td>No Intention</td>
<td>Cheap-talkers (Inconsistent): 1.38%</td>
</tr>
<tr>
<td>Makers (Inconsistent): 9.34%</td>
<td>Quitters (Consistent): 0.36%</td>
</tr>
</tbody>
</table>

The experimental conditions do not predict the ratio of inconsistent and consistent subjects. As Table 12 shows, the distribution of activists, makers, cheap-talkers, and quitters, is extremely uniform across all experimental conditions. The treatment did not change the intention-behavior consistency defined in a broad way.
Table 12: Intention-Behavior Inconsistency

<table>
<thead>
<tr>
<th>Variables</th>
<th>Cheap-Talkers</th>
<th>Activists</th>
<th>Makers</th>
<th>Quitters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self-Efficacy</td>
<td>−0.009</td>
<td>0.008</td>
<td>0.003</td>
<td>−0.003</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.023)</td>
<td>(0.022)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Collective Efficacy</td>
<td>−0.008</td>
<td>0.003</td>
<td>0.001</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.025)</td>
<td>(0.023)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Combined</td>
<td>−0.012</td>
<td>0.015</td>
<td>−0.007</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.023)</td>
<td>(0.021)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.021***</td>
<td>0.882***</td>
<td>0.094***</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.016)</td>
<td>(0.015)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Covariates</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Robust standard errors are in parentheses. Covariates include gender, language, education, age, and socioeconomic status.

7.3.4 Summary of Randomization Inference Trials

I sum up the results of the Studentized permutation tests that compare the t-statistic of the ATE with its empirical distribution under random reassignments of treatment (Lin et al. 2016, Chung & Romano 2013). The results of 10,000 simulations show that the two-tailed p-value for the ATE of the combined efficacy treatment, collective efficacy treatment, and self-efficacy treatment are all within the significance level of 0.05 (16). Of all attitudinal measures, the only marginally significant ATE is the one for electoral commission observation – however, this effect is not significant in alternative model specifications.
To test the sharp null hypotheses on no treatment effect on any unit, I generate the sampling distribution of the test statistic under the null hypothesis by simulating 10,000 random assignments for each treatment. P-values are calculated by comparing the observed test statistic to the distribution of test statistics under the null hypothesis of no treatment effect (Gerber & Green 2012). Figure 17 summarizes the results of 15 trials (five outcome variables, three experimental conditions). Simulating each random assignment 10,000 times, I find that for behavioral outcomes, the generated average treatment effects are larger than the observed average treatment effects in less than 5% of simulations (that is, the two-tailed p-values of the observed average treatment effects are less than 0.05). However, for all attitudinal outcomes, the observed average treatment effects are not different from most of the random assignments generated under the null hypothesis of no effect.

These results confirm the original findings on the positive effects of all treatments detected in behavioral outcomes but challenge the significance of the negative effect of the treatments on the expressed intention to join electoral commissions. We cannot reject the sharp null hypothesis on no effect on any unit for the expressed intentions to engage (‘attitudinal responses’).

Figure 18 shows the distributed treatment outcomes based on 10,000 simulations per experimental group within four randomization blocks. Three distributions represent three
Figure 18: Behavioral Response

differences in means: self-efficacy condition and control; collective efficacy and control; and combined treatment and control. To detect the p-value under the sharp null hypothesis, we look at the values of observed ATE as compared to the generated distribution of simulated ATEs under the hypothesis of no treatment effect on any unit. The visualization demonstrates the strong positive effect obtained under all conditions on the actual behavior of respondents. Even though the average treatment effect of the self-efficacy treatment is lower than the ones of other groups, we cannot confidently distinguish the treatment effects because their confidence intervals largely overlap.

Figure 19 and Figure 20 show the results of the randomization inference tests for the responses that capture the intention to engage. Figure 19 features the outcomes that are measured on a four-point Likert scale. For each variable, 1 stands for “definitely do not intend to act” and 4 stands for “definitely intend to act”. Figure 20 has the outcomes measured as binary variables (1 — intends to act, 0 – otherwise). The binary way of measuring the outcomes allows detecting the shift between present and absent intentions to engage rather than a gradual change. As the graphs suggest, we cannot reject the hypotheses of null effects on any unit.
Figure 19: Randomization Inference: Reported Intention to Participate (Likert Scale)
ATE(SE) = 0.0012
ATE(CE) = −0.019
ATE(C) = −0.027

Figure 20: Randomization Inference: Reported Intention to Participate (Binary Scale)
7.3.5 Added Variable Plots

The added variable plots visualize the treatment effects controlled for covariates. The plots show the relationships between the residuals from the regression of treatment outcomes on the covariates and the residuals from the regression of treatment assignment on the covariates. The plots below show the relationships for three experimental conditions and leaflets taken (Figure 21) or the intentions to engage in civic activities (Figures 22, 23, 24, 25), controlling for participants age, gender, education, socio-economic status, and language. The results of covariate control show that the behavioral responses still reflect a positive change, while the results for the verbal ‘attitudinal’ responses are not dramatically different from alternative specifications.
Covariates: age, gender, education, socio–economic status, language

Figure 22: Added Variable Plots: Electoral Observation

Figure 23: Added Variable Plots: Civic Council
Figure 24: Added Variable Plots: Recycling

Figure 25: Added Variable Plots: Homeowners Association
Predicted Behavioral Treatment Effects with Confidence Intervals

Figure 26: Generalized Random Forests: Civic Engagement

7.3.6 Heterogeneous Effects: Generalized Random Forests
Figure 27: Generalized Random Forests: Recycling

Figure 28: Generalized Random Forests: Electoral Commission
Figure 29: Generalized Random Forests: Civic Council

Figure 30: Generalized Random Forests: Homeowners Association
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