

**REVISITING THE RELATIONSHIP
BETWEEN SECRET BALLOTS AND TURNOUT
A New Test of Two Legal-Institutional Theories**

JAC C. HECKELMAN
Wake Forest University

Two theories within the legal-institutional framework concerning the Australian ballot system's effect on voter turnout are analyzed. The vote market hypothesis assumes secret ballots were designed to end the buying and selling of votes. The secrecy the new ballot provided discouraged candidates from buying votes they could no longer verify, disproportionately affecting poor voters who would respond to this loss of payments by voluntarily abstaining. Alternatively, the theory of strategic disfranchisement predicts Blacks and illiterates were specifically targeted for disfranchisement. The new ballots were expected to be more difficult for these voters to use and they would then be effectively prevented from participating in the active electorate. Although turnout decreases under either theory, the normative implications are very different. Controlling for race and illiteracy, regression analysis suggests poor voters were less likely to vote a secret ballot. A similar effect is not found for Black and illiterate voters when controlling for income. The evidence is thus more consistent with the vote market hypothesis than with a pure disfranchisement effect.

No arrangement of the suffrage, therefore, can be permanently satisfactory, in which any person or class is peremptorily excluded; in which the electoral privilege is not open to all persons of full age who desire to obtain it.

—John Stuart Mill (1861, p. 170)

It is much rather an object to prevent those from voting who are indifferent to the subject, than to induce them to vote by any other means than that of awakening their dormant minds. The voter who does not care enough about the election to go to the poll, is the very man who, if he can vote without that small trouble, will give his vote to the first person who asks for it . . . A man who does not care whether he votes, is not likely to care much which way he votes; and he who is in that state of mind has no moral right to vote at all.

—John Stuart Mill (1861, footnote, p. 219)

Author's Note: I thank Keith Dougherty, John Dinan, and two anonymous referees for a careful reading of earlier versions and suggestions that improved the exposition of the text. The usual caveat applies.

AMERICAN POLITICS QUARTERLY, Vol. 28 No. 2, April 2000 194-215
© 2000 Sage Publications, Inc.

Empirical evidence has consistently shown that secret (or Australian) ballots, introduced in many states prior to the turn of the century, were responsible for a marked reduction in voter turnout rates (Heckelman, 1995; Kenny & Lott, 1997; Kousser, 1974). The rationale offered for this result differs among various studies.

On one hand, behavioral theorists have argued party identification is the primary contributor to electoral participation. Some believe the relative strength of the major parties was altered when nondemocratic reforms were implemented primarily to reduce competition, which thereby solidified partisan control and alienated voters. Australian ballots may have contributed to this phenomenon by removing party control for ballot creation and distribution, thereby lessening partisan mobilization. Alternatively, the legal-institutional approach argues secret ballots were directly responsible for turnout reductions by erecting barriers to the desired participation of the voters themselves. Traditionally, debate has focused on the behavioral versus legal-institutional approach to explaining declining turnout (e.g., Piven & Cloward, 1989). Here, the focus will be on formally differentiating between two competing legal-institutional theories that have markedly different normative implications concerning the secret ballot.

Early studies concerning the secret ballot accepted the notion that rampant corruption and fraudulent voting practices forced legislators to adopt various measures of reform (Evans, 1917; Harris, 1929, 1934). Under secrecy, voters were free to vote as they had originally intended, regardless of any implicit vote contract. No longer able to monitor the voters' actions, candidates were less willing to offer money in exchange for their votes. Viewed in this vein, the secret ballot was used to block trades in the active market for votes. An extension of this analysis suggests the vote payments may have acted as an important motivating force for voting (Heckelman, 1995; Rusk, 1974).¹ The vote market hypothesis thus implies some voters would respond to this loss in income from the absence of payments by abstaining.

In an important deviation from this perspective, Kousser's (1974) study of Southern politics predicted a partisan advantage for the Democrats in power from passing the state secret ballot laws. Blacks and illiterate Whites generally favored the Republicans and, accord-

ing to Kousser, Democrats designed the secret ballot to strategically eliminate them from the voting arena altogether. Prior to the adoption of Australian ballots, voting was typically conducted by voice or by choosing easily identifiable separate party ballots. The Australian ballot system contained all offices and candidates on a single ballot making voting a very cumbersome procedure for those who struggled with the written word. Australian ballots were thus thought to be one "of the most effective ways of eliminating Negroes from politics" (Thomas Hardwick, as cited in Kousser, 1974, p. 53). If Blacks and illiterates were unable to correctly read and mark these new ballots, overall voting would decline due to the loss of these previously active partisan voters.

Thus, both legal-institutional theories lead to the same prediction that overall turnout rates would be lower for elections requiring secret ballots. Kenny and Lott (1997) mention both possibilities but do not attempt to distinguish between them in their empirical work. The difference between the two effects, however, is important. Although both theories predict lower turnout, the vote market hypothesis explains how voters removed themselves from the active electorate when secret ballots were used, whereas Kousser's (1974) disfranchisement theory describes a legislative process in which active voters lost their ability to vote through the adoption of secret ballots, similar to the known effect of poll taxes and literacy tests.

Concern over the fall in turnout since the late 19th century has been expressed in various studies (Abramson & Aldrich, 1982; Burnham, 1965; Converse, 1972). An implication from the vote market hypothesis suggests this concern may be somewhat misplaced. If the only true consideration is a higher turnout rate, elections could return to an open ballot system. According to the vote market hypothesis, this would allow citizens to sell their votes at a premium high enough to cover all their voting costs. Turnout would then increase, and competition for voters might also drive vote prices high enough to compensate them for voting against their most preferred candidates.

Of course, it is an untenable position to argue that the presence of a vote market is a public good. The preceding discussion should not be interpreted as advocating the return of vote selling. The important point is that consideration should be given not only to the absolute percentage of voters but also to the quality of the voting decisions. If dis-

franchisement occurred as a result of secret ballot reform, then this legislation prevented core voters from being able to contribute to the democratic process.

According to the vote market hypothesis, however, the voters that dropped out were only those who could no longer sell their votes. Without a monetary stimulus, they were not willing to take the time to vote. Thus, the voters that remained active were those who had always taken an active concern in the outcome of the election and were not voting solely to receive payment. The total number of voters fell, but the quality of the average voter may have increased when these marginal voters abstained. If these people were not willing to vote of their own volition, there is no reason to think they took the time to make informed decisions. The secret ballot may have actually improved the quality of societal decisions by reducing the incentives for marginal voters who were unlikely to have invested much time gaining information.

This study represents an attempt to empirically differentiate between these two legal-institutional theories of ballot-driven declining turnout, because the normative implications concerning adoption of the secret ballot rest on being able to distinguish between the vote market and the disfranchisement effect. It may be difficult to differentiate between the two effects. Both theories predict lower turnout as a result of adopting the Australian ballot, but different types of voters would be affected. Anderson and Tollison (1990) explained the voters accepting bribes were predominantly the poor, because they tend to be disorganized and face higher transaction costs. In an active vote market, the poor should be the ones expected to receive, not dispense, bribes. This suggests the poor were more likely to abstain once the vote market collapsed. Kousser's (1974) disfranchised voters were specific to Blacks and illiterates who could not read and properly mark the new ballots and hence were forcibly removed from the voting population in a similar manner as from the requirements of paying poll taxes and passing literacy tests. Thus, the interpretation of the normative implications of this particular institutional reform would turn on the types of voters who stopped voting when the Australian ballot was adopted. The individual-level empirical model of this article, aggregated by necessity, attempts to determine the primary characteristic for classifying the new abstainers. Determining legislative intent is

beyond the scope of this study. Rather, the focus here is limited to the actual impact of the secret ballot.

The rest of the article is structured as follows. An empirical model of voting is derived in the next section, along with consideration of several statistical issues. The secret ballot income effect from the vote market hypothesis is tested in the third section, followed by testing the secret ballot disfranchisement effect for Blacks and illiterates in the fourth section. Conclusions appear in the fifth section.

EMPIRICAL METHODOLOGY

It is assumed that the probability of an individual voting follows a logistic distribution of the form

$$P_{ist} = \frac{e^{(X_{it}\beta + Z_{it}\gamma)}}{1 + e^{(X_{it}\beta + Z_{it}\gamma)}} \quad (1)$$

where P_{ist} is the probability of voting, X_{it} is the vector of state-level variables (state laws), and Z_{it} is the vector of personal characteristics for each individual i in state $s = 1, \dots, S$ at time $t = 1, \dots, T_s$.

Due to the time period under consideration, data are not available to detail individuals. The ecological fallacy literature views the use of aggregate-level proxies for the individual-level variables as problematic. The range of problems associated with ecological regression are detailed in King (1997), but an alternative procedure used here based on the conditional aggregation model for logit analysis in Kelejjan (1995) mitigates these problems.²

First, ecological estimates may be hampered by spurious correlation. This can be adequately handled by using a proper set of control variables. The list of variables used here includes the state electoral laws and various personal socioeconomic characteristics, as described below. Second, linear estimation procedures can result in predicted turnout rates outside the logical [0,100] range, which indicates a serious model or methodological misspecification. This problem is particularly common when the dependent variable contains values in the tails of the distribution. The advantage to the nonlinear logistic equation in Equation 1 is that it automatically restricts all estimates to the

logical range. However, it also means that direct aggregation of Equation 1 over individuals is not possible. Third, aggregation bias may alter coefficient estimates. Kelejian's aggregate logit model provides a direct test for aggregation bias and yields estimates that in the absence of aggregation bias are the same as the individual-level parameters of interest. Finally, King (1997) warns that distributions conditional on unknown quantities are not useful for empirical inferencing. Kelejian's (1995) procedure generates estimates that are conditional only on the observed aggregate data.

Kelejian's (1995) methodology can be summarized as follows. First, rearranging Equation 1 yields

$$\ln\left(\frac{P_{ist}}{1-P_{ist}}\right) = X_{ist}\beta + Z_{ist}\gamma. \quad (2)$$

Although P_{ist} and Z_{ist} are unknown and direct aggregation is not possible, Kelejian shows the aggregate equivalent to Equation 2 can be represented by

$$\ln\left(\frac{\dot{P}_{st}}{1-\dot{P}_{st}}\right) = X_{st}\beta + \dot{Z}_{st}\gamma + g(X_{st}\beta + \dot{Z}_{st}\gamma) + e_{st} \quad (3)$$

where the vectors \dot{P}_{st} and \dot{Z}_{st} contain the state-level averages and e_{st} is an observation-specific error term resulting from aggregate approximations. The parameter estimates in Equation 3 can be consistently estimated, conditional only on the observed values in X_{st} and \dot{Z}_{st} .

VARIABLES AND DATA SOURCES

The dependent variable in Equation 3 is a monotonic transformation of the state turnout rate. State-level electoral data are available from the Inter-university Consortium for Political and Social Research (Burnham, Clubb, & Flanigan, 1971). The data set contains the total number of votes cast in gubernatorial elections from 1824 to 1972. The sample used in this article covers gubernatorial elections from 1876 to 1910.³ The panel of states is unbalanced due to differences in the duration of gubernatorial term length and the starting sample date predating statehood for certain states. The state turnout

rate is estimated as the number of votes cast divided by the gender-and/or age-eligible voting population size.

Based on the literature analyzing turnout in this period, the state laws considered here as explanatory variables include the presence of secret ballots, poll taxes, literacy tests, and female suffrage. Poll taxes directly increased the cost of voting, and literacy tests structurally disfranchised illiterate voters (Heckelman, 1995; Kenny & Lott, 1997; Rusk & Stucker, 1978). A few Western states extended full suffrage to women prior to the 19th Amendment, and women are usually assumed to vote in lower frequency when they first became enfranchised (Heckelman, 1995; Kenny & Lott, 1997; Kleppner, 1982).

One institutional factor not considered here is the introduction of registration requirements. The motivation behind registration adoption is still controversial. Similar to the secret ballot case considered here, registration may have been enacted as a progressive measure to curb fraudulent (and repeat) voting (Converse, 1972) or instead to strategically try to weaken the minority parties' ability to mobilize its voters (Burnham, 1974). (See also the discussion in Piven and Cloward, 1989). In a study of county-level data on non-Southern states, Kleppner and Baker (1980) showed the implementation of voter registration in the post-1900 period decreased turnout but its effects were only limited. (See also Burnham, 1974, for a similar conclusion.) Most states, in fact, initially enacted registration based on population size at the local level and continually altered the various requirements, which unfortunately prevents consistent coding at the state level.⁴

It should also be expected that the desire for voting differed among groups within each state. As mentioned above, to test the two competing theories, it will be necessary to include information for income, race, and literacy. To complete the specification, we also note that a major hindrance to voting involves transportation costs. This burden is most likely to have fallen heaviest on rural and older citizens.

The number of illiterates of voting age is available in the *Statistical Abstracts*, which is then divided by the population size to determine the illiteracy rate. Other population data to be used, including urban, Black, and older than 65 years of age, are taken from *Historical Statistics*. For a few Western states, the recorded numbers for some variables are listed only as less than 500. These numbers have instead been

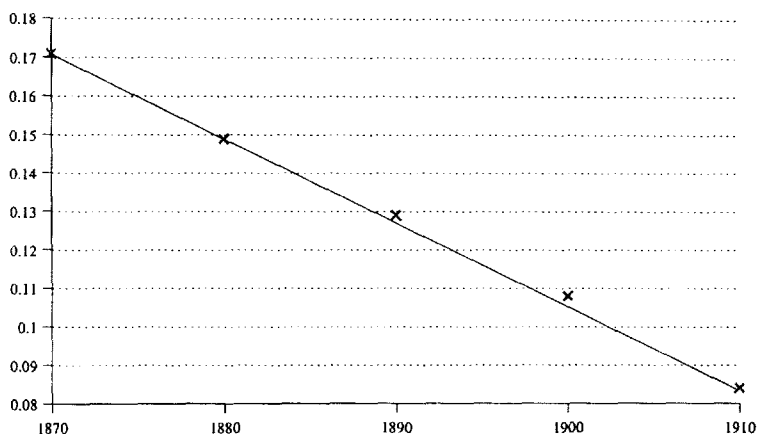


Figure 1: National Illiteracy Rates for Census Years 1870-1910

SOURCE: *Statistical Abstracts*.

derived by subtracting rural, White, and those aged 21 to 64, respectively, from the total population. For consistency, this computation was applied to all states.

The population data have been interpolated for the noncensus years based on an exponential growth rate. Illiteracy was interpolated linearly. Because this variable measures education as well as population change, there is no compelling a priori reason to prefer one interpolation method over the other (linear versus geometric). A simple plot in Figure 1, however, shows a demonstrable linear reduction in illiteracy among voting age citizens for the United States as a whole during the sample time period.⁵ Therefore, illiteracy rates for each state have been linearly interpolated for noncensus years.

Income is measured as the log of real wage per worker. Total wages and the number of workers in the state are available from the census, but an annual wage series for each state does not exist. Because income does not grow consistently, wages were indexed to gross national product (GNP) growth rather than interpolated, using annual GNP and price deflators estimated by Balke and Gordon (1989). The indexing procedure is detailed in a technical appendix. Summary statistics for each variable are listed in Table 1.

TABLE 1
Summary Statistics

<i>Variable</i>	<i>Mean</i>	<i>Standard Deviation</i>	<i>Maximum</i>	<i>Minimum</i>
Turnout	.591	.171	.963	.074
Logit (turnout)	.416	.818	3.27	-2.53
Secret ballot	.532	.499	1	0
Poll tax	.148	.356	1	0
Literacy test	.188	.391	1	0
Female suffrage	.053	.225	1	0
Presidential election	.435	.496	1	0
Log (wages/worker)	8.04	.293	8.87	7.10
Illiteracy rate	.113	.114	.492	.021
Black percentage	.104	.169	.779	.002
Urban percentage	.369	.252	.910	0
Older than 65 percentage	.040	.065	.088	0

FURTHER SPECIFICATION ISSUES

One potential shortcoming to the aggregated logit approach is the lack of a specific functional form for g in Equation 3, although Kelejian (1995) suggests it can be approximated by a general polynomial. The vector of coefficients for the nonlinear elements can then be used to construct a test for aggregation bias. Quadratic and cubic specifications, $F(1, 494) = .013$ and $F(2, 494) = 0.78$, respectively, failed to achieve (joint) significance for the nonlinear terms. The null of no aggregation bias cannot be rejected, which indicates it is reasonable to assume the estimated coefficients are consistent estimates of the desired individual-level parameters (Heckelman, 1997). Because the nonlinear terms fail to come close to statistical significance, for ease of presentation and estimation, the nonlinear function g in Equation 3 is dropped from subsequent analysis and ordinary least squares (OLS) can then be used to estimate the regressions.

Another potential problem in this framework is the assumption needed for OLS that the approximation errors in Equation 3 are independent across states and time. There are, however, several unobserved variables that may affect turnout in nonrandom ways. Ginsburg (1986) claims that data collection by the census has improved over time, making estimates of reported turnout more reliable in later years of the sample. If this is true, the errors will be correlated over time. It is

also well-known that Blacks were discriminated against at the polls even prior to any specific legislation aimed at them. In fact, Key (1949) concluded that obvious disfranchisement clauses, such as poll taxes and literacy tests, would have little additional impact due to the already rampant discrimination.⁶ The degree of discrimination, which varied across states, is not directly measurable, but the failure to include this factor results in a further misspecification bias.

It seems plausible therefore that the error term in Equation 3 can be decomposed into

$$e_{st} = v_s + u_t + w_{st} \quad (4)$$

where v_s is a state-specific error term, u_t is a time-specific error term, and w_{st} is a normally distributed random error component. Ignoring the nonrandom unobservable errors will result in biased estimates. To correct these problems, consider first a fixed effect model, which is based on the following specifications:

$$v_s = \sum_{j=1}^{S-1} \lambda_j n_{js}, \quad n_{js} = \begin{cases} 1, j = s, \\ 0, \text{else} \end{cases} \quad (5)$$

$$u_t = \sum_{j=1}^T \delta_j m_{jt}, \quad m_{jt} = \begin{cases} 1, j = t, \\ 0, \text{else} \end{cases}$$

The proper regression would include state and year dummy variables to capture the fixed state and time effects, which are not directly observable. In every regression that follows, F tests suggest the dummy variables are jointly significant in each dimension.

A second way to capture the degree of discrimination is to treat v_s as a random variable, where $v_s \sim (0, \sigma_v^2)$. The advantage is that a random effects model is more efficient due to the extra coefficients that must be estimated for the state dummies in the fixed effect model.⁷ If, however, the unobserved variables are correlated with the other explanatory variables, random effects yields biased and inconsistent estimates. One reason discrimination may have occurred more in, for example, Alabama than Vermont may be due to the lack of a large Black population in Vermont. There need not be discrimination against a group when their numbers are too small for them to have any substantial effect. Thus, discrimination, an unobserved variable, may be correlated with the percentage of Blacks in the state, which is a

directly observed explanatory variable included in the regressions. A Hausman test can be used to compare the fixed and random effects models. The null hypothesis of no correlation between the independent variables and the unobserved state errors, which supports a random effects model, can be rejected for each specification in the full sample but not for the subsample limited to Southern states.⁸ For comparison, both the fixed effects and random effects estimates are presented for each regression model. The central results remain robust to both specifications.

A final consideration is that presidential elections are often considered to be an important determinant of gubernatorial election turnout (Barzel & Silberberg, 1973; Heckelman, 1995). The fixed year effect prevents inclusion of a presidential election dummy due to perfect collinearity with the individual year dummies. The effect is still captured but not explicitly. The year dummies allow the presidential effect to vary in different years. An alternative specification (Model 2), which pools the time dummies for presidential years, allows direct estimation of a fixed presidential effect and can be interpreted as the mean value of those specific year dummies that are not included in the regression.

TESTING THE VOTE MARKET HYPOTHESIS

When party officials were purchasing votes directly, poor individuals were the voters accepting these bribes due to their lower reservation price. Due to diminishing marginal utility of income, the poor gain a higher utility payoff at the margin for a given cash bribe. They also faced higher political activation costs and therefore did not organize to purchase votes themselves (Anderson & Tollison, 1990). It seems likely then that if the vote market hypothesis is correct, bribes may have stimulated many poor people to vote and the elimination of bribes would have reduced their voting incentive.

An alternative explanation for a previously active vote market resulting in declining turnout under secret ballots is offered by Cox and Kousser (1981). They note that although the specific vote choice is now secret, the decision to vote is still public knowledge. Thus, specific voters might now be bribed not to vote if it was thought they were

likely to vote for the opposition. But if this is true, the opposition would be willing to pay that same person to ensure they do vote. As shown in Heckelman (1998), parties would be more selective in their bribery by targeting certain types of people and ignoring others. Bribery would be lessened but not eliminated. Secret ballots would still have a limited downward effect on turnout.

The implication in either case is that poor voters were directly affected by the alteration of the vote market. This notion can be tested by an interaction term between income and a secret ballot. Controlling for other personal characteristics, the interaction term captures the change in the effect of income on the likelihood of voting under a secret ballot, whereas the income term by itself measures the effect of income on the likelihood of voting without secret ballots. The vote market hypothesis predicts the interactive term coefficient to be positive, capturing the poor who voted only when bribed but otherwise abstained. Results from testing for the income effect from the vote market hypothesis implication are presented in Table 2.

In Model 1, the wages coefficient is not significantly different from 0. This suggests income did not factor into the voting decision when voting was open. Either income was not important or the poor were bribed often enough to yield a similar turnout rate compared to the rich. In Model 2 (using a presidential dummy variable), the wages coefficient is negative and significant, which implies the poor were more likely to vote an open ballot when the vote market was active, prior to enactment of a secret ballot law.

The coefficient for the interaction term between secret ballots and wages is significant in all regressions. Thus, the income effect differed under the presence of secret ballots. The positive sign of this coefficient would be consistent with two different possibilities. After secret ballots became law, either the rich entered the voting arena in larger numbers or the poor dropped out. Because the secret ballot has been shown elsewhere to have reduced state turnout overall (Heckelman, 1995; Kenny & Lott, 1997), only the latter conjecture is consistent with the empirical evidence.

As shown by the χ^2 statistic, the consistency of the random effects estimates is rejected in favor of the fixed state effect under both the full time dummy specification and the fixed presidential year effect. Regardless of the specification, the income effect found is consistent

TABLE 2
Secret Ballot Vote Market Hypothesis

Independent Variable	Model Version			
	Individual Time Dummies Only		With Presidential Dummy	
	Fixed State Effect	Random State Effect	Fixed State Effect	Random State Effect
Secret ballot	-5.814 ^a (1.608)	-6.319 ^a (1.525)	-3.124 ^a (1.438)	-3.546 ^a (1.412)
Poll tax	-.6484 ^a (.0723)	-.7121 ^a (.0680)	-.6401 ^a (.0722)	-.7150 ^a (.0683)
Literacy test	-.1217 (.0875)	-.2265 ^a (.0824)	-.1534 (.0882)	-.2822 ^a (.0826)
Female suffrage	.2023 (.1484)	.0419 (.1351)	.3209 (.1447)	.1612 (.1324)
Presidential election			.3741 ^a (.0952)	.4604 ^a (.0922)
Wages	.0285 (.2812)	-.0895 (.2281)	-.4686 ^a (.2232)	-.5240 ^a (.1980)
Urban	-1.152 ^a (.5663)	-.2427 (.3320)	-1.251 ^a (.4500)	-.5597 (.3008)
Black	1.240 (.7180)	-1.522 ^a (.5295)	1.627 ^a (.7207)	-2.211 ^a (.4938)
Illiterate	3.520 ^a (.7428)	2.110 ^a (.6644)	3.873 ^a (.6661)	2.660 ^a (.4211)
Older than 65	.1537 (.2785)	.1285 (.2765)	.1464 (.2825)	.1518 (.2806)
Secret × Wages	.7117 ^a (.1995)	.7783 ^a (.1891)	.3731 ^a (.1788)	.4176 ^a (.1755)
Adjusted R ²	.832	.800	.826	.788
Sum-squared residuals	47.58	56.67	50.22	61.31
Standard error	.335	.366	.341	.377
F (S, dof)	25.42 ^a (37,424)		24.75 ^a (37,432)	
χ ² (k)		72.09 ^a (44)		83.91 ^a (36)

NOTE: Dependent variable is the log of the odds of voting as estimated by the logit of state turnout rate. Each regression contains 506 observations. Coefficients for state (in Fixed State Effect columns) and year dummies not reported. Standard errors are in parentheses. *F* test is for null of no fixed effect. χ^2 test is for null of random effect versus alternative of fixed effect.

a. Significant at 5% error allowance.

with the central premise of the vote market hypothesis. The poor turned out to vote when they had the motivation of cash payments. They were more likely to vote when they were able to sell their votes. This effect diminished under the secret ballot.

TESTING FOR THE DISFRANCHISING EFFECT

Kousser's (1974) theory predicts Blacks and illiterates were less likely to vote under a secret ballot system than before. His theory does not incorporate a direct prediction on income. It could, of course, be

argued that Blacks and illiterates comprised the poor,⁹ but these factors were controlled for in the regressions by the race and literacy variables. The income effect was present independent of racial and literacy characteristics.

Just as the strict version of the vote market hypothesis is weakened by the incentive to continue bribing when voter preferences are predictable (Heckelman, 1998), so too may Kousser's (1974) expected illiteracy effect be overstated. In some of the Southern states, illiterates were already disfranchised by the presence of a literacy test requirement for voting.¹⁰ If potential voters could pass a literacy test, they should not have any trouble marking a ballot. There is little advantage gained from adopting restrictive legislation against illiterates once literacy tests are mandatory. For the remaining Southern states that did not yet have literacy tests, Kousser's argument might still hold. But why would some of these states later adopt literacy tests if the secret ballot was already successful in disfranchising illiterates?

Kousser's (1974) theory of a disfranchising effect can be tested in a multivariate framework by interaction terms between a secret ballot and race and a secret ballot and illiteracy, in the same manner income was analyzed in the previous regressions. The interaction terms capture a possible reduction in Black and illiterate voting after the secret ballot was adopted. Parameter estimates from the fixed and random effects regressions for the Southern states in the sample are listed in Table 3. The random effects specification is not rejected in either model 1 or 2.

None of the regressions directly support Kousser's (1974) notion that Blacks and illiterates were disfranchised when forced to vote an Australian ballot. The interaction term coefficients are not significant. The decision to vote made by Blacks and illiterates did not change when secret ballots were introduced. In the Southern sample at least, the disfranchisement effect is not supported. However, by restricting the sample only to Southern states, valuable information might be lost.

Although primarily concerned with the South in his book, Kousser (1974: 52-53) does make passing reference to a disfranchisement effect in Northern states as well. It would seem reasonable then that if Blacks and illiterates were supposedly unable to vote a secret ballot in the South, then neither could their counterparts in the rest of the

TABLE 3
Secret Ballot Disfranchisement Effect: South Only

Independent Variable	Model Version			
	Individual Time Dummies Only		With Presidential Dummy	
	Fixed State Effect	Random State Effect	Fixed State Effect	Random State Effect
Secret ballot	.7438 (.4257)	1.156 ^a (.3578)	.4688 (.4142)	.5806 (1.617)
Poll tax	-1.424 ^a (.2238)	-1.517 ^a (.1937)	-1.358 ^a (.2206)	-1.259 ^a (.2017)
Literacy test	.6382 ^a (.3239)	.3709 (.3058)	.2484 (.3196)	-.2707 (.2999)
Presidential election			-.6545 (.3608)	-.1251 (.3472)
Wages	.4141 (.6032)	.7679 (.5113)	-.2494 (.4290)	-.1371 (.4137)
Urban	-4.720 (4.143)	-3.643 (2.139)	-5.522 (4.183)	-4.127 (2.354)
Black	.8835 (.9257)	-.5333 (.8687)	1.516 (.9950)	-1.379 (.8022)
Illiterate	7.721 ^a (2.558)	.7607 (1.829)	5.708 ^a (1.988)	4.119 ^a (1.383)
Older than 65	22.32 (27.13)	32.49 (21.78)	49.85 ^a (23.20)	22.09 (19.35)
Secret × Black	1.468 (1.439)	-.3839 (1.346)	-.7717 (1.201)	-1.361 (1.175)
Secret × Illiterate	-5.518 (2.924)	-3.842 (2.792)	-1.044 (2.711)	-7.301 (2.509)
Adjusted R ²	.915	.871	.893	.838
Sum-squared residuals	6.92	10.50	9.819	14.93
Standard error	.340	.418	.380	.469
F(S, dof)	22.11 ^a (9,60)		24.91 ^a (9,68)	
χ ² (k)		31.04 (41)		35.42 (33)

NOTE: Dependent variable is the log of the odds of voting as estimated by the logit of state turnout rate. Each regression contains 128 observations. Coefficients for state (in Fixed State Effect columns) and year dummies not reported. Standard errors are in parentheses. *F* test is for null of no fixed effect. χ^2 test is for null of random effect versus alternative of fixed effect.

a. Significant at 5% error allowance.

nation. For whatever reasons Australian ballots were adopted outside the South,¹¹ the law should still disfranchise them due to their inability to properly mark the ballot. By expanding the scope back to the national sample, the number of observations is almost quadrupled and more variation is introduced into the sample. The disfranchising effect on Blacks and illiterates is tested in the full sample in Table 4.

Even in the larger sample, however, the interaction variables are not significant. Neither Blacks nor illiterates were significantly less prone to vote a closed ballot than an open ballot. The Hausman tests reject the random effects specification, although the conclusions drawn are not dependent on the modeling choice. In any of the four specifications, the empirical evidence does not support the notion that secret

TABLE 4
Secret Ballot Disfranchisement Effect: National Sample

Independent Variable	Model Version			
	Individual Time Dummies Only		With Presidential Dummy	
	Fixed State Effect	Random State Effect	Fixed State Effect	Random State Effect
Secret ballot	.0787 (.0957)	.1166 (.0935)	.0262 (.0872)	-.0350 (.0819)
Poll tax	.6445 ^a (.0784)	-.6553 ^a (.0748)	-.6256 ^a (.0772)	-.6287 ^a (.0747)
Literacy test	-.1312 (.0882)	-.2292 ^a (.0831)	-.1387 (.0885)	-.2427 ^a (.0831)
Female suffrage	.3741 ^a (.1392)	.2031 (.1278)	.3892 ^a (.1384)	.2221 (.1269)
Presidential election			.3265 ^a (.0920)	.3875 ^a (.0892)
Wages	.0111 (.2912)	-.1075 (.2309)	-.3391 (.2047)	-.4632 ^a (.1782)
Urban	-1.647 ^a (.5839)	-.5075 (.3367)	-1.522 ^a (.4615)	-.8230 ^a (.3066)
Black	1.055 (1.432)	-1.369 ^a (.5720)	1.396 (.7248)	-1.852 ^a (.5263)
Illiterate	3.947 ^a (.8878)	1.667 ^a (.7771)	3.740 ^a (.7779)	1.995 ^a (.7100)
Older than 65	.1260 (.4483)	.0815 (.2790)	.1335 (.2829)	.1082 (.2810)
Secret × Black	.4683 (.7510)	-.7135 (.7181)	.2153 (.7128)	-.7677 (.6918)
Secret × Illiterate	-2.059 (1.192)	-.8447 (1.153)	-1.639 (1.143)	-8.113 (1.125)
Adjusted R ²	.830	.799	.827	.792
Sum-squared residuals	48.12	56.87	49.93	59.97
Standard error	.337	.367	.340	.373
F (S, dof)	24.57 ^a (37,423)		24.54 ^a (37,431)	
χ ² (k)		68.47 ^a (45)		75.13 ^a (37)

NOTE: Dependent variable is the log of the odds of voting as estimated by the logit of state turnout rate. Each regression contains 506 observations. Coefficients for state (in Fixed State Effect columns) and year dummies not reported. Standard errors are in parentheses. F test is for null of no fixed effect. χ² test is for null of random effect versus alternative of fixed effect.
a. Significant at 5% error allowance.

ballots created an insurmountable barrier to voting for Blacks or illiterates.

As a final comparison, it is also possible to test the two proposed turnout effects from the legal-institutional theories against each other with a J test. The test statistic is calculated by including the predicted values from one specification as an additional explanatory variable in the other specification. Because the random effects is always rejected in the national sample, the test statistics will only be based on the fixed state effects models as

$$\ln\left(\frac{\hat{P}_{st}}{1-\hat{P}_{st}}\right) = (1 - \rho)\left(X_{st}\beta + Z_{st}\gamma + \sum_{j=1}^{S-1} \lambda_j n_{js} + \sum_{j=1}^T \delta_j m_{jt}\right) + \rho \hat{y}_{st} + w_{st}. \quad (6)$$

First, \hat{y}_{st} represents the predicted values from the income effect in Table 2 and the significance of ρ tests the null of the disfranchisement effect when \dot{Z}_{st} includes (in addition to the original population variables) the interaction variables for race and illiteracy. If the null is correct, there should be zero weight for \hat{y}_{st} in the regression and the full weight should be on the alternative specification. Thus, the t -statistic for $\rho = 0$ tests the null for accepting the disfranchisement effect. The roles are then reversed by using the predicted values from the disfranchisement effect in Table 4 for \hat{y}_{st} and the interaction variable for income in \dot{Z}_{st} . The significance of ρ would then be used to test the null of the income effect. Equation 6 can be estimated using a nonlinear least squares algorithm.

It is possible the J test will be unable to differentiate between the two effects. This would occur if ρ is found to be significant both times, thereby rejecting both nulls, or not significant either time, thereby accepting both nulls. Strong multicollinearity between the two hypotheses may hamper the J test, leading it to always accept both models. However, potential multicollinearity of variables within a given specification is not a problem because the J test does not attempt to sort out marginal effects from individual variables. As it turns out, the J test only yields a significant coefficient for the fixed time dummies (Model 1 in Tables 2 and 4) when the null is the disfranchisement effect. Thus, the J test rejects the disfranchisement effect on Blacks and illiterates in favor of the income effect. This serves as further reinforcement for the conclusions regarding rejection of the disfranchisement effect based on nonsignificance of the interaction terms in Table 4. For the fixed presidential effect specification¹² (Model 2), however, neither the income nor disfranchisement effect is rejected. In this case, it is not as clear which effect dominates.

CONCLUSIONS

The legal-institutional approach predicts secret ballots may affect voter turnout in two distinct ways. According to the vote market hypothesis, secret ballots remove (or weaken) the incentive to bribe voters, which in turn reduces their incentive to vote. The disfranchise-

ment effect suggests Blacks and illiterates, due to their inability to read and properly mark the new ballots, were prevented from voting.

Using aggregate data based on an individual-level voting model, this study has found direct evidence, based on coefficient significance, supporting an income effect consistent with the vote market hypothesis but not for a disfranchisement effect on Blacks and illiterates. Indirect evidence, through non-nested J tests, favors the income effect using a fixed time effect, but neither model can be rejected under the presidential election dummy specification.

The methodology of this study is not able to determine the legislative intent of secret ballot reform, but the empirical evidence suggests income, rather than race or literacy, was the crucial determinant for voting in secret ballot elections. The evidence is consistent with voluntary rather than forced abstention playing a significant role in reducing turnout in secret ballot elections. If an active vote market is considered detrimental to the democratic process, then part of the fall in turnout at the turn of the century should not be viewed as detrimental to social choice decisions.

APPENDIX

Deriving Wage per Worker in Noncensus Years

A wage index is defined as:

$$WI_s(c) = \frac{W_{sc}}{W_{s0}} \quad (A1)$$

where $WI_s(c)$ is the wage index for state s in period number c (noncensus year), W_{sc} is the average real wage per worker in period c , and W_{s0} is the same for the base (census) year in each state s . The counter $c = 1, \dots, 9$ represents the number of years since the last census. Because only census data for state income are available, the object is to find a suitable estimate for $WI_s(c)$. The ratio spanning 2 census years, $WI_s(10)$, is known, as is a similar index for real gross national product (GNP) per capita, $GNP(10)$. These indexes can be constructed as

$$WI_s(10) = \frac{W_{s10}}{W_{s0}} \quad (A2)$$

$$GNP(10) = \frac{GNP_{10}}{GNP_0}$$

Because annual GNP and price deflator estimates are available in Balke and Gordon (1989), $GNP(c)$ is also known for every year, where $GNP(c)$ is defined similar to Equations A1 and A2. Specifically,

$$GNP(c) = \frac{GNP_c}{GNP_0} \quad (A3)$$

It is assumed that wages grow similar to the rate of GNP growth, but each state is allowed its own index. First, the difference between the wage and GNP growth series between census years is calculated as

$$\frac{WI_s(10)}{GNP(10)} - 1 \quad (A4)$$

Equation A4 is the state adjustment term. The c^{th} percentage of the state adjustment is then added to the GNP index ($GNP(c)$) for that noncensus year. Equation A4 is normalized around 0 by subtracting 1 from the ratio. Because the ratio itself is always positive, normalizing the ratio ensures a positive value is added to $GNP(c)$ only when $WI_s(10)$ is larger than $GNP(10)$. In this way, when GNP grows faster than the state wages during the 10-year span so the ratio is less than 1, the yearly wage index is reduced by adding a negative amount, forcing it to grow slower than the yearly GNP index.

The wage index for each state is computed as a percentage adjustment of the GNP growth rate. Thus, Equation A1 is estimated as:

$$WI_s(c) = GNP(c) \left(1 + \frac{c}{10} \left[\frac{WI_s(10)}{GNP(10)} - 1 \right] \right) \quad (A5)$$

Because the base year, W_0 , is always known, W_{sc} can be easily found for each state by substituting Equations A1, A2, and A3 into Equation A5 and solving for W_{sc} as

$$W_{sc} = W_0 \frac{GNP_c}{GNP_0} \left(1 + \frac{c}{10} \left[\frac{\frac{W_{10}}{W_0}}{\frac{G_{10}}{G_0}} - 1 \right] \right) \quad (A6)$$

NOTES

1. These bribes may have included coercive threats. In these cases, voters save a negative amount by selling their votes. The analysis is the same.

2. King (1997) also offers an alternative to standard ecological regression, but his methodology is specific to linear models and requires truncation of the parameter distribution. A full

critical consideration of his procedure is beyond the scope of this article, but the methodology used here addresses most of the issues he raises.

3. The following states are not included in the sample because the secret ballot laws were not adopted uniformly throughout the state: Kentucky, Maryland, Minnesota, Tennessee, Texas, and Wisconsin. In addition, Oklahoma is dropped from the sample because it only contains one observation prior to 1910, and South Carolina is also dropped because the estimated turnout for the election of 1876 exceeds 100%, which prevents calculation of the logistic transformation of turnout. It should also be noted that not every state had adopted the Australian ballot by 1910, so these states serve as further controls.

4. Harris (1929) presents detailed case histories of registration for a few localities.

5. The census of 1900 enumerated illiterates older than the age of 20.

6. Rusk and Stucker (1978) debate this point.

7. The random effects estimator can be constructed from least squares analysis on the transformed equation $\hat{P}_{it} - \theta_s \hat{P}_{st} = (X_{it} - \theta_s X_{st})\beta + (\hat{Z}_{it} - \theta_s \hat{Z}_{st})\gamma + w_{it}$, where $\theta_s = 1 - \sqrt{\sigma_w^2 / (\sigma_w^2 + \tau_s \sigma_\epsilon^2)}$.

Because the variances are unknown, the feasible generalized least squares (FGLS) procedure outlined in Greene (1993) can be used to generate unbiased estimates. One potential problem with the FGLS estimator is that it may produce negative estimates for the denominator of θ in finite samples. Fortunately, the random effects specifications presented below do not suffer from this problem. (Large sample formulas are asymptotically correct only if T is large relative to S , so they are not appropriate for the current data set.)

8. The Hausman test statistic is distributed as a chi-square with k degrees of freedom, where k is the number of explanatory variables. The computed value is included in the regression tables along with the degrees of freedom in parentheses.

9. The correlation between log wages and Black is $-.45$ and $-.56$ between log wages and illiteracy.

10. North Carolina, South Carolina, and Georgia adopted literacy test requirements prior to secret ballots. Mississippi adopted both in the same year.

11. Kousser (1974) notes the "existing evidence of the intent of the Northern ballot reformers is only circumstantial and suggestive" (p. 53). For various theories concerning legislator motivation in the North not related to the vote market hypothesis, see Argersinger (1980), Burnham (1970), and the references in Kousser (1974).

12. The representation for Model 2 would be slightly different than Equation 6 because a presidential year dummy replaces some of the individual time dummies. The methodology remains the same.

REFERENCES

- Abramson, P. R., & Aldrich, J. H. (1982). The decline of electoral participation in America. *American Political Science Review*, 76, 502-521.
- Anderson, G., & Tollison, R. (1990). Democracy in the marketplace. In W. M. Crain & R. Tollison (Eds.), *Predicting politics: Essays in empirical public choice* (pp. 285-303). Ann Arbor: University of Michigan Press.
- Argersinger, P. (1980). "A place on the ballot": Fusion politics and antifusion laws. *The American Historical Review*, 85, 287-306.

- Balke, N., & Gordon, R. (1989). The estimation of prewar gross national product: Methodology and new evidence. *Journal of Political Economy*, 97, 38-92.
- Barzel, Y., & Silberberg, E. (1973). Is the act of voting rational? *Public Choice*, 16, 51-58.
- Burnham, W. (1965). The changing shape of the American political universe. *American Political Science Review*, 59, 7-28.
- Burnham, W. (1970). *Critical elections and the mainsprings of American politics*. New York: Norton.
- Burnham, W. (1974). Theory and voting research: Some reflections on Converse's "Change in the American electorate." *American Political Science Review*, 68, 1002-1023.
- Burnham, W., Clubb, J., & Flanigan, W. (1971). *State-level congressional, gubernatorial and senatorial election data for the United States, 1824-1972* (1991 ed.). Ann Arbor, MI: Inter-university Consortium for Political and Social Research.
- Converse, P. (1972). Change in the American electorate. In A. Campbell & P. Converse (Eds.), *The human meaning of social change* (pp. 263-337). New York: Russell Sage.
- Cox, G. W., & Kousser, J. M. (1981). Turnout and rural corruption: New York as a test case. *American Journal of Political Science*, 25, 646-663.
- Evans, E. (1917). *A history of the Australian ballot system in the United States*. Chicago: University of Chicago Press.
- Ginsburg, G. (1986). Computing antebellum turnout: Methods and models. *Journal of Interdisciplinary History*, 16, 579-611.
- Greene, W. H. (1993). *Econometric analysis*. Englewood Cliffs, NJ: Prentice Hall.
- Harris, J. (1929). *Registration of voters in the United States*. Washington, DC: Brookings Institution.
- Harris, J. (1934). *Election administration in the United States*. Washington, DC: Brookings Institution.
- Heckelman, J. C. (1995). The effect of the secret ballot on voter turnout rates. *Public Choice*, 82, 107-124.
- Heckelman, J. C. (1997). Determining who voted in historical elections: An aggregated logit approach. *Social Science Research*, 26, 121-134.
- Heckelman, J. C. (1998). Bribing voters without verification. *The Social Science Journal*, 35, 435-443.
- Historical statistics of the United States, colonial times to 1970*. (1975). Washington, DC: U.S. Department of Commerce, Bureau of Statistics.
- Kelejian, H. (1995). Aggregated heterogeneous dependent data and the logit model: A suggested approach. *Economics Letters*, 47, 243-248.
- Kenny, L. W., & Lott, J. R. Jr. (1997). *Long delays in utilization of new voting rights*. Paper presented at the Public Choice Society meetings, Houston, Texas.
- Key, V. O. (1949) *Southern politics in state and nation*. New York: Vintage.
- King, G. (1997). *A solution to the ecological inference problem*. Princeton, NJ: Princeton University Press.
- Kleppner, P. (1982). Were women to blame? Female suffrage and voter turnout. *Journal of Interdisciplinary History*, 12, 621-643.
- Kleppner, P., & Baker, S. C. (1980). The impact of voter registration requirements on electoral turnout, 1900-1916. *Journal of Political and Military Sociology*, 8, 205-226.
- Kousser, J. M. (1974). *The shaping of Southern politics: Suffrage restrictions and the establishment of the one-party South, 1880-1910*. New Haven, CT: Yale University Press.
- Mill, J. S. (1962). *Considerations on representative government*. Chicago: Henry Regnery. Originally published in 1861.
- Piven, F. F., & Cloward, R. A. (1989) *Why Americans don't vote*. New York: Pantheon.

- Rusk, J. (1974). Comment: The American electoral universe: Speculation and evidence. *American Political Science Review*, 68, 1028-1049.
- Rusk, J., & Stucker, J. (1978). The effect of the southern system of election laws on voting participation: A reply to V. O. Key, Jr. In J. Silbey, A. Bogue, & W. Flanigan (Eds.), *The history of American electoral behavior* (pp. 198-250). Princeton: Princeton University Press.
- Statistical abstracts of the United States*. (various years). Washington, DC: Bureau of Statistics.

Jack C. Heckelman is an assistant professor of economics at Wake Forest University. His main research interests deal with secret ballot elections and political business cycles. He has published articles in Economics and Politics, Public Choice, The Social Science Journal, and Social Science Research among other journals. He is coeditor of Public Choice Interpretations of American Economic History (Kluwer Academic Press, 1999).