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The Formation of Voting Habits

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Abstract

Rather than adhering to the cost-benefit calculus often employed in the study of turnout, a recent stream in this literature refers to voting as habit forming. The empirical findings supporting this developmental approach are still scarce, however. Using voting-age discontinuities among almost equally aged individuals, this study enables the identification of the effect of voting in one election on turnout in future elections. Tracing individuals for more than 30 years of their lives, the long-term effect of early voting experiences on people's turnout profiles is also examined. The findings show early voting experiences shape future voting profiles. Moreover, casting a ballot does not boost non-electoral participation.

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Notes

Other game-theoretic models have been also proposed. See indicatively Palfrey and Rosenthal (1985), see also the minimax regret rule suggested by Ferejohn and Fiorina (1974) and further elaborated by Aldrich (1993).

Following Aldrich et al. (2011: 542), context is defined as "the set of preceding actions, cues, events, and people that are associated with regular repetition of the action".

It is quite unlikely, however, that the divergence in the findings between Gerber et al. on the one hand and Meredith on the other is due to this potential violation of exclusion. Rather, there are two other reasons that seem more likely to account for this gap. First, Gerber et al. (2003a) examine the effect of voting in the November 1998 election on the likelihood of voting in the next local election, of November 1999. In the case of Meredith, participation in one presidential election is used as a predictor for participating in the next presidential election. Evidently the difference in the time interval between treatment and outcome in these two studies could help explain part of the difference in the magnitude of the effects. Second, as Meredith points out, whereas the causal effect in the case of Gerber et al. corresponds to the proportion of people whose treatment status changed as a result of the GOTV targeting (2% of their sample), in the case of Meredith the effect of the act of voting refers to all the population of young voters. Evidently, in the case of Gerber et al., the subgroup of individuals for which the treatment effect can be identified refers to people who have been mobilized to vote by GOTV messages and thus are by construction more likely to be marginal voters (Meredith, 2009: 27).

Secondary schools (97) were the primary sampling units, selected with a probability proportional to their size. Within each school 15–21 randomly designated seniors were

interviewed (N = 1669). In total 935 of the 1965 respondents were re-interviewed in all four waves. To enable comparability across elections, the main part of the analysis

uses only these respondents. Although the problem of panel attrition cannot be effectively addressed, it is at least reassuring that when using respondents who were interviewed in the first two waves, the estimated effect of voting in 1968 on voting in 1970 are very similar. Moreover, it is quite unlikely that the probability of remaining the panel was affected by 1968 eligibility status.

Although it helps the visualization of the research design, Figure 1 may raise questions about whether a Regression Discontinuity (RD) estimator instead of an IV estimator should be employed. In effect, the research set-up bears resemblance to a fuzzy RD design, with the 5 November – the day the 1968 presidential election was held – serving as the time-point which distinguishes between the two groups (fuzzy in that voting in 1968 is a deterministic function of whether one is born before or after 5 November only for those born before or after 5 November 1947; those born before this date may also not have voted). However, I do not use an RD estimator for the following reasons. First, all respondents are clustered within a very narrow range of values with respect to the forcing variable (age). Age matters for turnout but probably not within a nine-month bandwidth. In other words, I treat all respondents as being located sufficiently close to the cut-off point that being below or above this threshold (our instrument) is assumed to be assigned randomly. A placebo test presented later confirms that it is not the age gap between the two groups that accounts for the findings. Second, an RDD estimation is data-demanding. Closing the window on both sides of the threshold would result in remaining with very few observations especially among the group of non-eligibles, thus generating uninformative estimates. Third, minimally extrapolating at the point of the discontinuity would hardly add something to our inference, since there is no a priori reason to believe that the actual date of birth is of particular importance within the two groups. Accordingly, all observations included in this window are weighted equally. A robustness check, reported in the next section, shows that allowing for different windows hardly produces any change in the results. Previous studies using eligibility as an instrument of turnout have also used an IV approach (Meredith, 2009; Mullainathan & Washington, 2009). That said, for purposes of completeness, the ITT of vote in 1968 on turnout in 1970 is also estimated

with an RD estimator and, as explained in footnote 11, the result is almost identical.

Following standard notation, upper-case letters are used to denote random variables, whereas lower cases denote realized values of these variables.

With regard to the stable unit value treatment assumption (SUTVA), which requires that potential outcomes and treatments of individuals are unrelated to the treatment, assignment or outcome status of other individuals (Angrist et al., 1996: 446), it is reasonable to assume that it is not violated here. Given that school clustering refers to 1965 and the actual election of interest takes place three years later, even if such information effects exist, they should not manifest themselves by 1965.

None of these tests constitutes unambiguous evidence that exclusion is satisfied. However, when taken all together, they indicate that the attitudinal profiles of the two groups do not match the expectations one should have if, apart from the act of voting in 1968, eligibility also engendered a gap in political interest or political sophistication between eligibles and non eligibles due to possibly differential exposure to campaign messages.

The LARF estimation comes from Abadie's code for a linear response outcome in MATLAB: http://www.hks.harvard.edu/fs/aabadie/cls.m. Using a probit first step provides substantively identical results.

Estimating this Intention-To-Treat effect with a regression discontinuity estimator (using local regression estimates), we get a very similar effect, 16.8% (std. error 5.9). The window chosen by the optimal bandwidth formula suggested by Imbens and Kalyanaraman (2009) is 30.38 days.

To facilitate the evaluation of these effects when compared with the findings regarding future presidential elections, only respondents present in all four waves are used. To reassure that excluding individuals that replied in waves 1 and 2 but then disappeared

from the panel does not invalidate inference, the analysis shown in Table 2 has been replicated using all respondents of the first two waves. The 2SLS estimate, using only

those born in 1947 or in 1948, is almost identical: 0.263 (0.086), N = 814.

By 1972 all high school seniors were approximately 25 years old. Most of them had already finished their studies, had started their own family and were enrolled in so professional environment. Moreover, respondents are high-school seniors, more than half of whom eventually obtained at least some college education, hence there is an upward bias in the average level of education.

Details about how these outcomes are measured are given in the note accompanying Table 5.

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