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Mobilization, participation, and American democracy: A retrospective and postscript

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Abstract

Rosenstone and Hansen's (1993) treatise on the mobilizing effects of campaign activity and social interactions marks a turning point in scholarship on political participation. This essay looks back at this pivotal work and the experimental research agenda that emerged from it. Decades of subsequent research have reinforced and extended the book's key claims about the role of mobilization in promoting voter turnout. Those who study elections now have a much clearer sense of which types of mobilization activities are effective and which types of voters respond to them.

Keywords

field experiments, political mobilization, political participation, voter turnout

The publication of Stephen J Rosenstone and John Mark Hansen's *Mobilization, Participation, and Democracy in America* in 1993 marked an important turning point in the study of political participation. Like many books on the subject of American political participation dating back to Gosnell (1927), including such classics as Verba and Nie (1972), Rosenstone and Hansen frame their discussion by noting that patterns of political participation tend to accentuate existing class differences. By comparison to people with less money or education, the affluent and well-educated are more prone to follow politics and to engage in behaviors ranging from voting to contacting public officials. But in contrast to an earlier generation of books, most notably Wolfinger and Rosenstone's (1980) *Who Votes?*, Rosenstone and Hansen's *Mobilization* focuses not on the background attributes that predict whether citizens participate in politics but instead on the interpersonal influences that induce people to take action. Defining mobilization quite broadly to include both campaign appeals (e.g. phone calls from a party or organization urging one to vote in an upcoming election) as well as informal conversations with friends and family that might pique interest in politics, Rosenstone and Hansen argue that the nature and frequency of such interactions helps explain why participation rates vary across time and place.

In some ways, *Mobilization* reflects the zeitgeist of its era insofar as it argues that declines in voter turnout over time largely reflect declining mobilization activity on the part of campaigns and informal social networks. A similar argument was a centerpiece of the influential *Bowling Alone* (Putnam 1995, 2000), which charted the long-term decline in all forms of civic and political engagement. However, Rosenstone and Hansen's statistical claim that at least half the decline in voter turnout since the 1960s could be attributed to a decline in mobilization activity drew criticism from several directions. Using American National Election Studies data, Abramson et al. (2002) questioned whether the level of reported mobilization activity had in fact declined appreciably in presidential election years. McDonald and Popkin (2001) questioned whether the decline in turnout since the 1960s was appreciable after accounting for the growing share of the American adult population that is ineligible to vote, due to disenfranchisement in the wake of felony convictions or simple lack of US citizenship. Given the post-2000 rebound in voter turnout, the 1990s' fascination with turnout declines seems misplaced, but as we point out below, the subsequent

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rise in turnout is broadly consistent with *Mobilization*'s thesis that the volume of campaign activity affects the aggregate rate of turnout.

More important than the specific claim that trends in mobilization activities explain trends in voter turnout is the broader theoretical argument that strategic politicians target their mobilization efforts in ways that are designed to maximize electoral returns. During presidential elections, for example, campaigns saturate closely contested "battleground" states in an effort to win electoral votes while largely ignoring other states whose partisan coloration makes their results a foregone conclusion. At the group level, strategic calculations cause campaigns to mobilize groups like African Americans or conservative Christians, whose lopsided partisan proclivities make them bankable votes for Democrats or Republicans, respectively. Meanwhile, groups like Asian Americans, whose partisan preferences are less clear cut, are excluded from campaign mobilization because, as a group, they tend not to produce reliable votes for one party or the other (Wong et al. 2011). When targeting individuals, campaigns and politicians have an incentive to focus their efforts on those with extensive social ties, such as civic, religious, or union leaders.

This thesis about the strategic calculus of campaigns proved to be prescient. In the years since the publication of *Mobilization*, political campaigns have become increasingly adept at managing databases and directing communication to specific voters. Technological innovations combined with the rapidly expanding commercial market for consumer data have made it possible for campaigns to forecast which voters are likely supporters. Armed with this information, campaigns have strong incentives to mobilize supporters who would otherwise not vote. Whereas Republican campaigns might have formerly ignored Republican voters from heavily Democratic precincts, they now make efforts to target these Republicans but not their neighbors (Franz, 2013; Issenberg, 2012). *Mobilization* anticipated the advent of what became known as "microtargeting," or the tailoring of specific messages to potential voters.

Rosenstone and Hansen's normative message that mobilization is, on the whole, a good thing stems from the authors' observation that the socio-economic skew associated with widespread behaviors, such as voting, is less severe than for comparatively rare behaviors, such as contacting public officials. This rather optimistic assessment has drawn fire from those who argue that policies that lower the costs of voting have, ironically, been most successful at increasing turnout among relatively high-propensity voters (Berinsky, 2005), although this finding has been disputed in the case of voting centers and all-mail elections (Gerber et al., 2013; Stein and Vonnahme, 2008). There is evidence that mobilization drives, too, tend to be most effective among those at the upper end of the voter turnout spectrum, at least in low-salience elections (Enos et al., 2014). Although many scholars have called for greater efforts to mobilize those at the bottom of the socioeconomic ladder, they often lament Rosenstone and Hansen's broader point that campaigns have strategic incentives to focus their efforts on segments of the electorate that look much like those who already participate (García Bedolla and Michelson, 2012).

More influential than its normative argument has been the book's two central claims about micro-foundations, namely, that political participation is profoundly affected by (1) mobilization efforts directed by campaigns and (2) mobilizing interactions within social networks. The remainder of this essay provides an intellectual postscript on these two hypotheses, which have inspired a vast research literature investigating the causes of voter turnout both in the United States and abroad.

Like many researchers before them, Rosenstone and Hansen assessed the causal influence of campaign and impersonal mobilization by analyzing survey data. Specifically, they pooled decades of American National Election Studies surveys and performed a regression analysis predicting participation (e.g. self-reported voter turnout) based on self-reported exposure to mobilizing activities (e.g. encountering canvassers or receiving phone calls from campaigns) as predictors. They find a strong and statistically significant pattern of correlations between mobilization and participation, even after controlling for respondents' background attributes, such as age or ethnicity. Although one might quibble with the particular way in which these regression analyses were conducted, the results are very much in line with other authors' results using similar data (Caldeira et al., 1985; Cox and Munger, 1989; Huckfeldt and Sprague, 1992). Rosenstone and Hansen interpret these correlations to imply that campaign contact and interpersonal conversations about politics have a causal effect on voting:

> Mobilization, in all its forms, causes people to take part in electoral politics. Citizens who are contacted by political parties, exposed to intensely fought electoral campaigns, or inspired by the actions of social movements are more likely to vote, to persuade, to campaign, and to give. (Rosenstone and Hansen, 1993: 209–210)

Because intuition suggests that both forms of mobilization *do* affect turnout, these statistical findings and interpretations seem unobjectionable. Moreover, the authors furnish a convincing theoretical explanation for why mobilization matters: it both lowers the cost of voting by alerting citizens that an election is coming and creates social incentives for political participation (Rosenstone and Hansen 1993: 175–176). Given the plausibility of the empirical claims and accompanying theoretical arguments, it may seem surprising that *Mobilization* launched a cottage industry of research investigating these micro-foundations.

As it happened, *Mobilization* appeared at a time of ferment in the social sciences. A so-called "credibility revolution" (Angrist and Pischke, 2010) was afoot, calling for experimental research designs that could convincingly demonstrate cause and effect. The hallmark of experimental social science is the random assignment of subjects to treatment and control conditions. Random assignment implies that those who receive the treatment have the same expected attributes as those who do not. An experiment that randomly exposed some subjects to mobilization activities could convincingly assess the causal effect of those activities on subsequent political conduct. By contrast, non-experimental research (also known as observational research) relies on untestable assumptions about the comparability of those who do or do not experience mobilization activity. Such assumptions might be right or wrong; the concern is that even plausible conclusions remain heavily dependent on these assumptions rather than on an underlying experimental design.

With regard to the evidence presented in *Mobilization*, critics raised three concerns (cf. Gerber and Green, 2000). First, the use of observational data makes causal inferences vulnerable to unobserved confounders. If campaigns target their mobilization efforts at likely voters, the relationship between voter turnout and exposure to mobilization activities may be spurious. A skeptic might conjecture that mobilization activity has no effect on turnout; the apparent correlation bewteen the two occurs because mobilization activity tends to be directed at those who have an elevated probability of voting. Ironically, this criticism follows from the thesis that campaigns are strategic in the way in which they target voters, directing their persuasive appeals to relatively high-propensity voters.

Second, even in the absence of strategic targeting by campaigns, the correlation between mobilization and turnout could be spurious. If those whose lifestyles, work schedules, and accessibility to others make them more easily contacted by campaigns are more likely to vote than their less accessible counterparts, contact with campaigns could be correlated with voting for reasons that may have nothing to do with its causal influence. In effect, contact with campaigns is just a marker for unmeasured personal attributes that predict voting. In the years since *Mobilization* appeared, this point has been demonstrated repeatedly using experimental data. When one randomly assigns voters to receive phone calls – even calls about topics such as wearing seat belts or donating blood – those who answer the phone are much more likely to vote than those who do not (Arceneaux et al., 2010; Gerber and Green, 2005).

Third, survey data are susceptible to measurement error. Most of the ANES surveys measure turnout using self-reports, as opposed to validating turnout using public records. Perhaps more importantly, exposure to campaign activity and interpersonal mobilization rely on respondents' recollections. For example, the question that Rosenstone and Hansen use to measure campaign contact reads "As you know, the political parties try to talk to as many people as they can to get them to vote for their candidates. Did anyone from one of the political parties call you up or come around and talk to you about the campaign?" (p. 162). Interpersonal mobilization is not directly measured but is based on questions about social involvement that ask about length of community residence, church attendance, home ownership, and employment (pp. 157–159). Notice that these questions lack specificity. They do not refer to a specific form of contact. They are vague about when the contact took place. The response options ignore the frequency with which the specified contact occurred. Furthermore, recall measures of this sort are notoriously susceptible to misreporting (Nickerson, 2005; 22–23; Prior, 2012; Vavreck, 2007). If people who represent themselves as voters also represent themselves as the recipients of campaign attention, the relationship between contact and voting may be exaggerated.

A quite different way of assessing the causal effects of mobilization activities is to conduct experiments in field settings. This tradition dates back to the 1920s, when Harold Gosnell (1927) distributed mailings encouraging Chicago residents to register and vote. In the 1950s, Samuel Eldersveld (1956) added random assignment to this elegant research design and expanded the range of mobilizing treatments to include door-to-door canvassing and phone calls. A few such experiments were conducted in the decades that followed (Adams and Smith, 1980; Miller et al., 1981), but this style of research had become moribund by the 1990s, when Gerber and Green (2000) launched their experimental study of canvassing, commercial phone banks, and direct mail. Like Gosnell's early work, the Gerber and Green study was limited to a single location (New Haven), but replications applied similar designs to a long list of elections in other locations (García Bedolla and Michelson 2012; Green and Gerber 2008; Green et al., 2003), including several outside the United States (Guan and Green, 2006; John and Brannan, 2008; Wantchekon, 2003).

What has been learned from the hundreds of experiments testing the effectiveness of campaign tactics? How do the conclusions from this literature compare to the findings presented in *Mobilization*? The 30,000 foot overview of the experimental literature is that campaign tactics often do mobilize voters, a conclusion that squares nicely with *Mobilization*'s core argument. A substantial number of large, well-executed studies have found that encouragements to vote significantly increase turnout (see Green et al., 2013 for a recent meta-analysis). At the same time, the experimental literature is also replete with examples of tactics that do not work, or at least do not work well enough to have detectable effects. For example, prerecorded phone calls have negligible effects on turnout, even when statistical allowances are made for the fact that only some people actually pick up the phone when called

(Shaw et al., 2012). The same goes for email encouragements to vote; they seem to have negligible effects (Nickerson, 2007a) even when they are sent by a political party to its activists (Stollwerk, 2006). Sample ballots and voting guides seem to do little to increase turnout (García Bedolla and Michelson, 2009); weak effects are also found for direct mail that encourages voting for specific candidates or ballot measures (Gerber et al., 2003) or that simply reminds people about an upcoming election (Panagopoulos, 2011). Here, the nuances of the experimental literature go beyond the arguments advanced by Rosenstone and Hansen. Recall that the mechanisms by which mobilization is said to increase turnout include lowering the cost of information about how and when to participate. Evidently, the story is more complicated than this theoretical account suggests, as turnout fails to rise when people are reminded to vote and/or provided pertinent information about the electoral choices before them.

One interpretation of the litany of experimental findings is that the quality of interaction with voters matters. Given that a targeted voter can be reached successfully, authentic personal interaction seems to be more effective than scripted messages. For example, phone banks that get their callers to slow down and have a conversation with voters produce larger turnout effects than routinized calls from commercial phone banks (García Bedolla and Michelson, 2012; Nickerson, 2007b). Another conclusion that seems to emerge from the experimental literature is that social norms are highly influential. Messages conveyed by direct mail or pre-recorded phone calls – tactics that usually produce null findings – can be highly influential when they forcefully assert the importance of doing one's civic duty and remind voters that who votes is a matter of public record (Gerber et al., 2008; 2010a, 2010b). In some ways, these results lend credence to Rosenstone and Hansen's argument that interpersonal conversations stimulate turnout. Although they ascribe this effect to social incentives that arise from social involvement, another interpretation is that conversations matter because they reinforce prescriptive social norms that characterize voting as a valued civic obligation.

As the research focus shifts from campaign tactics to interpersonal influence, the experimental literature becomes thinner, largely because randomized studies become more challenging to implement. The two most prominent experiments conducted to date offer somewhat different conclusions. Nickerson's (2008) study involved door-to-door canvassing. Upon opening the door to canvassers, voters were randomly encouraged either to vote or to recycle. Nickerson found that, as expected, those who received the voting encouragement were significantly more likely to vote; more importantly, he found that the housemates of those who opened their doors to canvassers were also more likely to vote if others in the household received the voting message. This finding suggests that intra-household communication spreads the effects of the canvasser's message. On the other hand, Sinclair et al.'s (2012) multi-level experiment, which varied the density with which selected zip codes were saturated with get-out-the-vote treatments, found borderline significant intra-household contagion effects and no evidence whatsoever of effects passed from neighbor to neighbor. Although these experiments offer a much firmer foundation for causal inference than survey-based correlations between turnout and the amount of interpersonal conversation that occurs among friends, more work is needed before we gain a clear sense of how much interpersonal influence affects turnout within social networks. Two promising approaches are the study of transmission within friendship networks on social media platforms such as Facebook (Bond et al., 2012) or among family and friends who respond to surveys that use snowball sampling.

What, then, has the proliferation of experiments added to the original insights of *Mobilization*? A skeptic might say that a vast stock of experimental evidence merely tells us what we already knew based on decades of observational research: campaign tactics mobilize voters, as does interpersonal communication within social networks. Certainly, Rosenstone and Hansen deserve credit for anticipating many of the findings that followed their path-breaking book. At the same time, this skeptical assessment ignores the precision that field experiments have brought to our knowledge about mobilization. Prior to the advent of experimentation, survey evidence suggested that mobilization matters, but a great deal of uncertainty surrounded this conclusion, reflecting the inherent methodological uncertainty associated with non-experimental inference. Randomized experiments have reinforced these conclusions and made them much more credible.

In addition, experiments have contributed a number of important stubborn facts with which theories must now grapple. Not all mobilization tactics produce effects. Indeed, many of the most common campaign tactics – direct mail, phone calls from commercial phone banks, presidential television ads – seem to produce weak effects (Green and Gerber, 2008; Krasno and Green, 2008). The fact that ineffective tactics are widely used is sometimes attributed to their (rarely measured) persuasive effects (Gerber, 2004; Rogers and Middleton, 2015). Another explanation has to do with a layer of strategic behavior that receives little attention in *Mobilization*: campaign consultants have a business interest in deploying these kinds of tactics, and the fact that no one knows for sure whether such tactics generate votes allows sub-optimal campaign tactics to persist (Green and Smith, 2003).

Experiments also provide a useful sense of proportion. Prior to the accumulation of experimental evidence on the mobilizing effects of campaign activities, it was anyone's guess as to how many dollars were required to generate an additional vote (Gerber, 2004). Indeed, political scientists

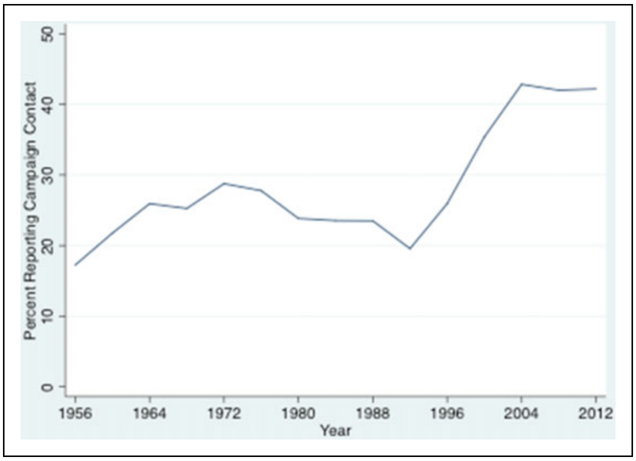

Figure 1. Percent of respondents reporting campaign contact in presidential election years.

Source: ANES Time Series Cumulative Data File, 1956-2012.

Question: "The political parties try to talk to as many people as they can to get them to vote for their candidate(s). Did anyone from one (1956, 1960, 1964, 1966, 1968: either) of the political parties call you up or come around and talk to you about the (1956, 1960, 1964, 1966, 1968: during the) campaign (1976: this year)?

sometimes made outlandish claims about the effects of interventions such as voter guides based on dubious observational evidence (Macedo et al., 2005; Wolfinger et al., 2005). We now have a sense that a 5 percentage point increase in turnout is a quite large effect and that the 7–10 percentage point effects reported in Rosenstone and Hansen (1993: 171–172) are truly extraordinary and may even be implausibly large. Experimental studies of other forms of mobilization, such as rallies (McClendon, 2014), membership drives (Han, 2014: 148–149), and fundraising (Green et al., 2015; Levine, 2015: 152–154; Miller and Krosnick, 2015), have also brought a needed sense of proportion to the treatment effects that might be expected from these activities.

In conclusion, let us return to the question of whether mobilization activity has contributed to over-time patterns of turnout in the United States. Obviously, this question cannot be answered with the rigor of a controlled experiment, but it is interesting to return to the question that helped inspire Rosenstone and Hansen's work. The recent work of Enos and Fowler (2014) offers intriguing evidence suggesting that as states move from non-battleground to battleground in successive presidential elections, they experience a profound increase in turnout, especially since 2000, when presidential campaigns began to place unprecedented effort into voter mobilization tactics (Figure 1 shows the rate of reported campaign contact during presidential election years over time). In an effort to differentiate the cumulative effects of ground-level mobilization from other effects, such as the saturation of television advertising, Enos and Fowler demonstrate that counties in non-battleground states that receive extensive advertising from battleground media markets experience no appreciable increase in turnout. These findings suggest that, if anything, Rosenstone and Hansen were more correct about the cumulative effects of mobilization activity than their own evidence let on. The increasing sophistication with which campaigns target and mobilize voters and their social networks (see Bond et al., 2012; Nickerson and Rogers, 2014) has amplified strategic politicians' influence on aggregate participation rates.

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