

The Effect of Information on Voter Turnout: Evidence from a Natural Experiment

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Do better-informed people vote more? Recent formal theories of voter turnout emphasize a positive effect of being informed on the propensity to vote, but the possibility of endogenous information acquisition makes estimation of causal effects difficult. I estimate the causal effects of being informed on voter turnout using unique data from a natural experiment Copenhagen referendum on decentralization. Four of fifteen districts carried out a pilot project, exogenously making pilot city district voters more informed about the effects of decentralization. Empirical estimates based on survey data confirm a sizeable and statistically significant causal effect of being informed on the propensity to vote.

A defining feature of advanced democracies is universal suffrage: everyone has the right to vote. However, not everyone exercises this right and voter turnout varies considerably, both over time and across countries and individuals. This variation is not random; across individuals, it is a stylized fact of the empirical voter turnout literature that better educated individuals participate more frequently in elections, as do those with greater wealth and higher incomes (Wolfinger and Rosenstone 1980). Unequal participation, whether in general elections or in direct democracy settings, has important implications: first and foremost, political participation is an instrument of representation and, therefore, unequal participation can distort the pattern of representation necessary for democratic responsiveness, leading to real effects on policy outcomes.¹ In an encompassing survey of voter turnout across countries and over time, Lijphart concludes that “unequal participation spells unequal influence” (1997, 1), and this stylized fact forms the basis of recent political economy models such as Benabou (2000). Second, participatory inequality is a problem if democratic participation is seen as an intrinsic good (Pateman 1970) in addition to its

role as a representational instrument, and it may create doubts about the democratic legitimacy of a given political setting.

Efforts to understand the determinants of voter turnout often take observed empirical regularities as their starting point. The key finding of Wolfinger and Rosenstone (1980) that education is the most important predictor of voting forms the basis of recent innovative work by Feddersen and Pesendorfer (1996, 1999) arguing that *informational differences* among voters can help explain the observed variation in political participation. In their models of single issue elections, uninformed citizens’ optimal choice can be to abstain from voting, even if they prefer one alternative to the other (the swing voter’s curse). Instead, they effectively delegate decision-making powers to informed voters, thereby increasing the likelihood of the optimal policy being chosen. Matsusaka (1995) and Ghirardato and Katz (2002) also consider the effects of being informed on the propensity to vote, but their explanation relies on subjective uncertainty about the quality of information and, hence, the risk of making the wrong decision, rather than through the strategic reasoning applied in Feddersen and Pesendorfer.

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¹One example that representation matters for outcomes is the finding by Ansolabehere, Gerber, and Snyder (2002) that court-ordered redistricting correcting disparities in the populations of legislative districts in the United States has had a significant impact on the flow of state transfers to counties.

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In these models of the turnout decision there is a causal effect from being, or feeling, informed on the propensity to vote. However, almost all empirical evidence, whether based on educational attainment or other measures of information and informedness, really reports correlations rather than causal effects, although the analysis is often embedded in a regression framework. The problem is that information acquisition is endogenous and, therefore, that both the decision to vote and the decision to obtain an education or become informed about political issues can be caused by some third, unobservable, factor. Hence, to make a statement about causal effects in order to empirically evaluate the theoretical work, it is necessary to address the endogeneity problem.

In this article I use unique data from a natural experiment to correct for possible endogeneity of being informed. Natural, or quasi, experiments have long been a part of program evaluation in psychology (Campbell 1969; Cook and Campbell 1979) and labor economics (Angrist and Krueger 2001; Meyer 1995). The key feature of natural experiments is to supply an exogenous source of variation in explanatory variables that determine the treatment assignment in a nonexperimental setting.² One type of natural experiments involve *pilot projects* (Campbell 1969, 426) where a subset of administrative units are involved in a trial program that can, eventually, be spread to other units. I use a pilot-project experiment on decentralization in Copenhagen, and a subsequent referendum determining whether full reform should be implemented, to estimate the causal effects of being (more) informed on voting propensity. The pilot-project structure of the social experiment results in citizens of the treated city districts having first-hand experience of the effects of decentralization in contrast to the control group of citizens, residing in the other city districts. As I show below, this exogenously determined variation in experience translates into differences in the degree of information across districts, making it possible to estimate directly the effect of being informed on voter turnout in a city-wide referendum using survey data.³

²See, though, Campbell (1969) and Besley and Case (2000) for a discussion of caveats in natural experiments, in particular when sources of policy differences across units are due to legislation reflecting political sentiments of the electorate. Green and Gerber (2002) provide an introduction to recent experimental work in political science.

³This formulation does not rule out the possibility that citizens residing in other districts receive information about the consequences of the experiment through, say, city-wide media, but assumes only that those living in treated city districts are *more* informed relative to some common level of information, due to direct experience with the effects of decentralization or possibly through local media. I return to this issue in the fifth section.

Endogeneity of information acquisition cannot be rejected, and I show that being informed does cause a greater propensity to vote. This effect is quantitatively important and larger than the effect estimated by conventional methods. The effect is stable over different configurations of instruments and different samples, it is stronger for people with no cost of voting, and appears to be driven primarily by increasing the propensity to vote for people who do not ordinarily vote in local elections. Furthermore, I find evidence of an indirect effect of education through increased levels of information.

The article proceeds as follows: After a brief look at related literature, the next section reviews theoretical work linking knowledge and turnout, while the third and fourth sections describe the natural experiment setting and the data, respectively. The fifth section presents the empirical analysis and the sixth concludes.

Related Literature. The literature on voter turnout is voluminous, and no attempt to survey it will be made here; recent surveys and discussions of the literature are provided in Blais (2000) and Schlozman (2002). Matsusaka and Palda (1999) review some empirical evidence, and Besley and Case (2003) discuss in detail the institutional determinants of the mapping from voting to policy outcomes. Two recent articles investigate the causal effect of education on voter turnout: following a literature in labor economics, Dee (2003) and Milligan, Moretti, and Oreopoulos (2003) use U.S. state government variation in compulsory schooling laws as instruments to identify the effect of education on voter turnout and other aspects of civic participation. Both studies find that more education causes a higher propensity to vote. Milligan, Moretti, and Oreopoulos further find that education also implies greater political knowledge and greater interest in politics. However, this does not imply a causal effect of more knowledge on the propensity to vote and, thus, leaves open the question of exactly how education increases turnout; several reasons are possible, including lowering costs of information processing but also through reducing alienation and increasing compliance with social norms through socialization.

A key result of Milligan and his colleagues is that the effect of education on turnout in the United States disappears when conditioning on registered voters, suggesting that the role of education is to overcome registration barriers, and that there is no effect of education on turnout in the United Kingdom, where registration to a large extent is carried out by local governments; see, though, Matsusaka and Palda (1999) for evidence of the effects of education on Canadian data. In Denmark, everyone is automatically

registered as voters, resembling the British rather than the American system, and, as I show below, I too find no direct effect of education on voter turnout, but there is some evidence of an indirect effect, through information.

Where appropriate natural experiments can be difficult to identify, field experiments are possible alternatives. Gerber and Green (2000) conduct a field experiment to investigate the effects of contacting voters to remind them of an upcoming election. The purpose of the contact is to inform voters about the fact that an election is taking place, rather than about the issues of the election and the candidates themselves. Such a reminder is automatically issued in Denmark, as everyone eligible to vote receives a “ballot card” to be presented at the polling station.

In an analysis of the effects of information on New Deal spending in the United States, Strömberg (2004) finds that regions that were more informed, measured by a higher share of radio ownership, had higher turnout. However, it is not possible to distinguish whether radio owners were more informed about the fact that there was a general election, whether it increased general political interest or whether it was exact information about the election issues that caused higher turnout. In any case, Strömberg’s interesting analysis concerns general elections and, as such, is not directly linked to the theoretical work described below.

One article to address issues similar to those considered here is a recent one by Larcinese (2002). He considers the relation between political information and voter turnout in a British General Election using an instrumental variables approach. However, it seems that his key instrument, readership of quality newspapers, could be related to unobserved heterogeneity (e.g., values) and, thus, is determined jointly with political information and voter turnout.

Why Being Informed Affects Voting Behavior

Three recent formal models consider the problem of how information about an issue to be voted upon in a referendum affects the choice of whether to vote or not. Feddersen and Pesendorfer (1996, 1999) propose a game-theoretic model of voting, where the turnout decision is influenced by the information structure facing prospective voters. The election they consider is a referendum on whether to adopt a new policy instead of the status quo. In the model, voting is costless for all agents and, thus, abstention cannot be explained by differences in the cost of voting, in contrast to the traditional decision-theoretic literature originating with Riker and Ordeshook

(1968). The difference in the voting behavior among agents comes from the presence of asymmetric information: some agents are informed, some uninformed. The central result of Feddersen and Pesendorfer (1996) is that it can be optimal for uninformed independent voters to abstain from voting even though they may prefer one alternative to the other. The reason is that by abstaining they effectively defer the choice to the informed voters who, by definition, vote for the correct policy. When there is a large number of voters, this will lead to the correct policy being chosen (Feddersen and Pesendorfer 1997). The central empirical prediction is that (more) informed agents should vote in the election, while uninformed agents should abstain from voting. At the aggregate level, increasing the expected fraction of informed voters will, then, lead to a lower level of abstention.

Recent decision-theoretic models yield a similar hypothesis. This approach considers a single voter and how being informed affects her decision to vote or abstain. Matsusaka (1995) models this by parameterizing the voter’s certainty that she votes for the right candidate, that is, the candidate that, if elected, yields the highest total utility to the voter. As stressed by Matsusaka, and also a feature of the other models reviewed in this section, it is the voter’s subjective belief about her information level that guides participation, and this can differ from “objective” measures of political knowledge. The key result is that voters who feel more confident about their choice derives a higher utility from voting. In a similar spirit, Ghirardato and Katz (2002) models the choice of ambiguity-averse voters who differ in their quality of information. Their careful modelling approach yields the key result that it will be optimal for an instrumentally rational voter to abstain from voting when different policy options are preferred in different states of the world.

In these models, voters are interested in an optimal policy being implemented. However, in actual referenda a number of issues can result in more “noisy” voting than that predicted by the model. First, the existence of a norm of voting, generally well documented in the empirical voting literature, can result in uninformed agents voting, regardless of the fact that they—in the Feddersen and Pesendorfer world—would be better off (in expectations) by not voting. Second, uncertainty about the issue could take the form of a status quo bias, documented in a variety of settings, leading uncertain voters to vote for the status quo where abstaining, according to the reasoning in the models, would be optimal. Third, the literature on protest voting, e.g., Horton and Thompson (1962), argues that local referendums may serve as institutional outlets for protests, leading to negative voting, i.e., against new proposals. All three effects will tend to increase turnout

for a given distribution of information; however, this is not a problem for the analysis below, as it biases the data against confirming the hypothesis.

A Natural Experiment on Decentralization

In 1996, the municipality of Copenhagen, Denmark, of almost half-a-million inhabitants, introduced an experiment on decentralization of the city administration. For the purpose of the experiment, the city was divided into fifteen city districts, and four districts, chosen such as to be representative of the city, introduced local administration for a four-year period. The actual selection of the four pilot city districts (PCD) was made by the Copenhagen Municipality Structural Commission in 1995 and, according to the chairman of the committee (reported in *Berlingske Tidende* (1995), a Danish national newspaper), the selection was made to achieve “a good balance of two inner city and two more suburban neighborhoods, four distinct social profiles—one strong, one slightly above average, one slightly below average, and one weak—as well as one large, two medium and one small city district.”⁴

The local administration had been set up and a city district council elected for each PCD in a local election, characterized by low turnout (PLS Consult 1999, 299), such that the experiment could take effect from January 1997. The degree of decentralization was considerable, amounting to approximately 80% of municipality tasks (Klausen 2001), including primary schools, daycare, care for the elderly, social assistance, and cultural and recreational activities. The most important limitation to decentralization was that tax-setting authority remained at the municipal level.

The decentralization experiment was evaluated in late 1999 by an external consulting firm (PLS Consult 1999) and on the basis of the evaluation report, the city council decided to hold a consultative referendum in the entire municipality of Copenhagen on whether decentralization should be extended to all city districts or should be abolished completely for all districts, including the PCDs.⁵ The City District Council (CDC) referendum was held the same day, in September 2000, as a nationwide referendum on whether Denmark should join the common European currency. Aggregate turnout in the city district referendum was 70.5%, roughly equivalent to nationwide averages for local elections, and considerably higher than

in the local elections for the city district councils, but substantially below the Copenhagen turnout in the nationwide referendum at 83.8%.⁶ The outcome of the referendum was a substantial majority against decentralization: only in one of the fifteen districts—a PCD—was there a (narrow) majority for decentralization. The regression analysis of referendum voting patterns in Klausen (2001) found that two of four PCDs voted (weakly) significantly more in favor of the reform. The outcome of the referendum was that a decision was made by the city council to discontinue the experiment, dismantling the pilot administrations from January 1, 2002.

The Copenhagen referendum on decentralization provides a unique natural experiment for testing whether turnout is higher for more informed voters. First, it was a simple referendum with a status quo and an alternative, and—importantly—the consequences of the implemented policy would be the same, at least ex ante, for both treated and nontreated districts such that PCD citizens would not gain or lose more than citizens in the other districts. Second, a suitable exogenous instrument for being informed can be identified. Third, the fact that the referendum was held in conjunction with the nationwide referendum on Danish membership of the common European currency makes it possible to focus on people with zero costs of voting, a key assumption of the theoretical studies outlined above.

Data

The data used in the main empirical analysis is based on a telephone survey of Copenhagen voters, carried out in November 2000. The survey, which was commissioned by the four PCDs, was carried out as part of a project to analyze the voting patterns in the referendum (Klausen 2001). Voters were partitioned into five strata, the four pilot city districts and the rest of the city as a whole. No further subdivision was made, making the individual voter the primary sampling unit. The (translated) question wording, variable coding, and descriptive statistics are given in the appendix. The response rate of the survey, calculated

⁶Total number of votes cast in Copenhagen in the city and Euro referenda were, respectively, 290,886 and 312,940, even though the set of eligible voters for the city referendum was considerably larger (412,425) than in the Euro referendum (373,422). The source is Copenhagen Statistical Office (2000a).

A brief note on Danish elections: every Danish resident has an identification number, the CPR-number. Everyone eligible to vote in a particular election automatically receives a ballot card sent to the address registered in the CPR-registry. The voter is required to present the ballot card at the polling station, typically the nearest public school, which in Copenhagen is rarely more than one kilometer away, and the fact that an individual has voted is registered.

⁴ Author's translation.

⁵The Danish constitution does not allow for binding referenda at the municipal level.

as the number of completed interviews relative to the sum of completions, refusals/no answer, and partial completions, was 55% resulting in a main sample of 3,021 observations. Within this sample, almost one-third did not wish to answer the question on yearly income and a few did not want to disclose whether they voted in the referendum. Therefore, the empirical analysis is based primarily on a sample of 2,026 observations, but the robustness of the results are demonstrated on the full data set, leaving out income as an explanatory variable.

Measurement Issues

To measure whether survey respondents themselves felt informed about the decentralization issue, I use a question that asked respondents their opinion on the decentralization experiment. If they responded that the experiment went well, medium well, or bad, they are coded as having an opinion and, thus, being informed. If they respond that they do not have an opinion, they are coded as being uninformed. This way of measuring informedness captures the subjective nature of being informed as stressed in the theoretical papers, in contrast to index measures based on factual questions. Does people's own perception of their informedness bear any resemblance to more objective measures of being informed? Survey and opinion research in political science and psychology suggests this to be the case (Krosnick 1999); for example, Faulkenberry and Mason (1978) find that survey respondents that answer "no opinion" or "don't know" have significantly lower factual knowledge of the survey issues.⁷ Hence, the measure used in this article serves both as a suitable measure of subjective informedness and as a proxy for more objective information measures.

A standard problem of voter turnout studies based on surveys is that the estimated turnout from the survey is higher than the true turnout. This is also the case here: the estimated turnout from the sample is 84.3%, 13.8 percentage points higher than the true turnout registered at 70.5% (Copenhagen Statistical Office 2001). Two effects can be at work: First, those who choose not to participate in the survey often are the very same people that did not vote (Brehm 1993). Second, people may report to have voted even if they did not. While intentional misrepresentation

of voters have long been thought to be a major problem in surveys, recent public opinion research instead emphasizes the problems of unrepresentative samples (Burden 2000; Krosnick 1999). Indeed, misreports are often a result of memory failure, rather than intentional efforts to misrepresent one's voting behavior (Belli et al. 1999).

The first effect above means that some people with certain characteristics are over- and undersampled, respectively, and this can be addressed by probability sampling (post-stratification). The sample is unevenly allocated over the PCDs and the non-PCDs, due to the survey design; this means that non-PCD citizens are severely underrepresented. However, the sample is also biased, as is typically the case, toward people with higher incomes and longer education.

By far the strongest imbalance, however, comes from the fact that people who participate in the political process on a regular basis, captured by a question on whether they voted in the previous municipal election, are strongly overrepresented in the sample. The estimated turnout for the municipal election (held in 1997) based on this sample is 80.0%, whereas the true turnout was only 58.0%, a noteworthy difference of 22.0 percentage points. Therefore, I reweight the sample by adjusting for past voting behavior in municipal elections by city district when comparing variable means below, but the exact reweighting procedure has no effect on the regression analyses below, as these are done on the unweighted sample; I return to the estimation procedure below. This reweighting results in an estimated turnout for the CDC referendum of 73.9%, now only 3.4 percentage points higher than the true turnout and comfortably including the true turnout in its 95% confidence interval.⁸ Whether the remaining difference is due to other socioeconomic factors underlying nonparticipation in the survey, or to misreporting cannot be determined as present, as no voter validation data—comparing survey responses with actual voting records—exist for Danish elections.⁹

⁷Faulkenberry and Mason divide a sample of respondents on a survey on wind energy conversion issues into those with substantive opinions (favor, oppose), those with ambivalent opinions (no opinion) and those with nonexistent opinions (don't know). They find that those with substantive opinions have more factual knowledge (measured on an eight-point scale) than those without such opinions, and, in turn, that those with ambivalent opinions have more knowledge than those with nonexistent opinions.

⁸Post-stratification on the matched sample (based on propensity score matching on the full sample, see below) yields an estimated turnout of .723, even closer to the true turnout. If survey respondents misreport past voting, is this a suitable variable on which to base post-stratification? First, the qualitative results are similar to those obtained by weighting on education or income levels. Second, there is no impact on results, as long as the share of those who misreport voting are similar across treated and untreated districts. Third, if some voters misreport past voting behavior, reweighting will decrease their sampling probability, resulting in a more balanced sample.

⁹Would using validated voting data change the conclusions? Krosnick (1999) provides a critical review of survey problems related to voting behavior, noting that the use of validated data does not change substantive conclusions. Presser, Traugott, and Traugott

The Effect of Information on Voting: Empirical Estimates

The empirical analysis proceeds in two steps. First, after briefly reviewing some estimation issues when using complex survey data, I compare the pilot city districts with the control districts to make sure that these are in fact comparable. Then I proceed to estimate the effect of exogenous exposure to information on the propensity to vote.

Analysis of Stratified Survey Data

The literature on estimation under complex survey sampling consists of two approaches: model-based analysis, using unweighted data for estimation, and design-based analysis, where features of the complex survey sampling such as differing sampling probabilities and stratification are taken into account (Levy and Lemeshow 1999).¹⁰ Deaton (1997) presents the two approaches to regression analysis using data from complex surveys and shows that estimating descriptive statistics such as means and variances in a consistent way requires design-based analyses, while matters are less clear cut in the case of regression analysis. A general result of the literature (Wooldridge 2002) is that unweighted estimators are consistent and more efficient when stratification is exogenous, as is the case here, where stratification is based on geographical units which are homogenous in observables (see below) and individuals' selection of district residency arguably is unrelated to the city district experiment.

Therefore, I use weights when tabulating and comparing means, while I use unweighted estimators in the regression analysis, but in fact regression results such as marginal effects and average treatment effects are remarkably identical to those obtained using design-based analysis, where sampling weights and within-stratum variation are accounted for.

Comparing Pilot and Control Districts

Above, I described the selection of the four PCDs. These were selected so as to be representative of the city's composition such that the average treated citizen would be identical to the average control citizen. This is important when evaluating the causality from information to voting. While individuals were sampled randomly

within each stratum, if the strata are different, for example due to sorting, the treatment effect on information could be reflecting differences in other variables. Further, as discussed above, heterogeneity across strata would change stratification from being exogenous to endogenous, which would require a different modelling approach (Wooldridge 2002).

Table 1 provides evidence that the districts are similar in observables. It reports means for key variables based on the weighted sample, taking into account initial and post-stratification, for the treated and the control city districts, respectively. The means reported are roughly the same across treated and nontreated districts, and are in only one case weakly significantly different. Thus, the weighted sample means correctly reflect the fact there are negligible differences in population means across the treated and nontreated districts, substantiating the claim that the pilot city districts are representative of the city as a whole.¹¹ It is reassuring to see that differences in political attitudes are not behind increased participation in the PCDs. For example, the reported level of political interest is exactly the same in the two groups, implying that the effect of exposure to the decentralization experiment is not through an increased interest in local politics.¹² The estimates reported in the table also validate the claim made above that the share of citizens using would-be decentralized services (service1 - service3) did not differ substantially across district types; the estimated district difference in the aggregate share of service users is only a third of its standard error.

The lower part of Table 1 compares population differences based on administrative data (from Copenhagen Statistical Office 2000b). While the categories generally are not comparable to the survey responses in the top part of the table, it demonstrates that population level differences are also of minor importance.

Are the few significant differences between the treatment and the control group a cause for concern when evaluating the effect of being informed on voting? If the treatment and control samples are not balanced, in the sense of having the same distribution of characteristics, differences in voting behavior could be reflecting this unbalancedness, rather than the treatment itself. One possibility, used extensively in the evaluation literature, is to use matching as a basis for comparison. Essentially, matching

(1990) demonstrate that administrative errors in vote registration is responsible for a large part of observed voter misreports, leading them to question the validity of voter validation studies.

¹⁰ A common feature of surveys is that data is collected in subunits (clusters) within strata; however, this is not a feature of the present survey where the primary sampling unit is individuals.

¹¹ This is further corroborated by the fact that accounting for stratification when estimating the model affects the standard errors only at the fifth decimal point.

¹² Citizens' interest in politics before the experiment began (in 1996) was also similar across would-be treatment and control districts (PLS Consult 1999, 204–05).

TABLE 1 Comparing Pilot City Districts and Control City Districts

Variable	Obs.	PCD Mean	CCD Mean	Diff.	t-Stat	p-Value ^a
Income (1000 DKK)	2,026	264.6	281.8	−17.2	−1.31	0.189
Turnout in municipal election (%)	2,870	59.6	57.4	2.2	0.72	0.471
Turnout in Euro referendum (%)	2,870	89.2	86.0	3.3	1.46	0.144
College education (%)	2,870	48.3	47.9	0.4	0.14	0.886
Age	2,870	39.6	39.4	0.2	0.23	0.817
Gender (% female)	2,870	55.2	54.9	0.3	0.09	0.925
Publicly employed	2,870	27.1	27.4	−0.3	−0.14	0.887
Privately employed	2,870	38.5	41.9	−3.4	−1.21	0.225
Service 1 (elderly care)	2,870	11.5	10.2	1.3	0.75	0.452
Service 2 (daycare)	2,870	18.5	17.3	1.3	0.62	0.535
Service 3 (primary school)	2,870	14.9	18.5	−3.6	−1.73	0.084
Political responsiveness	2,870	2.88	2.85	0.03	0.57	0.565
Political interest	2,870	1.03	1.03	0.00	0.12	0.906
Administrative Data						
Turnout in municipal election (%)		59.6	57.4	2.2		
Yearly income (1000 DKK)		179.0	176.0	3.0		
College education (%) ^b		28.5	25.0	3.5		
% of population > 60 y		19.1	20.5	−1.3		
Unemployment		7.4	6.1	1.3		

Note: Sample weighed such that population size = 412,425 in all tests.

^aTwo-tailed test. ^bNot comparable to survey data due to classification differences.

tries to recover a random research design from observational, or imperfectly randomized, data to provide a basis for causal interpretation of the estimates. While individuals could not self-select into treatment,¹³ matching ensures that assignment to the experiment, conditional on observables, is random and independent of informedness and voting propensity in the nonexperiment state of the world. Thus, the matching procedure ensures that inference is not based on treatment and control samples that are too different, which is not guaranteed by simply including these variables as controls in a regression in the standard fashion. As shown by Rosenbaum and Rubin (1983), it is under certain conditions sufficient to match on the propensity score, which is the probability of treatment conditional on observables, rather than on the vectors of observables themselves.¹⁴ While propensity score matching under complex survey sampling seems not

to have been addressed in the theoretical literature, I carried out nearest-neighbor matching based on a propensity score estimated from a probit regression. This results in 82 observations, or 2.9% of the sample, being outside the common support, i.e., being too different to form a reliable basis for comparison. In this reduced sample, all weighted sample mean differences are now insignificant. I report the results of regression on this sample below.

Empirical Model

I model the voting decision and the decision to become informed in a latent variable framework. For voter i , let T_i^* describe the net benefit from voting given by the following underlying behavioral specification:

$$T_i^* = \beta_T' x_{Ti} + \gamma INF_i + \varepsilon_i,$$

where x_{Ti} is a covariate vector and INF_i is a dummy variable indicating that i reported having an opinion about

experiments can sometimes be subject to unbalanced treatment and control samples.

¹³Unless they actually moved with the explicit aim of living in a PCD, a situation which seems highly unlikely.

¹⁴Imai (2004) provides a very accessible introduction to propensity score matching in the context of Gerber and Green's (2000) random experiment, showing how even truly randomized social

the city district experiment. The net benefit from voting is unobservable, but what is observed is whether individual i voted or not, designated T_i and defined by

$$T_i = \mathbf{1}(T_i^* > 0) = \mathbf{1}(\beta'_T x_{Ti} + \gamma INF_i + \varepsilon_i \geq 0) \quad (1)$$

where $\mathbf{1}(\cdot)$ denotes the indicator function. If being informed about the city district experiment was exogenous, the parameters of (1) could be estimated directly specifying a distribution for ε . However, the decision to become informed is endogenous, and failing to take this into account would result in biased estimates. Let INF_i^* be individual i 's net benefit from being informed. The reduced-form behavioral model, described in different contexts by the studies referred to above, is given by

$$INF_i^* = \beta'_{INF} x_{INF,i} + \alpha D_i + v_i$$

where $x_{INF,i}$ is a vector of possible covariates and D_i is a treatment dummy variable equal to one if i resided in a PCD. Again, we do not observe INF_i^* but rather a dummy variable INF_i which indicates whether i reports having an opinion or not. This is defined by

$$INF_i = \mathbf{1}(INF_i^* > 0) = \mathbf{1}(\beta'_{INF} x_{INF,i} + \alpha D_i + v_i \geq 0). \quad (2)$$

Ignoring endogeneity of INF , estimation of (1) proceeds by specifying a distribution for the error term ε , typically a normal (probit) or a logistic (logit) distribution.¹⁵ A natural extension of this assumption to include possible endogeneity of information acquisition is to assume a bivariate probit model where ε_i and v_i are jointly normally distributed with $E(\varepsilon_i) = E(v_i) = 0$, $var(\varepsilon_i) = var(v_i) = 1$ and $cov(\varepsilon_i, v_i) = \rho$. The model is identified if living in a PCD affects the propensity to vote *only* through its effect on information (the exclusion restriction). I return to this issue below.

Since INF is a binary variable, estimating the system (1) and (2) by linear methods such as two-stage least squares will not give consistent results. However, as noted by Angrist (1991), in certain cases the TSLS estimate can be close to the average treatment effect estimated by the bivariate probit model (see, though, also Angrist 2001). For comparison, below I present results from OLS, TSLS as well as the IV-probit, and the full bivariate probit models.¹⁶

¹⁵While the logit specification is typically employed in the voter turnout literature, this does not extend readily to the multivariate case. Single-equation probit on the present sample generally results in slightly lower z-statistics than in the logit case, making probit estimation significance levels conservative.

¹⁶If the errors are not jointly normal, the bivariate probit model is misspecified and can lead to inconsistent estimates. However,

TABLE 2 Estimating Informed Population and Turnout

Variable	Estimate	S.E.	t-Statistic	p-Value ^a
Informed, PCD (percent)	61.9	1.3		
Informed, non-PCD (percent)	49.1	2.5		
Difference	12.8	2.8	4.56	<0.001
Turnout, informed (percent)	78.4	2.4		
Turnout, uninformed (percent)	69.0	2.9		
Difference	9.5	3.7	2.56	0.011

Sample size = 2,870. Population size = 412,425. ^aTwo-tailed test. Standard errors are survey-corrected.

Main Results

Before embarking on the multivariate analysis, I look at mean differences. The upper part of Table 2 reports the estimated population means of informed individuals in treated and control city districts, and the lower part reports estimated population turnout rates for informed and uninformed individuals.

The difference in the fraction of informed people in the two groups is almost 13 percentage points and strongly significant. Similarly, the estimated difference in turnout between informed and uninformed individuals is almost 10 percentage points, nearly significant at the 99% level. Of course, inference from simple two-ways tables can be misleading, since it does not account for other potential influences as well as possible endogeneity. I now turn to multivariate analysis, to control for other potential influences on the propensity to vote.

Table 3 displays the results of three specifications of the full regression model based on the survey data: single equation probit, IV-probit (based on Newey 1987) and full bivariate probit. The table reports estimated coefficients, standard errors, and marginal effects (calculated at the means of the other variables) for the variable of interest *informed* and the full set of controls. The first column reports results of the single equation probit specification (equivalent to estimating Equation (1) directly, assuming ρ to be zero). The estimated coefficient for *informed* is, as expected, positive, and it is estimated with considerable

simulation results of van der Klaauw and Koning (2003) suggest that the effects of even serious misspecification may be limited.

TABLE 3 The Effect of Information on Voter Turnout

	Dependent Variable: Voter Turnout					
	Probit		IV-Probit		Bivariate Probit	
	Coeff.	Marg. Effect	Coeff.	Marg. Effect	Coeff.	Marg. Effect
Informed	.536 [‡] (.079)	.114	1.867 [‡] (.843)	.481	.991 [‡] (.309)	.181
log(income)	.178 [‡] (.074)	.034	.162 [‡] (.076)	.031	.175 [‡] (.072)	.027
Age	.007 (.014)	.001	-.011 (.019)	-.002	.007 (.014)	.001
Age ²	-.000 (.000)	-.000	-.000 (.000)	-.000	-.000 (.000)	-.000
Female	.182 [‡] (.079)	.035	.150* (.085)	.028	.179 [‡] (.077)	.028
College education	-.155* (.082)	-.029	-.172 [‡] (.088)	-.032	-.149* (.080)	-.023
Public employment	-.169 (.125)	-.033	-.222* (.131)	-.044	-.164 (.122)	-.026
Private employment	-.159 (.125)	-.031	-.149 (.128)	-.029	-.156 (.122)	-.025
User of elderly care	-.127 (.115)	-.026	-.134 (.124)	-.027	-.125 (.113)	-.021
User of daycare	.269 [‡] (.109)	.046	.199* (.127)	.035	.264 [‡] (.107)	.038
User of primary school	.174 (.109)	.031	.156 (.125)	.028	.174 (.107)	.025
Level of political interest	.070 (.058)	.013	-.087 (.117)	-.016	.069 (.057)	.011
Regular voter	1.037 [‡] (.090)	.281	.849 [‡] (.151)	.218	1.009 [‡] (.094)	.210
Observations	2,026		2,026		2,026	
Wald χ^2	245.67		196.04		246.45	
% correctly predicted	85.1		70.4		80.8	
ρ					-.283	
					.192	

Robust standard errors reported in parentheses. All models included a constant term, not reported. Instrument: PCD dummy. [‡]Statistically significant at the 1% level. [†]Statistically significant at the 5% level. *Statistically significant at the 10% level.

precision. Columns two and three show results when correcting for the endogeneity of *informed* using the pilot city district dummy ($D = 1$) as an instrument. The IV and bivariate probit specifications increase the estimate of *informed* substantially: the coefficient in the bivariate probit model almost doubles, and the marginal effect is increased by a factor 1.6. These estimates continue to be strongly significant. The covariance coefficient ρ is

estimated to be negative, but insignificant (see, though, below).¹⁷

The estimated effects of the control variables are also of interest: the propensity to vote increases with income

¹⁷A Rivers-Vuong test for exogeneity of a binary explanatory variable in a discrete response model suggests that *informed* is endogenous ($p = 0.034$); see Wooldridge (2002, 478).

and is higher for women, users of municipal daycare, those with low education and regular voters.¹⁸ The other variables, mainly age, employment, and service user indicators, are insignificant. Note that these results are conditional on past voting behavior and, thus, should be interpreted as effects beyond those captured by a political participation “fixed effect.” If past voting is excluded as a control variable, education becomes insignificant and age (squared) enters significantly. These results are generally stable, both quantitatively and in terms of significance, across the specifications. In particular, the bivariate probit specification is almost identical to the single equation probit except for the increase in the estimate of the endogenous variable. The estimated effect of being informed does not depend on set of covariates included; any subset of the covariates can be dropped without altering the qualitative result on *informed*.

Using *PCD* treatment as an instrument for being informed for causal inference requires it to be (1) a determinant of being informed and (2) to be uncorrelated with the error term (ϵ) of the main estimating Equation (1). As noted above, Table 2 shows it to satisfy the first requirement; a probit regression of *informed* on *PCD* yields a *z*-value of 5.39 for the full sample.¹⁹ Hence, the validity of the results from the IV-probit and the bivariate probit specifications depend on whether the second requirement is fulfilled. Problems could arise if (a) (unobserved) differences in political interest and activism made some districts self-select into the pilot program, (b) treatment assignment was not random (or unignorable), or (c) the experiment itself increased *PCD* inhabitants’ interest in local politics or affected other variables that could influence voting behavior.²⁰

¹⁸More than 80% of children in the preschool age are enrolled in daycare (Copenhagen Statistical Office 2001). Thus, this variable is a proxy for having small children which, in turn, can be interpreted as a proxy for being married or cohabiting. I have no information on marital status in the dataset, but being married tends to increase participation in most work on voter turnout.

¹⁹A rule of thumb in the IV-estimation literature on weak instruments (Staiger and Stock 1997) suggests that first-stage *F*-values of excluded instruments in two-stage least-squares estimation should be larger than 10. Here, $F(1,2012) = 20.03$. While the TSLS model obviously is incorrect in this binary framework, the large *F*-statistic nevertheless suggests that *PCD* is a strong instrument.

²⁰Angrist, Imbens, and Rubin (1996) provide a presentation of the assumptions necessary for IV estimates to have a causal interpretation. In addition to those mentioned in the main text, in the context of the current model the *monotonicity* assumption holds that those residing in a control district and report to be informed would not report to be uninformed if residing in a *PCD*, which seems intuitively reasonable. Furthermore, the *stable unit treatment value* assumption holds that there is no interference between units which, in the present context, means that treated citizens do not affect the vot-

Concern (a) cannot be valid as city districts did not exist as administrative entities before the experiment and, furthermore, the exact partition of the city into districts was made simultaneously with the choice of pilot districts.²¹ Concerns (b) and (c) can be (partly) evaluated by looking at Table 1. As noted above, there are no substantial differences between treated and control city districts, as confirmed by carrying out propensity score matching. This confirms the assumption of ignorable treatment assignment (b).

Regarding the exclusion restriction, (c), I could identify no other differences in attitudes (and other variables) than that through information: for example are the levels of political interest and the assessments of government responsiveness indistinguishable across treatment and control city districts, as are the more direct measures of political participation such as voter turnout in the previous municipal election and in the Euro referendum; this suggests that living in a pilot city district did *not* increase interest in local politics. It is important to note that the notion of being informed is broader than it may seem at first: for example, if a *PCD* citizen had a bad experience with, say, decentralized elderly care and because of this decided to vote (presumably no) in the election, then that is covered by the present model. The experience, whether good or bad, serves to inform the treated citizens about the consequences of decentralization, making it easier for them to form an opinion on the decentralization issue.

While it is not possible to test directly the validity of the exclusion restriction, an indirect approach could be to employ a test for overidentification in the linear two-stage least-squares model. Evans and Schwab (1995) follow this approach in their bivariate probit model of the effect of Catholic schooling on educational achievement, noting that this may be the best available diagnostic. However, this calls for additional instruments. I investigate two possibilities for increasing the number of instruments.

First, I utilize the fact that the experiment was implemented in four distinct districts and include as instruments a dummy variable for each district rather than the *PCD* variable used above. This allows for the possibility that the effect of treatment on the degree of informedness for treated individuals could differ, perhaps owing

ing decision of others, an assumption shared (if implicitly) by the randomized field studies of Gerber and Green (2000) and Gerber, Green, and Shachar (2003). To the extent that informed *PCD* citizens exert a positive informational externality on control district citizens inducing them to change their behavior, the estimates reported in this article provide a lower bound of the true effect. See Miguel and Kremer (2003) for an analysis of treatment effects in the context of externalities.

²¹Berlingske Tidende (1995); PLS Consult (2001).

TABLE 4 Share of Informed Respondents by District and Service User Status

	Entire City	Non-PCD	PCD
Service users	57.0 (3.7)	51.3 (4.9)	72.2 (2.4)
Non-service users	55.4 (2.8)	53.3 (3.7)	60.8 (2.0)
Difference	1.7 (4.6)	-2.1 (6.2)	11.4 [‡] (3.2)
	n = 2026 N = 412425	n = 402 N = 299148	n = 1624 N = 113277

Survey-corrected standard errors in parentheses. [‡]denotes significance at the 1% level. Note that numbers may not add up due to rounding.

to (unobserved) differences in local implementation of decentralized services. Using individual PCDs as instruments yields a first-stage *F*-statistic of 9.4, indicating that the instruments are acceptable, and a *J*-statistic of .523, $p = .914$; thus, the hypothesis of no overidentification clearly cannot be rejected.

Second, I use the fact that users of decentralized services (elderly care, child care, and primary schools) are likely to know more about the effects of decentralization than those who just lived in the PCDs, who in turn know more than non-PCD citizens. As I argued in the introduction, a premise for the entire analysis is that citizens who lived in a PCD had more experience with the effects of decentralization which, in turn, should constitute itself through a higher level of informedness. Table 4 substantiates this claim by looking at how direct experience with decentralized services translates into more information.

Table 4 shows the estimated shares of informed citizens in PCDs and non-PCDs, respectively, broken down by whether the respondent was a user of a decentralized service. In PCDs, service users were significantly more informed about the experiment, and, importantly, this was not due to the fact that service users in general were better informed, as there was no significant difference in the information levels between service users and non-service users in non-PCDs. I put this to use in the regression analysis by interacting the PCD dummy with the service-user dummies to find that treated individuals who were users of daycare or primary schools reported having an opinion more frequently than others in the PCDs (not shown). I now have three instruments, and this yields $F(3,2010) = 8.35$ and a *p*-value of the overidentification test of .817. Together, both specifications

with more than one instrument suggest that the PCD indicator variable does not belong directly in the estimating equation.²²

To make sure that the results presented in Table 3 are not artifacts of the sample or the set of instruments chosen, in Table 5 I compare the results across econometric specifications including one and four instruments in two different samples. The two top rows display the coefficient and standard error of *informed* for the same sample as Table 3, using the same set of covariates, using the single and four instruments, respectively, such that the results for IV-probit and bivariate probit in the first row are identical to those reported in Table 3. The two bottom rows consider a different sample, where income has been left out of the estimating equation to increase sample size and to make sure that leaving out people who did not report income does not bias the results. Instead, as described above, I have matched the treatment and control groups using nearest-neighbor propensity score matching. This results in 82 (control group) observations (2.9%) being outside the common support.²³

The results reported in Table 5 are broadly similar across specifications: In particular, the choice of a single or four instruments seems to make little difference, whereas the estimated effects on the larger, matched sample are slightly smaller than the results based on those who reported income.²⁴ In total, the effect of exogenous exposure to information on voter participation is positive, strongly significant, and robust to alternative specifications. The effect of being informed, here defined as the ability to formulate an opinion on the referendum issue, is numerically large with average treatment effects in the neighborhood of 20 percentage points. Note that the treatment effects are smaller and the covariance coefficient ρ significantly negative in the lower

²²In addition to the results of the TSLS test of overidentification, some comfort can also be derived from the observation made by Angrist, Imbens, and Rubin (1996, 451) that the stronger the instrument, “the less sensitive the IV estimate is to violations of the exclusion assumption.” As noted earlier (note 19), the PCD dummy is a strong instrument for *informed*.

²³Propensity score matching is carried out by running a probit regression of *PCD* on all control variables from Table 3 and a dummy variable for voting in the referendum on the common currency as well as all pairwise interactions. The predicted values are used as propensity scores in nearest-neighbor matching. The estimation is carried out using the *psmatch2* procedure in Stata 7.0. (Sianesi and Leuven 2003). The distribution of propensity scores is shown in Figure A1 in the appendix.

²⁴Propensity score matching makes no difference to the results; results on the full sample (not reported) are almost identical to those reported here, reflecting the fact that the samples are very well balanced from the outset, as suggested by Table 1.

TABLE 5 IV-Estimation of the Effect of Information on Voter Turnout

Sample (# obs)	Obs.	# IVs	OLS Coeff.	TSLS Coeff.	IV-Probit			Bivariate Probit			ρ
					Coeff.	Marg. Effect	ATE	Coeff.	Marg. Effect	ATE	
Income sample	2,026	1	.115 [‡] (.019)	.378 [†] (.189)	1.867 [†] (.843)	.481	.365	.991 [‡] (.309)	.181	.250	-.283 (.192)
	2,026	4		.404 [‡] (.138)	2.005 [‡] (.667)	.520	.367	1.276 [‡] (.267)	.269	.220	-.465 [†] (.163)
Matched full sample	2,788	1	.115 [‡] (.016)	.415 [†] (.168)	1.968 [‡] (.708)	.511	.327	1.074 [‡] (.237)	.204	.197	-.344 [†] (.148)
	2,788	4		.419 [‡] (.140)	2.002 [‡] (.623)	.520	.328	1.179 [‡] (.219)	.236	.185	-.412 [‡] (.136)

All models included the full set of covariates shown in Table 3, including a constant term. Robust standard errors are reported in parentheses. Income sample: Excluding respondents with missing income information.

Matched full sample: Excluding control group members outside common support; see text for details (income not included as control variable).

[‡]Statistically significant at the 1% level.

[†]Statistically significant at the 5% level.

*Statistically significant at the 10% level.

part of the table, where the larger, matched sample is employed; a likely reason for this is that individuals who did not want to report yearly income may be less likely to have voted, and, thus, the average treatment effect is overestimated when income nonrespondents are excluded.

I also explored possible determinants of being informed in addition to the exogenous treatment indicators; the first-stage results of the TSLS estimation gave some indication of variables that influenced whether respondents had an opinion on the decentralization experiment. In results not reported, I find that age enters strongly in an inverted U-shape; this is consistent with the findings of Visser and Krosnick (1998) that attitude importance is greater in middle adulthood. Furthermore, I find education to be significant: those with longer education are more likely to have formed an opinion, a standard result in opinion research (Faulkenberry and Mason 1978; Krosnick 1999). Above, I noted that education levels has no direct impact on the propensity to vote when past voting is not included, echoing results from Milligan, Moretti, and Oreopoulos (2003). However, the finding that education increases the probability of being informed suggests that education may influence voting indirectly, possibly by lowering the costs of information processing. The estimated average treatment effects of being informed are slightly larger than those reported in Table 4.

The Cost of Voting

Feddersen and Pesendorfer (1996, 1999) and Ghirardato and Katz (2002) motivate their theories of informational voting by appealing to empirical evidence of *roll-off*, the fact that some voters facing multiple questions on a ballot do not cast a vote on every issue. Hence, they envision a world where there are no costs to voting, in contrast to the costs traditionally emphasized by economic theories of voting (Riker and Ordeshook 1968). One potential cost is that associated with being aware of the election; however, as noted above, everyone was provided the same stimulus to vote through a mailed ballot card to be presented at the polling station. To ensure that other differences in voting costs are not the reason behind the results on the effect of information—even though it seems unlikely that there should be differences in the costs of voting across city districts—in the following I exploit the fact that the referendum was held on the day of the nationwide referendum on whether Denmark should participate in the common European currency. Arguably an important decision, turnout in the nationwide referendum was high, 87.6% at the national level (Statistics Denmark 2002) and, indeed, the rationale of holding the city referendum on the day of the nationwide referendum was explicitly to increase turnout. While the costs of voting, for whatever reason, might differ between those voting and those not voting in the nationwide referendum, the cost of voting

in the city referendum would be practically zero for those *already* voting on the Euro. Furthermore, everyone eligible to vote in the euro referendum was also eligible to vote in the city referendum, the former set of voters being a strict subset of the latter.²⁵ The results based on the euro voter sample (not reported) are almost identical to those reported above, with slightly larger ATEs.

As noted above, the existence of a social norm of voting for a given distribution of information will bias the data against finding any effect of information on turnout. People who are guided by such a social norm would be less affected by information, as they vote anyway. Hence, I would expect people who are regular voters to be less affected by information, while (exogenously) receiving information may induce those who do not ordinarily vote to do so—that is, to increase T_i^* above zero in terms of the latent variable model. If I exclude past voting as a control variable and instead split the sample ($n = 2,026$) into those who voted in the previous municipal election and those who did not, dropping those who were not eligible to vote three years earlier, I find that the effect of information is only borderline significant for the municipal voter sample, whereas it is very strong and significant for those who reported not to have voted in the municipal election, even though the sample in this case only has $n = 293$ (the instrument is somewhat weaker in this sample). A similar pattern obtains for the larger, matched sample ($n = 2,788$) where the effect is also significant for those who voted in the municipal election but is much larger for those who did not.

Why Does $\hat{\gamma}$ Increase Under IV-Estimation?

The results reported in Table 3 show the estimate of $\hat{\gamma}$, the coefficient on *informed* in the empirical model of voter turnout, to increase under instrumental variables estimation; hence, the single-equation approach underestimates the effect of information on the propensity to vote, which is also reflected in the negative ρ obtained from the bivariate probit model. This is somewhat surprising

²⁵In this discussion, I implicitly assume that the main motive to go to the polling station would be to vote in the nationwide referendum. Could it be the case that the city district referendum caused some people who would otherwise *not* have voted in the euro referendum to go to the polling station? While possible, aggregate figures suggest that this is not the case. The ratio of turnout in Copenhagen relative to the rest of the country in the Euro referendum was .957, whereas the corresponding ratios in the previous general election (March 1998) and the previous nationwide referendum (on the Amsterdam Treaty, May 1998) were .957 and .976, respectively, suggesting that turnout for the Euro referendum in Copenhagen was not unusually high, which would have been the case if it was the city district referendum that had been the primary reason to go to the polls for some people.

as one would expect the existence of unobserved heterogeneity with respect to voting and information acquisition to overstate the importance of information. However, the results resonate with the findings of Dee (2003) and Milligan, Moretti, and Oreopoulos (2003), who estimate the causal effect of education on turnout. These studies also find the estimates to increase under IV-estimation, as do Brady, Verba, and Schlotzman (1995) when instrumenting political interest.

One likely reason for the result is the existence of measurement errors. Measurement error in an independent variable is known to lead to attenuation bias in the estimate. While measurement error is less likely to be a problem when considering education indicators or demographic variables, as these are well-defined, subjective assessments are much more likely to be prone to such problems. It is a general finding of the survey literature (Krosnick 1999) that respondents find it much easier to answer questions on past actions, such as whether one voted in a particular election, than to answer attitudinal question on topics subjects that may not have been given much thought. This means that some degree of randomness will enter into the answer of such questions, leading to measurement error. As the IV-approach, in addition to addressing the endogeneity problem, corrects for measurement error, the estimate increases.

Concluding Remarks

Theoretical work, with roots in observational empirical studies of voter behavior, has argued that being informed affects the propensity to vote. Using a unique natural experiment referendum, where a random fraction of the electorate was exogenously informed, the empirical analysis presented in this article suggests that information acquisition is endogenous and demonstrates that there is a causal effect of being informed on the propensity to vote in a referendum setting. The estimated effect is considerable: I find that the average treatment effect of being informed on the propensity to vote is 20 percentage points, which is more than the effect estimated by conventional methods.²⁶ The effect is stable over different configurations of

²⁶A rough estimate of the total effect of living in a pilot city district on the propensity to vote can be calculated as follows: ATE of informed on voting \times ATE of PCD treatment on being informed $= 0.213 \times 0.106 = 0.023$, where the former is the average over estimated ATEs from Table 4, and the latter is the estimated treatment effect of living in a PCD on being informed from the propensity score matching procedure. This number is very close to the actual estimated difference in voter turnout between the pilot and control city districts equal to $0.720 - 0.696 = 0.024$ (calculated from Copenhagen Statistical Office 2000a). OLS estimation on district

instruments and different samples; it is stronger for people with no cost of voting and appears to be driven primarily by increasing the propensity to vote for people who do not ordinarily vote in local elections. The natural experiment used here does not allow for distinguishing the decision-theoretic and game-theoretic approaches presented earlier; this may call for careful laboratory experiments, as the predictions of the various models differ in only subtle ways that can be difficult to accommodate in even random social experiments, but the results reported in this article can serve as a necessary first step in motivating the importance of such experiments by confirming the key hypothesis on real-life data.

The empirical results also suggested an indirect effect of education on turnout. As noted in the introduction, Milligan, Moretti, and Oreopoulos (2003) found, on U.S. data, that education does not influence the propensity to vote when conditioning on registered voters, a finding corroborated by the insignificance of education on turnout in British elections, where most voters are registered through local governments. In the Danish case considered here, where all eligible voters are automatically registered, a similar result of no direct effect of education on turnout was obtained. However, the empirical findings show a strong effect of education on being informed and, since being informed was shown to affect voting propensity in a causal fashion, this suggests that education, though indirectly, does contribute to a higher propensity to vote. Combined, these findings suggest that education enters directly into the calculus of voting by reducing expected utility costs associated with voter registration and information acquisition, rather than through contextual or socialization effects. Future research should investigate the relative importance of these different channels of influence in more detail for both general elections and referenda in a causal framework.

Appendix: Descriptive Statistics and Coding

Survey Questions and Coding

1. Did you vote in the last municipal election? (1 = Yes, 2 = No, 3 = Do not remember, 4 = Refuses to answer).
2. Did you vote in the referendum on the Economic and Monetary Union in September? and if so, what did you vote? (1 = Voted yes, 2 = Voted no, 3 = Did not vote, 4 = Voted, but will not say what for, 5 = Blank vote, 6 = Refuses to answer).
3. Did you vote in the referendum on city district reform in September? and if so, what did you vote? (1 = Voted

yes, 2 = Voted no, 3 = Did not vote, 4 = Voted, but will not say what for, 5 = Blank vote, 6 = Refuses to answer). Coding: Category 6 excluded from sample.

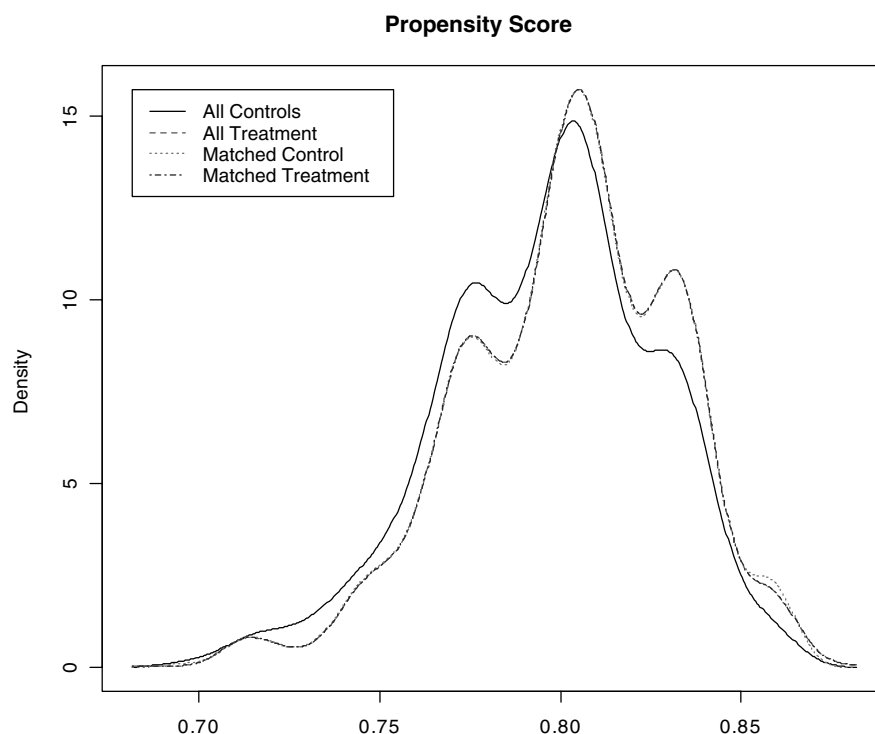
4. In the municipality of Copenhagen an experiment on city districts has been carried out in four districts. Would you say that this experiment went well, medium well or bad, or do you not have an opinion? (1 = Good, 2 = medium good, 3 = bad, 4 = no opinion) Coding: Opinion = 1, 2, 3.
5. Do you find that municipal council members are highly responsive, medium responsive, or not responsive to popular opinion, or do you not have an opinion? (1 = highly, 2 = medium, 3 = not, 4 = no opinion) Coding: Ordinal, 1, 2, 4, 3.
6. How interested would you say you generally are in political issues? (0 = little interest/don't know, 1 = medium interested, 2 = very interested).
7. Demographic questions: Gender, Age, Education (primary and lower secondary school, high school, college, master's degree, vocational training), yearly income (in thousands), employment (private, public, not employed), user of decentralized services (old-age care, child care, primary school, none).

Descriptive Statistics

TABLE A1 Descriptive Statistics

Variable	Obs.	Initial Sample		Weighted Sample	
		Mean	Std. Dev.	Mean	Std. Error
Turnout	2,870	0.846	0.361	0.739	0.018
Informed	2,870	0.646	0.478	0.526	0.018
PCD	2,870	0.801	0.400	0.275	0.006
Municipal voting	2,870	0.828	0.378	0.580	0.018
Euro voting	2,870	0.924	0.266	0.874	0.013
Gender (F = 2, M = 1)	2,870	1.552	0.497	1.555	0.017
Age	2,870	41.801	18.453	39.873	0.571
College education	2,870	0.489	0.500	0.480	0.018
Income	2,026	272.862	197.523	283.253	8.109
Public employment	2,870	0.287	0.452	0.293	0.015
Private employment	2,870	0.365	0.481	0.400	0.017
User of elderly care	2,870	0.121	0.326	0.106	0.011
User of daycare	2,870	0.194	0.395	0.176	0.013
User of primary schools	2,870	0.163	0.370	0.175	0.014
Political interest	2,870	1.089	0.677	1.027	0.026
Political responsiveness	2,870	2.902	1.042	2.859	0.037

level administrative data ($n = 14$) yields an estimate of 0.030 (S.E. 0.010).

FIGURE A1 Propensity Score Matching Across Districts

Propensity Score Matching

Propensity score matching is carried out as described in the text. Figure A1 shows the distribution of the estimated propensity scores for the different groups.

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