THE EFFECT OF REGISTRATION LAWS AND EDUCATION ON U.S. VOTER TURNOUT

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I show that restrictive registration laws do not dissuade individuals with lower levels of education from voting any more than individuals with higher levels of education. This finding contradicts the result reported in Wolfinger and Rosenstone's classic analysis of turnout. I show that their conclusion was actually an artifact of the methodology they employed. Examining predicted probabilities generated by a nonlinear model such as probit or logit may produce spurious results when used to determine interactive effects between two independent variables. By respecifying the model of turnout to explicitly include terms to test interactive hypotheses and reanalyzing the data from the 1972 Current Population Survey (as well as data from the 1984 survey), I show that in fact, no such substantive interactive effect between registration laws and individuals' level of education exists at the micro level.

I shall address a simple question: Do voting laws affect poorly educated individuals' turnout more than highly educated individuals' turnout? In their seminal work, *Who Votes?*, Wolfinger and Rosenstone (1980) noted that according to their estimates of a multivariate model of voter turnout in the 1972 presidential election, the severity of the voting law restricion had its largest impact on the least educated. They interpreted this to mean that there is an interactive effect at the micro level between the two factors. They explained that "formal education increases one's capacity for understanding and working with complex, abstract, and intangible subjects—that is, subjects like politics" and that "this heightened level of understanding and information would also reduce the cost of registering" (pp. 79–80).

Such a conclusion is seductively appealing. The idea that an individual facing a high cost of voting is less likely to vote is consistent with Anthony Downs's assertion that "a rational man decides whether to vote just as he makes all other decisions: if the return outweighs the costs, he votes; if not, he abstains" (1957, 260). However, I shall demonstrate exactly how Wolfinger and Rosenstone reached this conclusion. I show that it does not necessarily follow from the data analysis and is instead an artifact of the methodology used. Correcting the specification of their model, I show that increased education does not enhance individuals' abilities to comply with complex registration requirements any more than does any other variable Wolfinger and Rosenstone considered.

In most work on the effects of registration laws on voting turnout, it is not really the individual who is the center of attention. Rather, the substantive interest is in groups of individuals sharing a common trait: blacks, poor people, women, and so on. Groups sharing such traits are also likely to share preferences on policy issues, hence serious normative issues are raised if an institutional feature of the political system affects one group's propensity to vote more than another group's. The question becomes how to show the effect of registration laws on a *group* of individuals—in this case the group with a low level of education.

Interpreting Estimates from Nonlinear Models

While groups of individuals are ultimately of interest, actual analysis of voter turnout frequently proceeds at the individual level. Estimating the effects of several independent variables on a dichotomous (0/1) variable, such as the decision whether or not to vote, is usually done with either probit or logit analysis. Both the probit and logit models assume that while we only observe the values of zero and one for the variable Y, there is a latent, unobserved continuous variable Y^* that determines the value of Y.[1] We assume that Y^* can be specified,

$$Y_i^* = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki} + u_i$$

and that

$$Y_i = 1 \text{ if } Y_i^* > 0,$$
$$Y_i = 0 \text{ otherwise,}$$

where $x_1, x_2, \ldots, x_k$ represent vectors of random variables and u represents a random disturbance term.

If we make the usual assumption that the disturbances u are normally distributed then we can derive the probit model:

$$Pr(Y_i = 1) = \Phi(\beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki}) = \Phi(X_i \beta), \qquad (1)$$

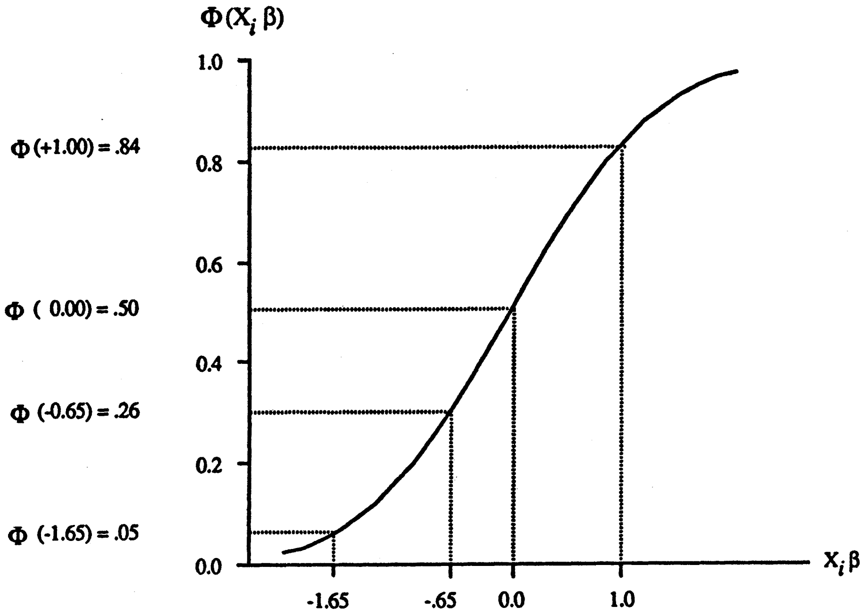
where Φ represents the cumulative normal distribution function.

In common parlance, both models assume an S-shaped response curve such that in each tail of the curve the dependent variable, $Pr(Y_i = 1)$, responds slowly to changes in the independent variables, while toward the middle of the curve (i.e., toward the point where $Pr[Y_i = 1]$ is closest to .5), the dependent variable responds more swiftly to changes in the independent variables (see Figure 1). Using maximum likelihood techniques we can compute estimates of the coefficients (βs) and their corresponding standard errors that are asymptotically efficient. However, these estimates cannot be interpreted in the same manner that normal regression coefficients are. These coefficients give the impact of the independent variables on the latent variable Y^*, not Y itself. To transfer Y^* into a probability estimate for Y we compute the cumulative normal of Y^*.

Because of this transformation there is no linear relationship between the coefficients and $Pr(Y_i = 1)$. Hence, the change in $Pr(Y_i = 1)$ caused by a change in x_{ji} will depend upon the value of all of the other xs and their corresponding coefficients, or, more precisely, on the value of the sum $X_i\beta$, as well as the change in x_{ji}. To simplify things, assume there is only one independent variable and that $\beta_0 = 0$, and $\beta_1 = 1$. Hence, the expression in equation 1 simplifies to $Pr(Y_i = 1) = \Phi(x_1)$. As the points on Figure 1 indicate, a change in x_1 from 0 to 1 causes a change in $Pr(Y_i = 1)$ of .34 (.84 − .50); while a change in x_1 from −1.65 to −.65 (also a change of only one unit) causes a change in $Pr(Y_i = 1)$ of .21 (.26 − .05). Hence, simply knowing the change in x cannot tell us the predicted change in $Pr(Y_i = 1)$; that change depends on where on the curve we start.

Imagine now that Figure 1 represents a model with two independent variables, x_1 and x_2. Now the effect of a given change

Figure 1. Predicted Probabilities Using the Cumulative Normal Distribution



in x_1 upon $Pr(Y_i = 1)$ will depend upon the sum $(x_1\beta_1 + x_2\beta_2)$. The effect of x_1 will be greatest when the sum $(x_1\beta_1 + x_2\beta_2)$ is closest to zero and weakest as $(x_1\beta_1 + x_2\beta_2)$ approaches $\pm\infty$. But this means that the effect of x_1 depends on the value of x_2. Or, without making any substantive inference about the relationship between x_1 and x_2, we have been able to conclude that they "interactively" affect $Pr(Y_i = 1)$. This interactive effect is assumed in the model specification. And the interaction is assumed to be greatest when $(x_1\beta_1 + x_2\beta_2)$ is closest to zero, or, when $\Phi(x_1\beta_1 + x_2\beta_2) = Pr(Y_i = 1)$ is closest to .5. This suggests the hazards of drawing inferences about relationships among independent variables from probit estimates of predicted probabilities. And this is precisely what confounds Wolfinger and Rosenstone's (1980) analysis.

Education and Closing

According to Wolfinger and Rosenstone's (1980) estimate of a subsample of the 1972 Current Population Survey, reported in Table 1, column 3, changing the registration laws would raise the average turnout rate of respondents with zero-to-four years of education by 8.2%, while the average turnout rate of respondents with five-plus years of college would go up only 1.9%. Wolfinger and Rosenstone (1980) used a complicated, but effective, technique to estimate these effects. First, they estimated a probit model to predict turnout with education, education-squared, age, age-squared, a dummy for the South, a dummy to denote the presence of a gubernatorial election in the state, and four measures of registration laws as independent variables: (1) a dummy to denote irregular hours for registration, (2) a dummy to denote the availability of Saturday and/or evening registration, (3) a dummy to denote the unavailability of absentee voting, and (4) the number of days before the election that registration closes.[2] They then used the estimated coefficients to compute a predicted probability of voting, $\widehat{Pr}_i = \Phi(X_i\beta)$ for each individual i in their sample. Following this, they made the appropriate change in the explanatory variable of interest (they reset the number of days to registration closing to zero) and recomputed the probability of voting for each individual. Call this new hypothetical probability $\widetilde{Pr}_i$. $\widetilde{Pr}_i$ gives the estimated probability of the i^{th} individual voting *if* there were no registration requirement. Each individual's actual values of the other independent variables are used to compute their own hypothetical probabilities; there is no reason to substitute values for any of the independent variables besides days to closing. In other words, $\widetilde{Pr}_i = \Phi(\tilde{X}_i\hat{\beta})$, where $\tilde{X}_i = [X_i | x_{ji} = 0]$, and x_j is the variable of interest (closing) that is being hypothetically set to zero. The impact of the registration requirement on the i^{th} individual is arrived at by subtracting the first number ($\widehat{Pr}_i$) from the second number ($\widetilde{Pr}_i$).

Table 1, columns 4 and 6 show the results I obtained by applying the probit model just described to the entire sample and computing the change in expected voting rates for different education groups (as well as different income and age groups) under two different assumptions: (1) that the closing requirement for registration is removed (i.e., set to zero) and (2) that all states allow some form of absentee voting. Since hypothetical probabilities and actual probabilities have been computed for everyone in the sample, it is simple to present the results aggregated over any subsample, as opposed to presenting the result for the entire sample. According to these estimates, if the closing date were eliminated, an additional 6.2% of persons with four or fewer years of education would vote, while turnout would increase among those with five or more years of college by only 2.2%. The 6.2% and 2.2% figures are arrived at by averaging the change in probability experienced by each individual in the respective categories (and multiplying by 100).

To make individual-level inferences from these results would, however, represent a misinterpretation of the probit estimates. As we know, because of the shape of the cumulative normal distribution, probit estimates are most sensitive to the impact of any variable for the respondents closest to probability .5 of voting. The effect of changes in the closing date Wolfinger and Rosenstone (1980) report are biggest within each category for the lowest turnout group; that is, the effect is biggest on the least-educated, lowest-income-earning, and youngest (Table 1, col. 3). Coincidentally, for two of the three categories in Table 1 (income and age) the lowest turnout group was also the group closest to 50% in turnout. Among the education groups, the group closest to 50% in expected voting rate was the next-to-least-educated group: those with five to seven years of education, whose expected voting rate was 42.2%. That Wolfinger and Rosenstone report a larger increase for the least-educated group, whose predicted voting rate was 32.2%, rather than the second-least-educated group, who had a predicted voting rate of 42.2%, can only result from differing predicted voting rates for these groups within the subsample of the data set they used for their analysis.[3]

Table 1, column 4 reports the predicted change in voting rates based on analysis of the entire sample rather than the subsample used by Wolfinger and Rosenstone (1980). These figures correspond with the predicted voting rates offered in column 2. The predicted change for the least-educated group is less than the predicted change for the next-least-educated group, which is higher than the predicted change for all other education groups. Thus, to the extent that those with less education are nearest to the .5 mark in their expected probability of voting (or become nearest to the .5 mark in their expected probability of voting after altering a different independent variable), they will be more affected than those with higher education levels by changes in *any* independent variable.

This problem is easy to see if one considers it in terms of the normal density curve illustrated in Figure 1. Since persons with the lowest education level are those who on average are closest to having .5 probability of voting, estimates of change based on that group will necessarily be larger than estimates of change for any other group. Thus, the smaller effects of closing on better-educated groups is simply an artifact of the changing slope of the normal density curve, not a result of a unique relationship between individuals' education and state registration requirements. Hence, to infer substantive interaction between registration laws and education based on the differing estimated impacts is incorrect. This interaction is *assumed* by the model specification. This

Table 1. Predicted Change in Voting Rate for Selected Samples of the Population Based on Changes in Voting Laws

			Predicted Change			
			Closing Days Set to Zero		Absentee Balloting Allowed	
Group	CPS Sample Voting Rate	Predicted Voting Rate[a]	*Who Votes?*[b]	Final Model, 1972	*Who Votes?*[b]	Final Model, 1972
Education (years)						
0–4	32.9	32.2	8.2	6.2	1.0	.209
5–7	44.9	42.2	7.9	6.4	.9	.229
8	57.0	56.2	6.9	5.8	.6	.124
9–11	53.7	59.0	7.0	5.7	.6	.142
12	66.1	66.1	6.1	5.2	.5	.111
1–3 college	76.0	72.1	5.4	4.8	.4	.094
4 college	83.6	83.2	3.8	3.6	.4	.088
5+ college	87.4	90.8	1.9	2.2	.1	.046
Income						
Under $2,000	45.1	54.7	7.1	5.7	1.0	.221
$2,000–7,499	55.1	59.9	6.6	5.5	.6	.150
$7,500–9,999	63.3	63.6	6.3	5.3	.5	.115
$10,000–14,999	71.6	67.7	5.8	5.0	.4	.099
$15,000–24,999	79.7	73.0	5.1	4.5	.3	.072
$25,000+	83.4	76.7	4.3	4.0	.2	.072
Age						
18–24	51.9	52.7	7.2	5.9	.5	.128
25–31	60.0	61.2	6.7	5.6	.6	.132
32–36	64.3	65.6	6.0	5.2	.5	.130
37–50	70.5	70.2	5.5	4.8	.5	.112
51–69	73.0	70.7	5.3	4.6	.5	.111
70–78	66.5	65.5	6.0	5.0	.5	.116
79+	50.9	60.2	6.8	5.4	.5	.118

[a]Predicted voting rate for entire CPS sample based on replication of Wolfinger and Rosenstone 1980, App. F.
[b]From Wolfinger and Rosenstone 1980, Table 4.2.

result is not an artifact of the averaging procedure used by Wolfinger and Rosenstone. If we compared the changes caused by the closing law on individuals with mean or modal values for all variables except education, we would still find a larger change for the little-educated individual than the well-educated individual when the value of the registration requirement increases.[4]

For comparison, examine the estimated effect of the availability of absentee voting. It is unlikely that absentee voting is primarily a tool of the poorest and least-educated. It seems far more likely that absentee voting is utilized most by the well-educated and affluent. However, examining the results in Table 1, we observe the most severe effect on the poorest and least-educated. Again, we are merely observing an artifact of the methodology. It would be wrong to change prior beliefs about the relationship between absentee voting and socioeconomic status based on these results. I shall respecify the model to allow for an explicit test of the hypotheses that registration laws most severely affect poorly educated individuals.

Two Proper Interactive Tests

I use two statistical tests to determine whether registration requirements depress turnout for little-educated groups more than would be predicted based simply on the aggregate turnout level of the group (i.e., based on the group's proximity to a 50% turnout rate). The most common technique used to test for interactive effects between two variables x_j and x_k is to assume that such effects are multiplicative and to add a term equal to the product of the two variables ($x_j \times x_k$) as an independent variable (Jaccard, Turrisi, and Wan 1990). A second technique is to disaggregate the data based on the value of x_j (or x_k) and compare the coefficients of x_k (or x_j) for the different samples. To add to the reliability of the results and because of the counterintuitive nature of the findings for 1972, I also analyze census data from 1984 using both techniques.

Consider a simple model of turnout where education and closing are the only independent variables, and an interactive term is included in the model:

$$Y_i^* = \beta_0 + \beta_1\text{education}_i + \beta_2\text{closing}_i + \beta_3(\text{education}_i \times \text{closing}_i) + u_i. \quad (2)$$

The total effect on Y_i^* of a one-unit increase in the closing variable is given by

$$\Delta Y_i^* = \beta_2 + \beta_3 \times \text{education}_i.$$

Since β_2 is conjectured to be negative (the further from the election registration closes, the less likely an individual is to vote), if β_3 is positive, then the contribution of the last term will *decrease* the total effect of closing on Y_i^*. And the larger this last term is (i.e., the higher the individual's level of education), the more the effect of closing will be attenuated. Table 2 reports estimates of the Wolfinger and Rosenstone model with the addition of (1) the multiplicative interactive term *education × closing,* (2) the multiplicative interaction term *education-squared × closing,* and (3) both multiplicative interactive terms.

Looking at the third column of Table 2, the estimated value of the coefficient of the interactive term is negative and significant ($t = -3.38$).[5] This is a perverse result, suggesting that the higher an individual's level of education, the more the effect of closing will be *increased.* The estimates of the model including *closing × education-squared* offer similar results; the coefficient of the interactive term is again negative and significant ($t = -2.97$).

Interpretation of the fuller model including both interactive terms is slightly

Table 2. Estimates of the Final Model, 1972

Independent Variable	Wolfinger & Rosenstone[a]		Replication of Wolfinger & Rosenstone		Inclusion of Interactive Term(s): First		Inclusion of Interactive Term(s): Second		Inclusion of Interactive Term(s): First & Second	
	Estimated Coefficient	t-ratio	Estimated Coefficient	t-ratio	Estimated Coefficient	t-ratio	Estimated Coefficient	t-ratio	Estimated Coefficient	t-ratio
Intercept	-2.7001	-11.20**	-2.4928	-53.74**	-2.6562	-39.62**	-2.5759	-47.55**	-2.7597	-26.51**
Education	.1847	15.39**	.2635	20.52**	.3003	17.83**	.2654	20.65**	.3544	7.90**
Education-squared	.0120	2.40**	.0035	2.56**	.0033	2.36**	.0067	3.83**	-.0029	-.59
Age	.0707	15.71**	.0653	48.69**	.0652	48.63**	.0653	48.66**	.0652	48.60**
Age-squared	-.0006	-6.00**	-.0005	-35.93**	-.0005	-35.83**	-.0005	-35.86**	-.0005	-35.79**
South	-.1371	-3.32**	-.1935	-15.03**	-.1936	-15.05**	-.1934	-15.03**	-.1939	-15.07**
Hours	.0336	2.11**	—	—	—	—	—	—	—	—
Gubernatorial election	.0634	1.88*	.0682	6.66**	.0683	6.67**	.0682	6.66**	.0686	6.70**
Irregular registration hours	-.1005	-2.29**	-.0155	-.97	-.0137	-.86	-.0141	-.88	-.0134	-.84
Evening/Saturday registration	.1253	3.63**	.1009	10.10**	.1009	10.10**	.1009	10.10**	.1010	10.11**
No absentee registration	-.0909	-2.26**	-.0291	-1.92*	-.0295	-1.95*	-.0296	-1.95*	-.0293	-1.94*
Closing date	-.0073	-4.87**	-.0062	-10.27**	-.0005	-.26	-.0033	-2.93**	.0032	.96
Closing date × education	—	—	—	—	-.0012	-3.38**	—	—	-.0032	-2.07**
Closing date × education-squared	—	—	—	—	—	—	-.0001	-2.97**	.0002	1.30
Number of cases	7,936		90,279		90,279		90,279		90,279	
Percent voting	66.7		65.30		65.30		65.30		65.30	
Correctly predicted	71.4		70.62		70.63		70.64		70.61	
Log-likelihood	-577		-51,915		-51,909		-51,911		-51,909	

[a]From Wolfinger and Rosenstone 1980, App. F.

**$p \leq .05$, two-tailed test.
*$p \leq .10$, two-tailed test.

more complex because of the opposite signs of the coefficients of the terms. Here the change in Y_i^* based on a one-unit increase in closing can be given by

$$\Delta Y_i^* = .0032 - .0032 \times \text{education}_i + .0002 \times \text{education-squared}_i. \quad (3)$$

First, since education is only scaled from zero to eight, the value of ΔY_i^* given by the right-hand side of equation 3 will be negative for any value of education. Hence, increases in closing have the predicted effect of decreasing the probability of voting. Second, over this range of possible values of education, any increase in education levels will *decrease* the value of the right-hand side of equation 3. This means that an additional unit of education will again result in an *increase* in the tendency of closing to decrease an individual's likelihood of voting; or, individuals with higher levels of education will be more affected by the closing date than will individuals with low levels of education. (See Appendix for a more technical discussion of this model.)

I estimated similar models for 1984.[6] The pattern of the coefficients of closing and the interactive terms is identical (Table 3). When added individually each interactive term has a negative coefficient. And when both interactive terms are included in the model, increased education still *increases* the effect of closing over the relevant range of the variable. Log-likelihood ratio tests also indicated that the full models with interactive terms are significant improvements over the initial specification. The calculated chi-squared statistic is significant at the 99% level for 1972 and 95% level for 1984.

Disaggregation

A simpler alternative to specifying the multiplicative interactive term is to disaggregate the sample by the variable of interest. By estimating the same model for the entire sample, one is assuming that the effects of each independent variable are fixed across all members of the sample or, in the case of probit, across all members of the sample with the same initial probability of voting. Disaggregating the model explicitly allows the coefficients to vary. Given a large-enough sample, this method allows us to examine the conjectured pattern of interaction much more closely. Table 4 presents the results of the same models presented in Tables 2 and 3; but the education terms are removed and, instead, the model is estimated at 10 different levels of education: 0–4 years, 5–7 years, 8 years, 9–11 years, 12 years, 1–3 years of college, 4 years of college, 5+ years of college, 0–11 years, and 12+ years. Rather than presenting all the coefficients for the 20 different sets of estimates, only the coefficient of interest (i.e., closing) is presented for each subsample.

These results are consistent with the counterintuitive result from the interactive terms. Comparing the last two groups—those without a high school degree and high school graduates—we see that the magnitude of the coefficient of closing is larger in the *latter* group for both 1972 and 1984: −.00574 versus −.00669, and −.00598 versus −.00726, respectively. This suggests that more-educated persons have a *harder* time than less-educated persons in dealing with the burden of registration. The first eight rows of the table explain this result in more detail. The effect of the registration requirement generally increases as respondents' education levels approach a high school education and does not tail off until individuals acquire some college education. Individuals with a high school education are more affected by registration requirements than individuals without high school degrees. Yet individuals with college degrees are less affected than individuals with only high school degrees. The pattern is identical for both years.

Table 3. Estimates of the Final Model, 1984

Independent Variable	Replication of Wolfinger & Rosenstone		Inclusion of Interactive Term(s): First		Second		First & Second	
	Estimated Coefficient	*t*-ratio	Estimated Coefficient	*t*-ratio	Estimated Coefficient	*t*-ratio	Estimated Coefficient	*t*-ratio
Intercept	-2.5241	-51.47**	-2.5919	-39.78**	-2.5501	-47.04**	-2.7565	-25.68**
Education	.1806	12.42**	.1941	11.51**	.1806	12.42**	.2673	6.44**
Education-squared	.0124	8.63**	.0124	8.61**	.0133	8.01**	.0048	1.17
Age	.0696	53.10**	.0696	53.09**	.0696	53.09**	.0695	53.06**
Age-squared	-.0005	-37.47**	-.0005	-37.45**	-.0005	-37.46**	-.0005	-37.41**
South	-.1129	-10.81**	-.1127	-10.79**	-.1128	-10.80**	-.1124	-10.76**
Gubernatorial election	-.0213	1.97**	.0211	1.95**	.0211	1.95**	.0218	2.01**
Closing date	-.0076	-17.20**	-.0050	-2.88**	-.0066	-6.50**	.0014	.37
Closing date × education	—	—	-.0005	-1.58	—	—	-.0034	-2.23**
Closing date × education-squared	—	—	—	—	-.0004	-1.12	.0003	1.93*
Number of cases	98,860		98,860		98,860		98,860	
Percent voting	67.01		67.01		67.01		67.01	
Correctly predicted	71.04		71.04		71.05		71.05	
Log-likelihood	-55,375		-55,374		-55,374		-55,372	

**$p \leq .05$, two-tailed test.
*$p \leq .10$, two-tailed test.

Table 4. Comparison of the Effect of Closing for Different Education Levels

	1972			1984		
Education Level (years)	Coefficient of Closing	*t*-ratio	*N*	Coefficient of Closing	*t*-ratio	*N*
0–4	−.00121	−0.37	3,216	−.00296	−0.72	1,495
5–7	−.00335	−1.40	5,234	−.00530	−1.97**	2,995
8	−.00279	−1.56	8,922	−.00701	−4.20**	5,459
9–11	−.00425	−2.84**	14,234	−.00463	−3.61**	11,784
12	−.00807	−8.17**	32,471	−.00872	−12.85**	38,389
1–3 college	−.00705	−4.42**	14,487	−.00764	−7.82**	20,522
4 college	−.00724	−2.86**	6,910	−.00769	−4.83**	10,510
5+ college	−.00679	−2.04**	4,805	−.00538	−2.65**	7,706
0–11	−.00574	−5.92**	31,606	−.00598	−6.52**	21,733
12+	−.00699	−9.19**	58,673	−.00726	−14.55**	77,127

Note: The model from Wolfinger and Rosenstone 1980, App. F (with the education variables omitted) was used to generate these coefficients.

**$p \leq .05$, two-tailed test.

Such a pattern sharply contradicts the theory that an increase in an individual's level of education increases his or her ability to adapt to registration requirements.

Conclusion

We generally think of interactive effects as having substantive meaning. When we say that there is an interactive effect between education and registration requirements, we want it to mean that an individual's level of education will determine how he or she is affected by registration requirements. And we ought to have a meaningful explanation as to why there should be such an interaction. One is readily available in this case, that poorly educated individuals will have difficulty with a complex registration requirement.

However, once such a relationship has been specified, we need to test it *explicitly* within the estimation framework we are using. Nonlinear models impose an "interactive" relationship on all of the independent variables. We are not interested in this assumed relationship among all the independent variables. We are interested in the relationship between only *two* independent variables: education and registration requirements. To disentangle this relationship requires adding an interactive term involving only these two variables to the underlying model or disaggregating the sample and comparing estimated coefficients rather than predicted probabilities. The analytical and quantitative analyses have demonstrated that using nonlinear models to show substantive interactive effects between independent variables is a hazardous exercise. I have also shown a straightforward method of overcoming this hazard while retaining the use of common nonlinear models such as probit and logit.

The modest contribution of the empirical research presented here is to show that what was thought to be a fact, namely, that poorly educated persons are more deterred from voting by registration laws than well-educated persons, is not a fact. This claim has become commonplace in introductory texts on U.S. government (e.g., Johnson et al. 1990, 244; Patterson 1990, 207). However, for 1972 and 1984 both tests of the differential effects of closing on education groups lead us to reject the contention that there is an individual-level interaction between education and the closing date that makes little-educated individuals more sensitive to registration impediments. Use of an interactive term did not produce the predicted coefficient. And when individuals were broken down by level of education, the less-educated individuals were not found to be especially sensitive to changes in the closing date.

The broader substantive point argued here is that if we accept the assumptions of the probit model, changes in *any* systemic feature that raises turnout will raise it disproportionately among less-educated individuals. To concentrate on registration laws as a means to address the imbalance in voting rates between the poorly educated and well-educated would be a potentially inefficient means of achieving a specified public policy outcome.

I have concentrated on refuting the individual-level assertion made by Wolfinger and Rosenstone (1980). However, the aggregate assertion—that the less-educated as a group are more affected by restrictive registration laws—is no less a result of Wolfinger and Rosenstone's assumption of the probit specification. This specification assumes that individuals at the extremes are fixed in their propensity to vote. However, if one wishes to test *which* individuals are most affected by changes in explanatory variables, then the probit model is not the answer; for when using probit, any grouping procedure that segregates people based on their value of the dependent variable (or based on a characteristic correlated with the dependent variable) will produce a predetermined result: the group closest to .5 probability on the dependent variable will experience the largest impact of any group for changes in any independent variable.

Should there be a reason to question the assumptions of the probit or logit model substantively, the researcher may wish to explore other functional forms. The probit model is a result of assuming normality of the disturbances. Going back to equation 1, *any* cumulative density function could have been assumed for the disturbances. This includes cumulative density functions that are not symmetric about zero. In other words, this includes cumulative density functions that would correspond to situations where individuals with very low or very high—rather than close to .5—probabilities of voting respond most to changes in the independent variables. The exploration of such models would allow us to test an important, but currently untested, behavioral assumption.

Appendix: Testing for Interaction

We say that there is an interactive effect among x_1 and x_2 if the effect of x_1 on Y depends upon the value of x_2. Technically, this means that there will be an interactive effect if either $\partial Y/\partial x_1$ is dependent upon x_2 or

$$\frac{\partial(\partial Y/\partial x_1)}{\partial x_2} = \frac{\partial^2 Y}{\partial x_1 \partial x_2} \neq 0.$$

In the probit specification, where the dependent variable is $Pr(Y_i = 1)$, this is given by

$$\frac{\partial^2[Pr(Y_i = 1)]}{\partial x_1 \partial x_2} = \frac{\partial^2[\Phi(X_i\beta)]}{\partial x_1 \partial x_2}$$
$$= -\beta_1\beta_2(X_i\beta)\phi(X_i\beta).$$

As this is generically nonzero, there is generally an interactive effect between *any* two variables x_1 and x_2 in the probit model. However, this is not the case for the underlying linear model. Now consider the general linear model with multiplicative interactive terms:

$$Y_i^* = \alpha_0 + \beta_1 x_{1i} + \ldots + \beta_k x_{ki} + \beta_{k+1}(x_{li} \times x_{ji}) + u_i.$$

Here the interactive effect between x_1 and x_j is given by

$$\frac{\partial^2 Y^*}{\partial x_1 \partial x_2} = \beta_{k+1}.$$

If β_{k+1} is positive, it indicates that the effect of x_1 on Y^* increases as x_j increases. If β_{k+1} is negative, it indicates that the effect of x_1 on Y^* decreases as x_j increases. Now consider the specific case of the complete model from Table 2. We had

$$Y^* = Z + \text{closing} \times (.0032 - .0032 \times \text{educ} + .0002 \times \text{educ}^2).$$

Therefore,

$$\frac{\partial Y^*}{\partial \text{closing}} = .0032 - .0032 \times \text{educ} + .0002 \times \text{educ}^2. \qquad \text{(A-1)}$$

This is the effect of closing on Y^*. This effect clearly depends upon education. To see how the effect of closing *varies* with education, we want to see how the right-hand side of equation A-1 changes as education changes, that is, we want to evaluate

$$\frac{\partial^2 Y^*}{\partial \text{closing}\, \partial \text{educ}} = -.0032 + .0004 \times \text{educ}.$$

We are only interested in the sign of this expression. For educ < 8, it is negative. This indicates that as education increases, the effect of closing on Y^* is decreasing, or becoming *more* negative. In other words, as education increases, the magnitude of the deterrent effect of closing on voting is *increasing*.

Notes

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1. See Madalla 1983 for a discussion of limited dependent variable models generally; see pp. 22–27 for a concise exposition of probit and logit. Goldfeld and Quandt (1972, 132–34) offer some empirical comparisons of probit and ordinary least squares estimates.

2. The only variable from Wolfinger and Rosenstone's model omitted here is the number of hours that the polls are open on election day. This number was not readily available for all 50 states; and since the estimates generated without it are quite close to Wolfinger and Rosenstone's own estimates, I did not feel that the expense of collecting the additional data was justified.

3. Barely 3% of the sample is in the least-educated category, so that of the 7,936 cases Wolfinger and Rosenstone used to generate their estimates, only 240 of them would be expected to be in this category. It is thus well within reasonable sampling error to think that the least-educated group in their subsample had a higher predicted voting rate than the next-least-educated group in their subsample.

4. Jacobson uses an equivalent graphical technique with a logit model to show which voters are more affected by challenger campaign spending (1990, 353–55). He recognizes the problem with the technique and offers the appropriate caveat on interpretation.

5. Adding the interactive term *education* × *closing* and removing the *education-squared* term gave the same result: the interactive term was again negative and significant ($t = -3.52$). The same was true for 1984 ($t = -1.70$); thus, the perverse result cannot be attributed to the presence of the quadratic education term. This is reinforced by the disaggregated estimates reported in Table 4, where the *education-squared* term is also absent. Finally, log-likelihood tests indicate that including the *education-squared* term did yield the significantly better ($p < .05$) specification.

6. Three additional measures of registration laws—irregular hours, evening or Saturday registration, and absentee balloting—were absent from these models. Since Wolfinger and Rosenstone showed that closing is by far the most important of the registration variables, I do not think that this is a major omission. In addition, the 1984 estimates omit noncitizens from the sample.

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