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THE RATIONALITY UNINFORMED ELECTORATE:  
SOME EXPERIMENTAL EVIDENCE

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### Abstract

This essay reports on a series of twenty four election experiments in which voters are allowed to decide between voting retrospectively and purchasing contemporaneous information about the candidate challenging the incumbent. Each experiment consists of a series of election periods in which dummy candidates choose spatial positions which represent either their policy while in office or a promise about policy if elected. Subjects (voters) are told the value to them of the incumbent's policy, but they must decide, prior to voting, whether or not to purchase information about the value of the challenger's promise. In general, our data conform to reasonable expectations: voters purchase less information and rely more on retrospective knowledge when the candidates' strategies are stable, and their likelihood of purchasing information during periods of instability is tempered by the likelihood that their votes matter, by the reliability of the information available for purchase, and by the degree of instability as measured by changes in each voter's welfare.

## The Rationally Uninformed Electorate: Some Experimental Evidence

A considerable empirical literature documents the low levels of information which characterize voters' knowledge of election issues and the candidates' positions on issues. Although such deficiencies are at odds with the assumptions of many of the formal theories of elections developed since Downs's [1957] seminal volume, an imperfectly informed electorate was not only anticipated by the Framers of the U.S. Constitution, but, in retrospect, seems a reasonable response to the costs of information and to the improbability in most elections that one's vote is decisive. That is, the expected utility loss of casting a ballot based on erroneous beliefs is insignificant, especially when it is compared to the net cost of aspiring to the ideal of the fully informed citizen. Correspondingly, voters reduce information costs by basing their decisions on retrospective evaluations, coupled with a variety of contemporaneous cues such as the candidates' party labels, interest group endorsements, and mass media reports on the candidates' personalities and lifestyles. As Popkin as his co-researchers (1976) summarize the matter (p. 787): "Most voters will only learn enough to form a very generalized notion of the position of a particular candidate or party on some issues ... [and] the investor-voter will use partisan and ideological labels as practical solutions to the problems of costly information."

Although retrospective evaluations and cues probably admit significant opportunities for erroneous judgments, they achieve a closer match between the costs of error and of information. Moreover, we can interpret the electorate's reliance on indirect sources of information as "systemically rational" in the sense that the long run equilibrium strategies for candidates in such electorates correspond, under various general conditions, to the median voter's preference if such a preference exists (McKelvey and Ordeshook, 1986, Collier et al, 1987). Since the median prevails theoretically with a perfectly informed electorate, there is no social cost associated with the use of cues, and

evaluations based on retrospective judgments of the candidates and their parties: hence the notion of "systemic rationality."

We should not interpret arguments about the systemic rationality of democratic processes to mean, however, that patterns of information are irrelevant. Circumstances exist under which the electorate's incomplete information yield different outcomes than what prevails under complete information, and the processes whereby candidates and voters adjust to each other may be sufficiently slowed by incomplete information which no equilibrium is ever achieved (McKelvey and Ordeshook, 1985). Thus, it is important that we understand the relationship between a citizen's propensity to gather information and the parameters which characterize elections. To this end, we report here on a series of twenty four experiments in which voters (subjects) must decide whether or not to buy information about the challenger to an incumbent and thus they must choose between becoming informed -- between learning the promises of challenging candidates -- and voting retrospectively -- voting on the basis of the incumbent's performance.

Although we do not offer a general calculus for a voter's decisions, our general conclusions match reasonable hypotheses: the likelihood of buying information increases if such information is a reliable indicator of what challengers do once elected, if one's vote is likely to matter, if the voter experiences a sudden change in his or her welfare, and if it is "reasonable" for a voter to contemplate switching loyalties to another candidate. And, in the aggregate, our data exhibit several suggestive and interesting patterns: Increased loyalty to one candidate or the other is accompanied by a decreased propensity to purchase information; instability in candidate strategies increases this propensity; and the proportion of voters purchasing information is negatively correlated with the proportion voting "incorrectly." To the extent, then, that our experiments offer subjects a decision problem similar to the one voters confront in actual elections, our results suggest that we should not be surprised to observe electorates which invest few

resources in political information, especially if no great issues arise to upset the kind of equilibrium which is likely to prevail in stable democracies in which the imperatives of Median Voter Theorem apply. On the other hand, there is much idiosyncratic behavior -- some identically situated voters buy information in almost every election, some never become informed, and some buy information on a seemingly random basis.

Section 1 of this essay describes our experimental procedures and offers more detail about the voters' decision environment. Section 2 discusses some general patterns in the data, whereas Section 3 provides a more thorough analysis in which we try to explain decisions about information on the basis of experimental parameters.

### 1. Experimental Design

Each experiment consists of a series of election periods (34 plus an initial period).<sup>1</sup> In each period the incumbent chooses an issue position -- a number between 0 and 999 -- which is transformed, via a single peaked utility function, into a payoff for each of five voters. In all experiments, the voter's ideals are at 400, 250, 775, 400, and 830. At the maximum, each voter earns 16 "Franks," which is translated into actual payoffs at the rate of two cents per Frank (all subjects learn the exchange rate at the beginning of the experiment, but they are not informed about their ideal points, or even that the candidates' strategies consist of positions on some line), and payoffs decline to zero at the rate of .02 Franks/unit as a policy moves in either direction away from the voter's ideal. Each voter learns his or her payoff from the incumbent's policy, but not the policy. The challenging candidate, on the other hand, selects a position which ostensibly represents the policy he or she would have adopted if in office, but the value of this

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<sup>1</sup> Excepting those instances, however, in which time expired or the network over which the experiments were conducted crashed. The number of election periods per session are as follows: For promises not kept, ex. 1: 34,34,29,34,34,27; for ex. 2: 34,34,34,34,32,34. For promises kept, ex. 1: 34,34,31,34,34,34; for ex. 2: 29,34,34,32,34,34.

position is not reported to the voters. Instead, after learning the value of the incumbent's policy, each voter must decide whether or not to purchase information about the value of the challenger's promise. This cost, equal in all experiments to 1 Frank is deducted from the subject's payoff, and all voters must then choose between retaining the incumbent or electing the challenger. The vote totals are reported to all voters, but no voter learns who voted for whom, how many voters purchased information, or the payoffs to other voters. The election winner becomes the new incumbent, and the process is repeated. (The experiment is initialized in period 0 by choosing the incumbent randomly. After the incumbent selects a position and its payoff value is reported to the voters, the voters decide whether to retain this person as the incumbent for period 1. The challenger's makes no promise in this initial period.)

The form of these experiments is similar, then, to the ones which Collier et al (1987) review in their study of retrospective voting. Owing to our research interests, however, we introduce two important differences in experimental design. First, by not allowing voters to purchase information about challengers, Collier et al force voters to choose retrospectively. Such a procedure is warranted if we want to learn whether retrospective voting induces the candidates to choose median policy positions, but presently we are interested in learning whether or under what conditions voters will actually choose to act retrospectively -- the circumstances under which they will forgo the opportunity to be informed about contemporaneous data and will choose instead to act on the basis of the candidates' past performances. Second, given our focus on voter as against candidate decisions, our experiments use dummy candidates. Although voters are led to believe that the candidates are real subjects, the actual policy positions of the candidates are predetermined at the beginning of the experiment in order to facilitate testing certain hypotheses about voting.

Subjects were mostly undergraduates untrained in spatial models of elections enrolled in Freshman and Sophomore introductory courses. At the beginning of each experiment, the five "real" subjects were designated as voters and two "shills" (usually graduate students from the department) were assigned the role of candidates. Subjects were informed that the experiment would be conducted entirely over a network linking computer terminals. Each voter and each candidate sat at a separate terminal (subjects were told that the candidates' terminals were in adjoining rooms) and all communication occurred over the network. No other communication was permitted. After reading the instructions (see Appendix), the two candidates were asked to leave the room containing the voters' terminals. A 5-period trial session familiarized subjects with their task. At the termination of the first experiment, a second was run using a different sequence of candidate positions.

We report on a total of twelve experimental sessions, or twenty four experiments. In six sessions, unless the candidates are in equilibrium or "near"-equilibrium with respect to the electorate's median preference, a promise bears no necessary relationship to the policy the challenger chooses if elected. In the other six sessions, challengers always keep their promises (no subject was allowed to participate in both types of experiments, and with but one exception, subjects were all "inexperienced"). Thus, information is considerably less valuable in the first six sessions, so if subjects respond rationally to the value of information, we anticipate that more information will be bought in the second series.<sup>2</sup> The first experiment within a session has the two candidates slowly converging to positions near the electorate's median preference (one candidate converges to the median, 400, and the other to the point 390), remaining at those positions until

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<sup>2</sup> We emphasize that information is not entirely worthless in the first six session since promises correspond to selected position when the candidates positions are temporally stable. We can hypothesize, then, that the difference we report here between the two series of experiments would be magnified considerably if promises were entirely unrelated to positions.

the 29th period, and fluctuating wildly once again until the experiment's termination. Figure 1 shows the sequence of candidate positions, which also serve as promises for the challenger. Hence, in election period 4, for example, suppose the incumbent is candidate 1 and the challenger is 2. The incumbent, 1, chooses 400, and 2 promises 600. If 2 is elected, 2 then chooses 900 and 1 promises 750. Notice, then, that 2's promise is unreliable except in those periods when the candidates' positions are stable (periods 17 - 29). In the second experiment of each series (see Figure 2), the candidates converge more rapidly towards the median until the 20th period, at which time they fluctuate for a few periods, and converge identically to the new point 690. This experiment also ends with a few periods of instability. Figures 3 and 4 graph the candidates' promises for the second series of six experimental sessions -- promises which candidates must keep if elected. Hence, referring to Figure 3, if in period 4, for example, candidate 1 is the incumbent at 250 (his promise in period 3) and 2 is the challenger promising 600, then if 2 is elected, 2 must choose 600 and 1 promises 750; but if 1 is reelected, 1 chooses 400 and 2 promises 900 (to disequilibrate strategies and accomplish the "strategic shift" more rapidly in experiment 2 of this series, the incumbent chooses 900 in period 21 if he is candidate 1 and 100 if he is candidate 2).<sup>3</sup>

## 2. Basic Hypotheses

It seems self-evident that voters will buy information only if they believe that its value exceeds its cost. Transforming this supposition into an formalized and testable decision rule, however, is difficult since it may imply different things for different people. Risk acceptant voters, for example, might purchase information in environments in which candidates rarely change positions to protect against the possibility that the

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<sup>3</sup> If candidate 1 (\*) is the initial incumbent and thereby makes no promise, then that candidate chooses 550 in period 1; if candidate 2 (o) is the initial incumbent, it chooses 800.

challenger has found a better policy; risk averse voters might never purchase information; and voters which form high subjective probability estimates of their decisiveness should be more willing to purchase information than voters with low subjective estimates. Nevertheless, despite the possible variation in the decision calculus which might characterize voters, several hypotheses are consistent with our basic supposition and can be tested with our experimental data.

Our first hypothesis is based on the simple premise that if voters believe that incumbents will act as they have in the past because the strategies of both candidates are temporally stable, then there is little reason to purchase information.

**H1:** voters should be less likely to purchase information if the candidates' strategies are stable. Conversely, voters should be more likely to purchase information if the candidate's positions are unstable and likely to change from period to period.

This hypothesis is important, because it suggests that a poorly informed electorate -- an electorate which pays little heed to the contemporaneous utterances of candidates -- may not indicate any basic defect in democratic institutions: indeed, it suggests that such low levels of information may merely signal a stable democratic system in which voters do not anticipate that either party will nominate candidates which depart greatly from long-term equilibrium strategies.

Our second hypothesis summarizes the idea that voters should purchase information only if they believe that their votes are likely to be relevant to the determination of the election winner. Otherwise, becoming informed about politics is akin to becoming informed about baseball batting averages -- a widespread hobby certainly, but not one which is undertaken to influence events. Political information, in this instance, is the

consumable commodity, rather than the candidates' policies. Presumably, however, the hobbyists are a constant in society (or at least in an given experiment), so

**H2:** voters should be less likely to purchase information if their vote is unlikely to be decisive.

Finally, if voters learn that information provides little basis for predicting the performance of candidates, then presumably any purchase is unwise. Hence,

**H3:** voters should be more likely to purchase information the greater the reliability of that information -- in this instance, for those experiments in which challengers necessarily keep their promises.

Indeed, we can add as a corollary to this hypothesis the proposition that whenever information is worthless, hypotheses H1 and H2 are irrelevant, because they implicitly suppose that information conveys something useful about alternatives and the future. Thus, variables such as the plurality of victorious candidates and the variability of candidate positions should count for less in our first set of experiments.

We prefer, of course, to give these hypotheses a firmer theoretical foundation by deducing them from some explicitly stated model of a voter's decision calculus. But even a simplified model reveals the considerable latitude in alternative formulations and, as our data confirm, the opportunities for idiosyncratic behavior. To see this, let  $I$  denote the payoff from the incumbent, and let voters regard this number as an indicator of what they are likely to get if the incumbent is reelected. Suppose the payoff from the challenger is either  $C$  or  $C'$ , where  $C < I < C'$ , and let the voter believe that  $C$  occurs with probability  $p$  and  $C'$  with probability  $1-p$ . More generally, we should regard  $C$  and  $C'$  as conditional expectations:  $C$ , for instance, is the expected value of the challenger's promise, given that the promise's value is less than  $I$ , which occurs with probability  $p$ .

Finally, let  $C_0$  be the cost of information, and, to simplify matters, let the voter condition his actions on being decisive -- let the victorious candidate be determined solely by this voter's vote. The voter, now confronts a two-stage decision: whether to purchase information, and for whom to vote. If the information is bought, the voter receives  $I - C_0$  if  $C$  is revealed as the challenger's payoff, or  $C' - C_0$  if  $C'$  is revealed, so the expected value of buying is  $pI + (1-p)C' - C_0$ . Without information the voter chooses the incumbent if  $I > pC + (1-p)C'$ , and he chooses the challenger if this inequality is reversed. Suppose, first, that the voter, if he does not purchase information, intends to vote for the incumbent. Believing that  $I > pC + (1-p)C'$ , the voter secures information if and only if  $I < pI + (1-p)C' - C_0$ . This second inequality requires that  $C' - I > C_0/(1-p)$ , whereas the first inequality requires that  $C' - I < p(C' - C)$ , so  $p(C' - C) > C_0/(1-p)$ , or simply

$$C' - C > C_0/[p(1-p)] \quad (1)$$

Alternatively, if the voter intends to vote for the challenger if no information is purchased -- if  $I < pC + (1-p)C'$  -- the purchase is made if and only if  $pC + (1-p)C' < pI + (1-p)C' - C_0$ , which requires that  $I - C > C_0/p$ , and which, when combined with the first inequality, yields expression (1) again. This expression clearly implies that the likelihood of purchasing information declines as  $C_0$  increases, but this consequence is trivial. Confounding the identification of additional consequences, however, is the fact that, because  $C$  and  $C'$  are conditional expectations,  $C' - C$  and  $p(1-p)$  are linked by the structural form of the voter's uncertainty. For example, if the voter believes that the payoff from the challenger is given by a stationary uniform density centered at  $I$ , and if the voter confronts a change in  $I$  to some extreme value, say  $I'$ , then  $p(1-p)$  decreases whereas  $C' - C$  remains constant, so the likelihood of purchasing information decreases as well. Conversely, if the extreme value  $I'$  is the initial payoff, which changes suddenly to  $I$ ,  $C' - C$  again remains constant,  $p(1-p)$  increases, and the purchase of information

becomes more likely. Thus, payoff changes from less to more extreme values decrease the likelihood of purchasing information, and changes in the other direction -- from extreme to more moderate values -- reverse this likelihood's direction of change.

One inference to be drawn from these facts is that if a voter does not initially anticipate changing his or her vote because the incumbent's performance is so poor or so good relative to expectations about the challenger, but if a sudden change in incumbent's performance is observed, then the likelihood of purchasing of information should increase. Stated as an empirical hypothesis,

**H4:** A decline in loyalty, as occasioned by a change in payoff from the incumbent, will be accompanied by an increased likelihood of purchasing information about the challenger.

This hypothesis should be treated carefully, since its analytic justification makes strong use of the uniform density's properties. Indeed, not only might other densities yield a different relationship between  $C' - C$  and  $p(1-p)$ , but even if we use the uniform density for analytic tractability, its mean and variance remain subjective parameters which can occasion idiosyncratic behavior. Voters who believe that the challenger is most likely to yield payoffs which are clearly superior or inferior to the incumbent will react differently to a change in the incumbent's performance than voters who regard the two candidates as equivalent on average. Also, our model is too simple to represent the complex decisions which actually confront subjects. It treats one candidate, the incumbent, as a certain alternative and, thus, it does not make any accommodation for the changes in priors which might occur whenever the payoff from an incumbent suddenly changes; moreover it does not consider the possibility that the uncertainty about the challenger changes over time in a coherent way based on the information which the voter gathers. The empirical analysis which follows, then, should be

interpreted less as the testing of a specific model of voter decision making and more as an exploratory mission into the kinds of correlations in information gathering and candidate choice which we are likely to find in actual elections.

## 2. Summary Statistics and General Observations

Beginning with some general summary statistics and patterns in the data, the most general fact concerns the number of voters who purchase information. Overall across all experiments, the average number of purchases per election period is 1.92 with a standard deviation of 1.23. Table 1 breaks this average down by experimental type, and also reports the standard deviation of this average, the average payoff difference between the incumbent's policy and the challenger's promise, and the aggregate number of election periods across all experiments of each type.

Table 1: Aggregate Summary Data

	promises not kept		promises kept	
	Exp. #1	Exp. #2	Exp. #1	Exp. #2
Avg. # buying	1.77	1.44	2.38	2.12
std. dev.	1.22	1.06	1.22	1.17
Avg. payoff diff.	1.98	2.55	2.21	1.96
# of periods	192	202	201	197

Several tentative conclusions can be drawn from these numbers, but the most striking fact is that for no type of experiment does the average number buying exceed one-half of the electorate, which is interesting given that the cost of information (1 Frank) is significantly less than the average payoff to voters in each period (approximately 11 Franks). Indeed, this cost equals only one half the cost of the average difference in payoffs associated with the incumbent's policy and the challenger's promise. Thus, voters

generally appear to decide retrospectively even though the cost of information is less than the average potential cost of electing the wrong candidate (but not necessarily less than the cost of an incorrect ballot since ballot may not be decisive). Comparing the averages by experiment type, moreover, offers the first bit of evidence in support of hypothesis H3: voters purchase more information on average when challengers necessarily keep their promises than when they do so only in periods of stable strategies.

No mean in Table 1, though, is significantly different from any other, which stems from the fact that although the means differ in an anticipated way, they disguise much variation within and across experiments. As a partial view into this variation, Figures 5a - 5d graph the proportion of voters purchasing information by period. These figures show some of the variation which yields the large standard deviations for the overall averages which Table 1 reports. A closer inspection of these figures suggests, nevertheless, that our data, at least for this aggregate level of analysis, are consistent with the first hypothesis, H1. Notice that in Figures 5a and 5c, the trend in average purchases is negative up to periods 29 and 30 respectively -- up to the period in which, after nearly converging, the candidates once again fluctuate wildly, at which point the average number of purchases increases sharply. Similarly, this trend is negative in Figures 5b and 5d up to periods 19, after which the candidates again fluctuate wildly and average purchases increase sharply. Moreover, to see from these data that subjects are responding to the value of information, Figures 6a and 6b graph the trend in proportion buying through the initial periods of unstable candidate positions (through period 15 in the first experiment in a session, and through period 8 in the second experiment). Notice that if promises are kept, the decline in proportion over time is slight, whereas this decline is accentuated if promises are not kept. Indeed, consistent with the supposition that "experienced" subjects will more quickly learn the value of information, this decline is greatest in the second experiment if promises are not kept.

Disaggregating the data by election period continues nevertheless to obscure the considerable variation across experiments and subjects. The average voter, for example, purchases information 39 percent of the time, but the standard deviation of this average is 26. Three of 120 persons never make a purchase, and three make purchases on every occasion. To see this differently, Table 2 reports the correlations in these decisions among voters, as well as their ideal points. Notice, in particular, that voters 1 and 4 share the same ideal, and thus they confront an identical decision problem (except to the extent that that problem is altered by different patterns of earlier information-purchasing decisions). Nevertheless and despite the fact that the correlations in their decisions in the highest among any pair of voter types, this correlation is only .192. Similarly, although voters 3 and 5 share nearly identical ideal points, so that they also share a decision problem in those instances in which both candidates choose policies to the left of 775 (all but one instance in the first experiment of a session, and all but a maximum of three instances in the second experiment), their decisions correlate only .043.

Table 2: Correlations of buying decisions among voters

	v1 at 400	v2 at 250	v3 at 775	v4 at 400	v5 at 830
v1	-				
v2	-.003	-			
v3	-.048	.002	-		
v4	.192	.128	.104	-	
v5	.000	.156	.043	.098	-

Not only do the decisions of similar voter types fail to correlate, but there is no significant relationship between a voter's ideal and his or her propensity to purchase information. Voters with ideal points at 400 purchase information 43 percent of the time; those at 250 purchase information 37 percent of the time; at 775, 37%; and at 830,

42%. If we look at only the first experiments in a session -- those in which the candidates vary their positions about or converge to the median, these percentages become 44%; 44%; 40%; and 46%, respectively. This pattern appears to contradict Palfrey and Pool's [1986] conclusion that "Informed voters tend to be located at the extreme of the political spectrum. Uninformed voters tend to be in the center (p. 12)." And even if we keep in mind that these percentages measure only the decision to purchase information and not the accuracy of beliefs about candidates, another aggregate measure suggests that accuracy of beliefs does not correlate either with the location of a voter's ideal. Overall, voters with ideal points at 400 make "correct" decisions -- decisions in which voters choose the incumbent if the incumbent's policy is worth more than the value of the challenger's promise (which may or may not be known to the voter, depending on whether information is purchased) -- 59 percent of the time; at 250, 49%; at 775, 58% and at 830, 54%. For those experiments in which challengers keep their promises, these averages are 58%, 53%, 65%, and 63%, respectively. (These percentages are not significantly different from the results of random decisions, but we see later that these errors are not distributed randomly.)

General statistical summaries, however, do not allow us to answer several important questions. Does the sharp rise in purchases which occur when candidates depart from stable positions support hypothesis H1 or is it merely a statistical artifact? Are the negative trends which Figures 5a - 5d portray and which we suggest are occasioned by candidate convergence, merely indicative of a general tendency for subjects to purchase less information as each experiment proceeds? If voters react at all to payoff changes, do they react more to negative as against positive changes? Are voters more sensitive to parameters when challenger's keep their promises than when they do not do so? Our analysis in the next section seeks answers to such questions.

Figure 1: Candidate Positions  
Experiment 1, promises not kept

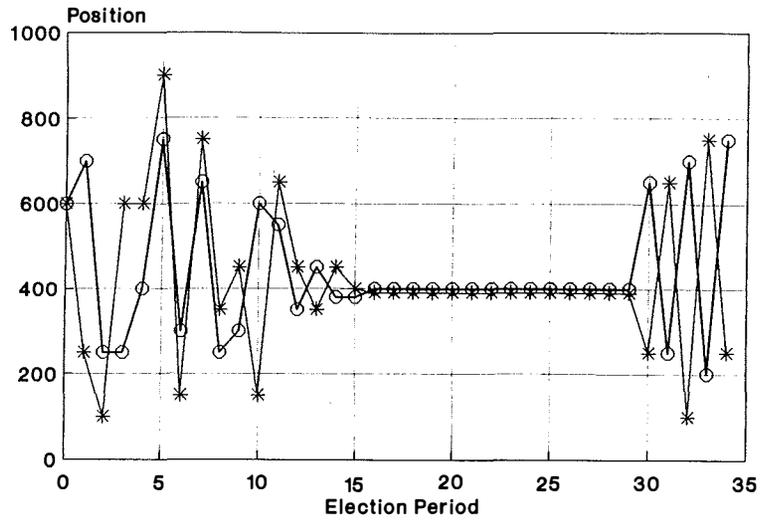


Figure 2: Candidate Positions  
Experiment 2, promises not kept

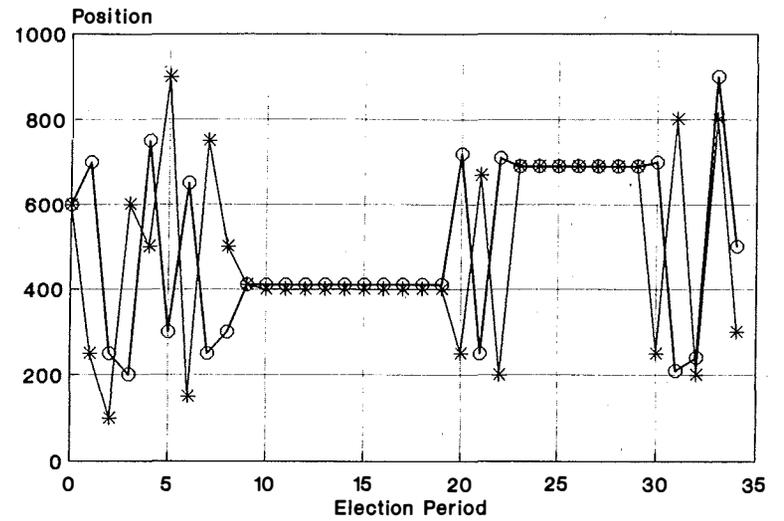


Figure 3: Candidate Promises  
Experiment 1, promises kept

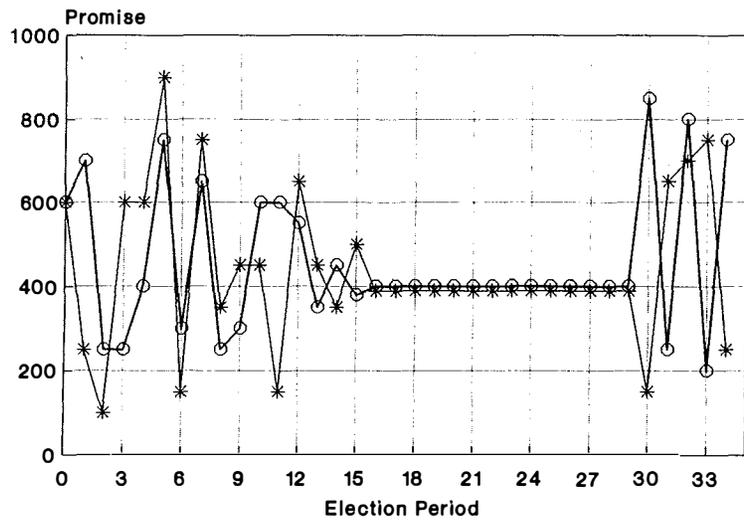


Figure 4: Candidate Promises  
Experiment 2, promises kept

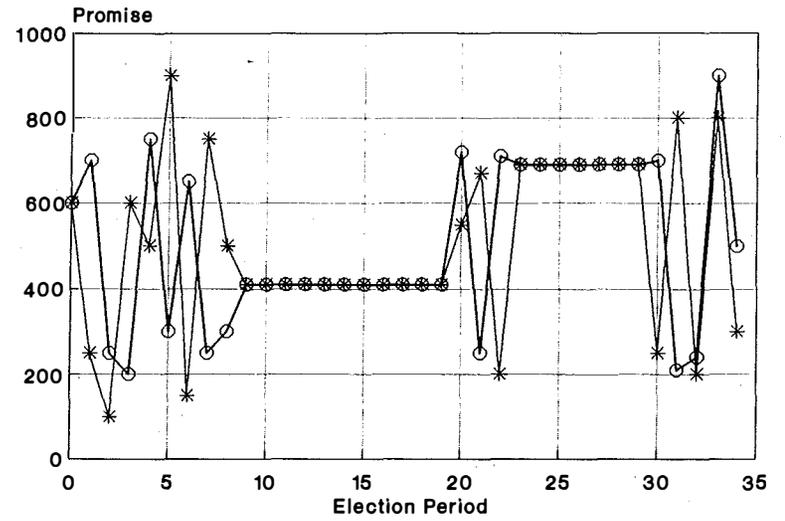


Figure 5a: Proportion Buying  
Experiment 1, promises not kept

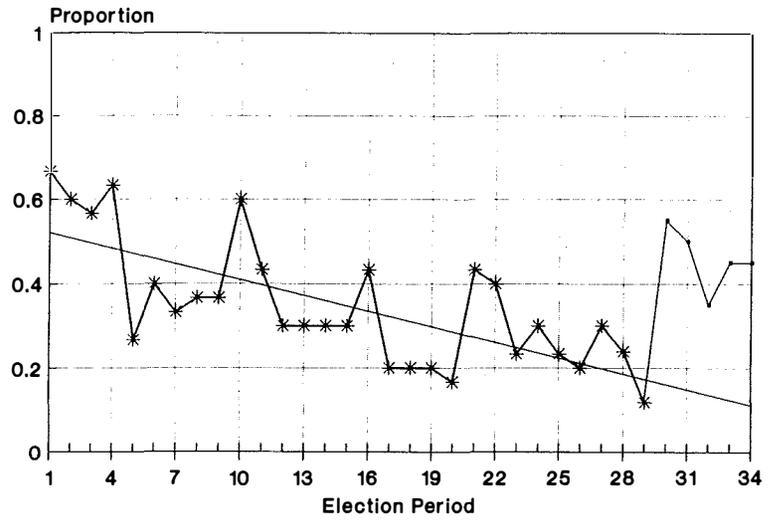


Figure 5b: Proportion Buying  
Experiment 2, promises not kept

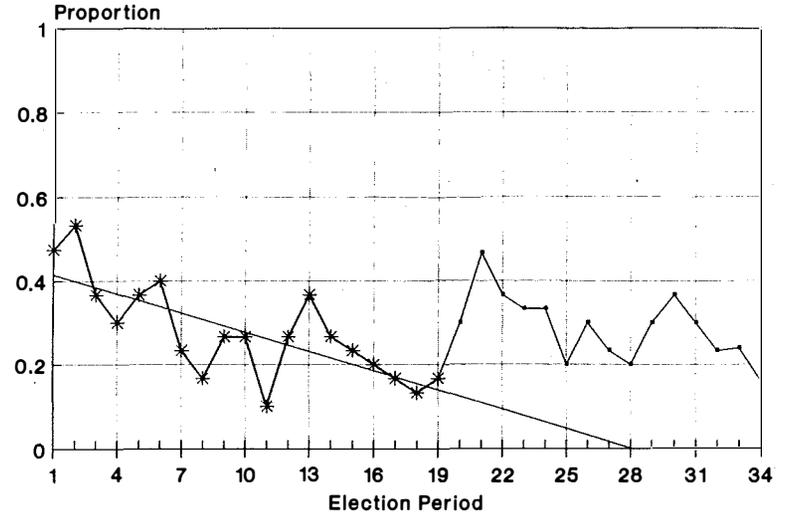


Figure 5c: Proportion Buying  
Experiment 1, promises kept

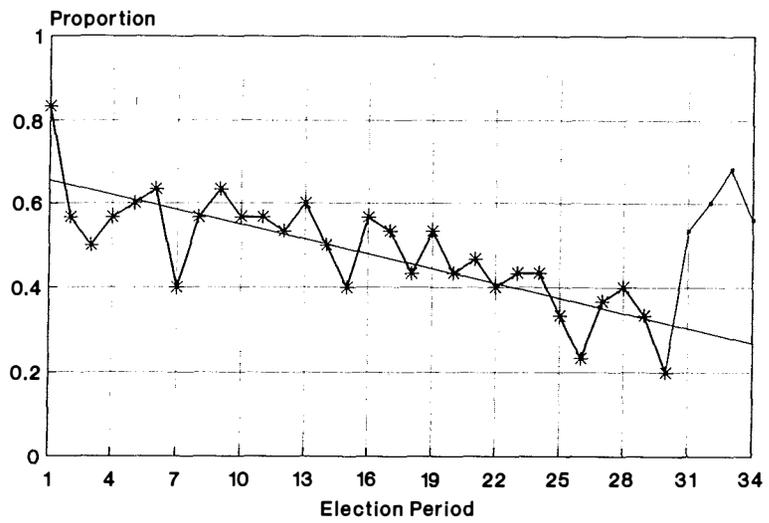


Figure 5d: Proportion Buying  
Experiment 2, promises kept

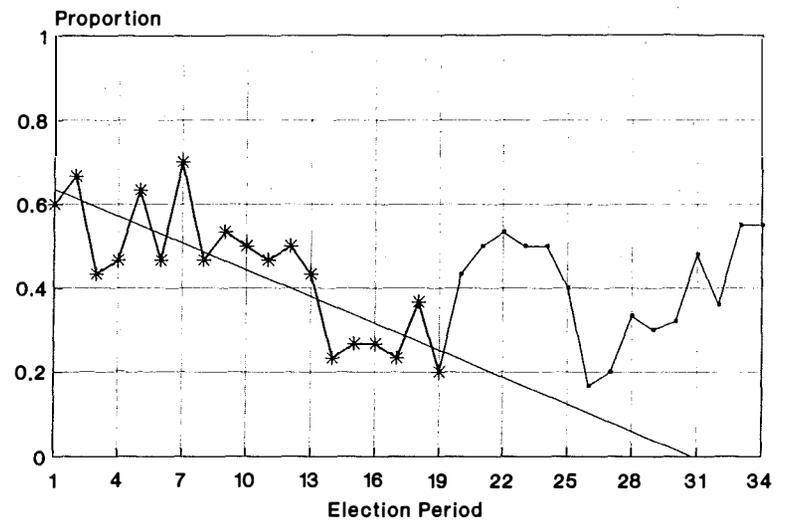


Figure 6a: Trend in Proportion Buying  
Exp. #1, periods 1-15

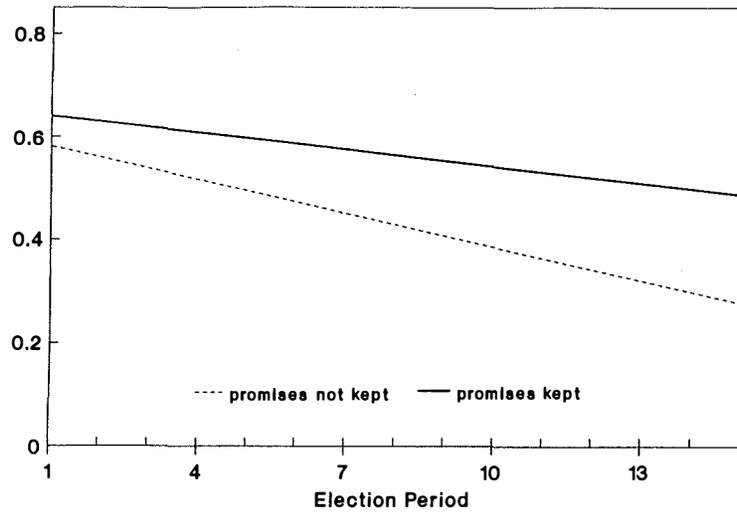
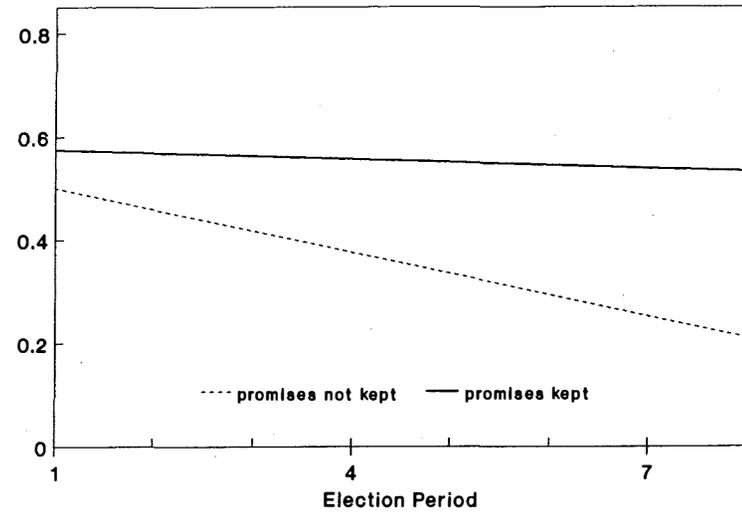


Figure 6b: Trend in Proportion Buying  
Exp. #2, periods 1-8



### 3. Testing the Hypotheses

Our analysis focuses now on some simple regressions in which we first partially disaggregate the data by experiment and election period. Within each election and period, we have this data: the period, each voter's payoff change from the incumbent, the previous plurality of the incumbent (pl), and the number of voters purchasing information. If we compute for each period the average of positive payoff changes (ppc), and the average of negative changes (npc), we can run the regression for each experiment type which Table 3 summarizes. This table also reports the regressions which result when all experiments are pooled, and dummy variables (d1 - d23) distinguish each of the twenty four experiments. In this and in all subsequent tables, standard errors are in parenthesis, \* denotes coefficients significant at .1, and \*\* denotes coefficients significant at .05 or less.

Table 3: Regressions by Experiment and Period

	promises not kept		promises kept		pooled with dummies	
const.	1.839	(.164)**	2.644	(.170)**	2.685	(.204)**
period	-.018	(.006)**	-.020	(.006)**	-.020	(.004)**
ppc	.006	(.011)	.042	(.011)**	.019	(.007)**
npc	.034	(.011)**	.001	(.019)	.014	(.006)**
pl	-.040	(.038)	-.115	(.040)**	-.044	(.026)*
d1 - d23	-		-		14 of 23 sig. diff. from 0	
n	382		386		768	
adj. R <sup>2</sup>	.06		.12		.35	
std. error	1.114		1.128		.989	

The results Table 3 reports warrant several comments. First, the sharp increase in adjusted R<sup>2</sup> when dummy variables are added for each experiment and the fact that 14 of 23 dummy estimates differ significantly from zero (at .05) are indicative of the idiosyncratic behavior which subjects exhibit, and the difference in average purchases depending on whether challengers keep promises. Second, except for the intercept and for increases in payoff (ppc), we cannot assert that coefficients are significantly different across the two experiment types -- although, with the exception of npc, the magnitudes of the coefficients for those experiments in which challengers keep promises are consistently greater. The coefficient for pl is significant when promises are kept, but it is insignificant and smaller when promises are irrelevant to future candidate positions. Thus, not only do subjects respond to the value of information by purchasing more when information has predictive value (notice the difference in the constant terms), but, in accord with hypothesis H2, increases in plurality tends to decrease purchases -- voters are less likely to purchase information if they believe that their vote is not likely to be decisive -- and, in accord with hypothesis H3, the relevance of plurality is greatest when promises are kept. The negative coefficient on "period" indicates that, even if we take the gradual convergence of candidates into account (via declines in ppc and npc), there is a general trend toward the purchase of less information as each experiment proceeds. Hence, subjects appear to be learning how to vote retrospectively. The one apparent anomaly which Table 3 reports is that the coefficient for ppc but not for npc is significant if promises are kept, whereas exactly the opposite is true if promises are not kept. We might offer alternative speculations to account for this difference, but this difference disappears when we examine estimates using data at the individual level.

Our suggestion that voters learn to act retrospectively leads to a seemingly paradoxical yet logical fact -- the correlation between the percentage of subjects voting correctly in an election and the percentage of voters purchasing information is negative.

To see this pattern graphically, consider Figures 7a and 7b, which show the proportion of subjects "voting incorrectly" -- the proportion voting for the incumbent if the incumbent's policy is worth less than the challenger's promise and for the challenger if the promise is worth less than the incumbent's policy -- for the first experiment in each series (since the candidates in the first but not the second experiment uniformly choose or promise distinct positions). Notice the decline in incorrect voting up to period 29, at which point the proportion of incorrect votes increases dramatically, since it is in this period that the candidates depart from their stable strategies. But Figures 5a and 5c, which graph the number of voters purchasing information, look quite similar to 7a and 7b, respectively -- hence the negative correlation. This correlation does not mean that if we possessed a measure of voter beliefs about candidates, we would find our measure correlating negatively with information purchases, since the stability of strategies from periods 16 to 29 should provide voters with a basis for establishing reliable beliefs, for choosing retrospectively, and, thus, for dispensing with direct purchases of information. And although our experiments abstract greatly from real election processes, we suspect nevertheless that the most prominent empirical manifestation of this correlation is the emergence of partisan as against "issue" voting in stable electoral systems -- in systems in which both the general issues which concern voters and the policy prejudices of the parties and their nominees remain relatively constant.

Disaggregating our data to individual subject observations tells much the same story as Table 3, although we must now use probit for our dichotomous dependent variable -- whether or not a subject buys information. Table 4 summarizes our results using these dependent variables: absolute change in payoff,  $apc$ ; a variable indicating the sign of the payoff change,  $sgn\ pc$  (virtually no change occurs in the estimates of other coefficients if we use the single variable, payoff change, in lieu of  $apc$  and  $sgn\ pc$ ); the magnitude of the plurality which the incumbent enjoyed in the previous election,  $pl$ ; the election

Figure 7a: Proportion of Errors  
Experiment 1, promises not kept

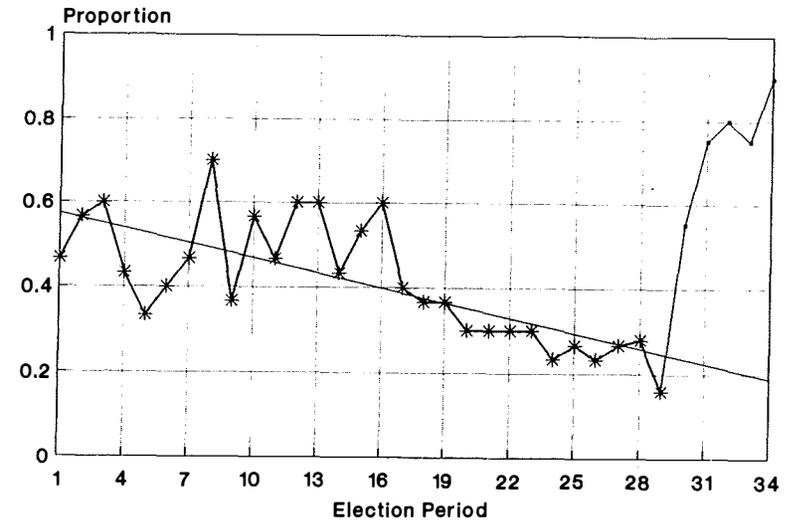
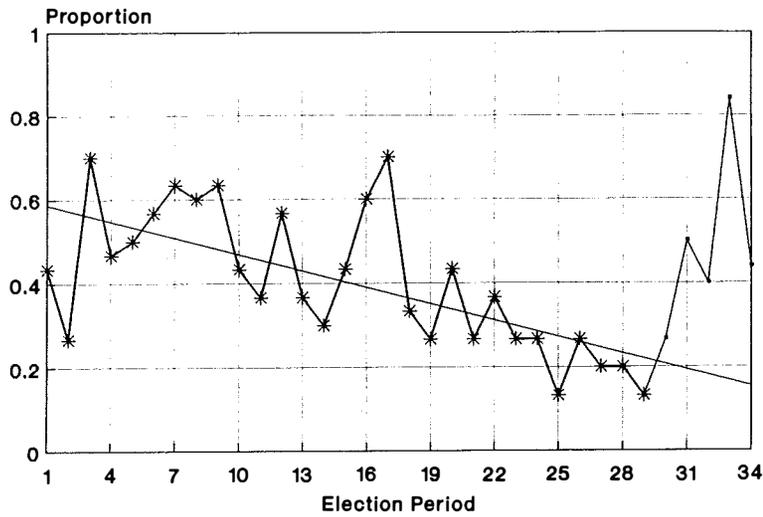


Figure 7b: Proportion of Errors  
Experiment 1, promises kept



period, period; and, when we pool all experiments, a dummy to differentiate between experiments in which challengers keep their promises ( $d_0 = 1$ ) and those in which they do not do so ( $d_0 = 0$ ). Aside from the fact that all coefficients have the signs anticipated by hypotheses H1 and H2, notice that both the magnitude and the significance of the coefficients for *apc* and *pl* are appreciably less for those experiments in which promises are not kept. Although there is a clear trend for the purchase of less information in experiments of both types as an experiment proceeds (the negative and significant coefficient for "period"), those variables which we might reasonably anticipate entering an expected value calculation about the utility of information -- *apc* and *pl* -- are significantly different from 0 at .05 only if that information has value. This is the result which hypothesis H3 anticipates. To test for income effects, adding the magnitude of a subject's payoff as a variable changes the coefficient for other variables insignificantly and does not change any of our qualitative conclusions, and itself has an insignificant coefficient. Thus, we can reject the hypothesis of such an effect.

Table 4: Individual probit results for buying information

	promises not kept	promises kept	pooled
const.	-.251 (.084)**	.138 (.083)	-.237 (.063)**
period	-.011 (.003)**	-.012 (.003)**	-.012 (.002)**
apc	.013 (.009)	.040 (.009)**	.027 (.006)**
sgn pc	-.278 (.040)**	-.103 (.038)**	-.186 (.028)**
pl	-.032 (.020)	-.060 (.020)**	-.043 (.014)**
d0	-	-	.366 (.042)**
n	1910	1930	3840
LLf diff.	168	38	199

Figure 8a: Proportion Loyal vs Buying  
Experiment 1, promises not kept

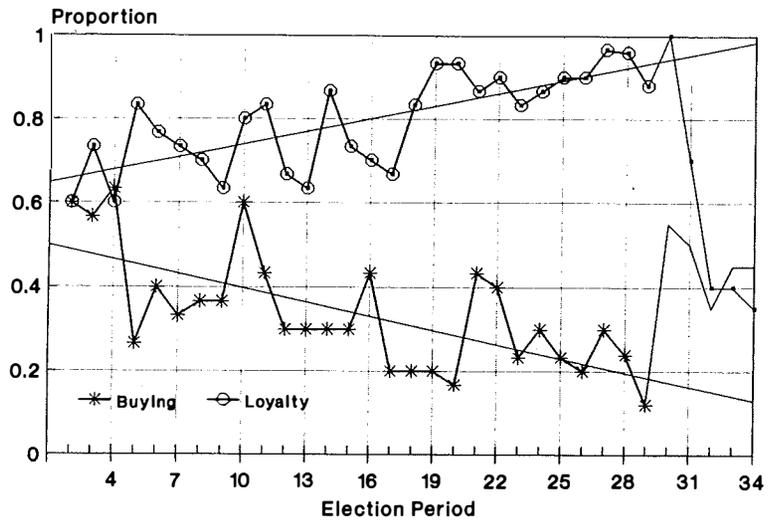


Figure 8b: Proportion Loyal vs Buying  
Experiment 2, promises not kept

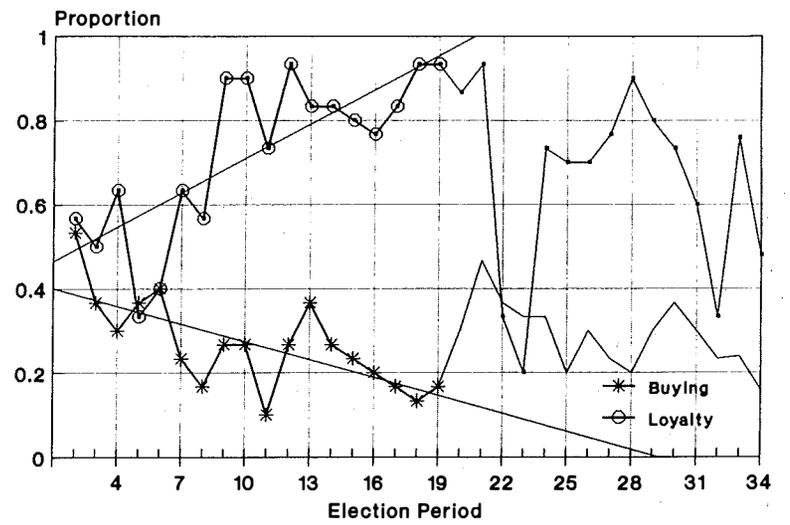


Figure 8c: Proportion Loyal vs Buying  
Experiment 1, promises kept

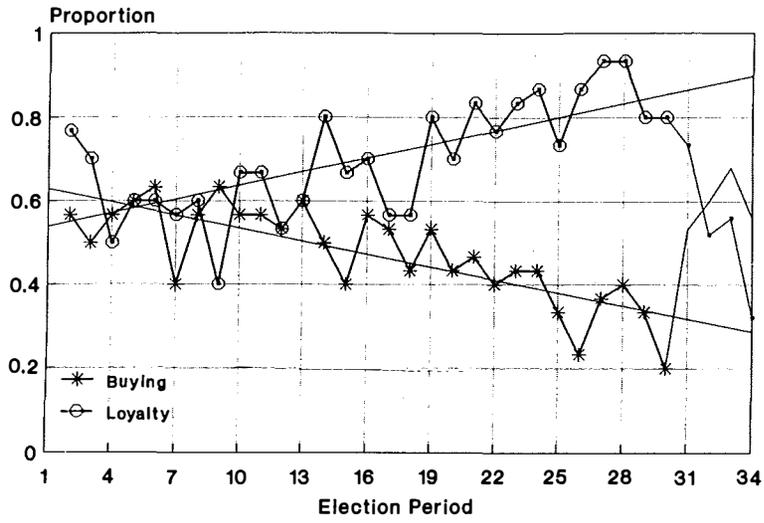
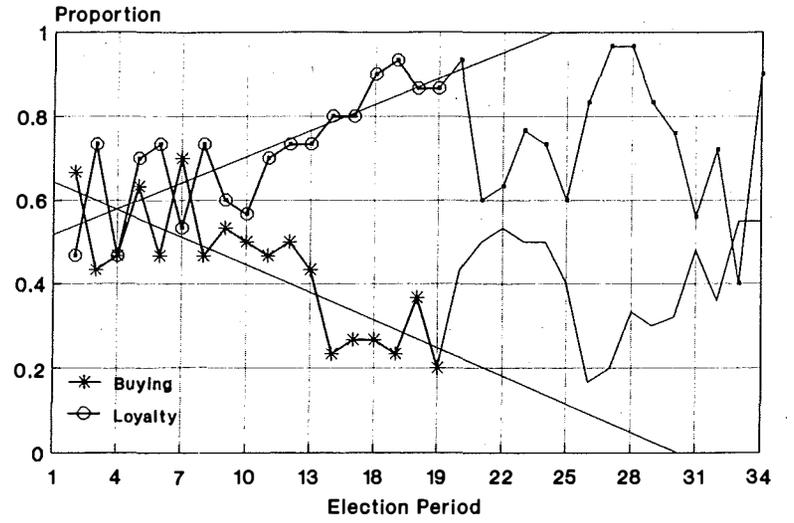


Figure 8d: Proportion Loyal vs Buying  
Experiment 2, promises kept



Finally, notice that although the coefficient for the sign of payoff changes is significant and negative for both sets of experiments -- subjects are more likely to purchase information when they experience decreases in payoffs than when their payoffs rise -- the coefficient is significantly smaller if promises are kept. The simple decision calculus which we offer in Section 2, however, predicts that payoff changes have symmetric effects, and, thus, a significant coefficient for *sgn. pc* is at odds with that model. Closer inspection of Figures 5b and 5d, though, suggest an extension of the analysis which Table 4 summarizes and which brings our conclusions about this coefficient more in line with theoretical predictions.

Recall that, in a session's second experiment, the candidates, after converging to 410, disequilibrate in period 20 for several rounds, converge to 690, and then, after seven rounds, disequilibrate again. The data in Figures 5b and 5d suggest, but only dimly, that voters increase their purchases of information during the two eras of disequilibrium and decrease their purchases when the candidates converge in the interim. In light of hypothesis H3, however, we are especially interested in ascertaining whether voters respond appropriately to the fact that promises are meaningless for the experiments which Figure 5b summarizes, but predictive for the experiments summarized by Figure 5d. Consistent with this hypothesis is the fact that forty percent of the electorate buys information in these periods if the candidates keep their promises, whereas only twenty nine percent make similar purchases if the candidate's do not keep their promises. Further, the probit estimates which Table 5 reports reveals that the variables which hypotheses H1 and H2 identify are relevant to decisions only when promises are kept: although neither the coefficient for *apc* nor the one for *pl* are significant at even the .1 level when promises are not kept, both are significant when promises are kept.

Table 5: Probit results for periods 20-34, exp. #2

	promises not kept	promises kept
const.	.102 (.434)	.135 (.426)
period	-.024 (.016)*	-.013 (.015)
apc	.005 (.018)	.035 (.016)**
sgn. pc	-.336 (.086)**	-.129 (.082)
pl	-.016 (.045)	-.073 (.044)*
n	440	415
LLf dif.	50	13

Finally, looking at the coefficient for *sgn. pc* when promises are kept, the inference we draw is that "experienced" subjects -- those who have participated in one and a half experiments -- respond equally to positive and negative payoff changes. Although the coefficient we estimate for this variable remains negative, it is not significantly different from zero, as it is if promises are not kept and as it is for both type of experiments overall (see Table 4). And it is this symmetric response which is consistent with our model in Section 2.

There is one final pattern in our data, illustrated in Figures 8a - d, which is maintained regardless of how we aggregate that data and which bears on our fourth hypothesis, about loyalty and information. These figures add to Figure 5a - d, respectively, the frequency with which subjects vote for the same candidate as they voted for in the previous election (and the trend lines for this data), and they document the increasing degree of "loyalty" as the candidates converge which accompanies the decreasing propensity to purchase information, up to the point which the candidates' strategies diverge. Thus, we observe overall a negative correlation between loyalty and information purchases.

Naturally, we should be concerned that this correlation can be accounted for by the fact that loyalty and the likelihood of buying information correlate with the same variables -- notably the election period (period), the absolute change in a voter's payoff (apc), and the previous plurality of the incumbent (pl). The probit results which Table 6 reports, however, show that, although the effects of period, apc, and pl are in general significant and of the appropriate signs (loyalty increases as an experiment proceeds, decreases with sudden changes in payoff, and increases as previous election margins increase), the decision to buy information has a uniformly significant independent effect on a subject's degree of loyalty and, in particular, buying information decreases one's chances of remaining loyal.

Table 6: Individual Probit Results for Loyalty

	promises not kept	promises kept	pooled
const.	.307 (.091)**	.387 (.093)**	.352 (.065)**
period	.002 (.003)	.010 (.003)**	.006 (.002)**
buy	-.173 (.068)**	-.200 (.062)**	-.184 (.045)**
apc	-.066 (.009)**	-.047 (.009)**	-.057 (.006)**
sgn. pc.	-.104 (.041)**	-.247 (.040)**	-.181 (.028)**
pl	.199 (.022)**	.199 (.022)**	.149 (.016)**
LLf diff.	289	230	507

#### 4. Conclusions

Our experimental data confirm the supposition that voters purchase less information and rely more on retrospective knowledge when the candidates' strategies are stable. Their likelihood of purchasing information during periods of instability, however, is tempered by the likelihood that their votes matter, by the reliability of the information available for purchase, and by the degree of instability as measured by changes in

welfare. Aside from these correlations, individual decisions also appear to be idiosyncratic. Despite the statistical significance of our estimated parameters, variances explained, even when we aggregate the data in various ways, remain low by most standards (the highest adjusted  $R^2$  reported for any regression, in Table 3, is only .34). Raising the stakes in an election might reduce unexplained variance considerably, but we should also be prepared to think of political information as a consumption good, the purchase of which is determined as much by taste as by a rational calculation based on the character of an election.

#### Appendix: Instructions

This experiment is a study of voting in elections. As subjects you will each be assigned to be either a voter or a candidate, and you will each be paid for your participation on the basis of the decisions which you make. If you are careful and make good decisions, you can make a substantial amount of money. However, the payoffs in the experiment are not necessarily fair. Your payoff depends partly on your decisions, partly on the decisions of others, and partly on chance. The experiment uses computer terminals, and when we begin you will each be assigned to a terminal. The entire experiment will take place through a network connecting the terminals.

In this experiment, there are two candidates, A and B, and the rest of you are voters. The experiment is divided into a number of election periods, and each period consists of six steps. Except in the first period when the initial incumbent is chosen randomly, the voters first all vote whether to elect candidate 1 or 2 -- that is, whether to make 1 or 2 the incumbent for that period. Second, the vote is tallied and announced, and the winning candidate becomes the "incumbent" for that period. Third, both the incumbent and the challenger each choose an election strategy. For the incumbent, that strategy represents his policy for his term in office. The challenger's strategy, on the other hand, serves merely as that person's promise about what he or she might do if elected. Voters do not learn either the incumbent's or the challenger's strategy. Instead, in the Fourth stage, voters are told what the incumbent's policy is worth to them. This payoff is presented in "Franks", which can be exchanged for dollars at the end of the experiment. After the incumbent adopts his policy, and the challenger his promise, the computer computes how much each policy is worth to each voter, and the value of the incumbent's policy is displayed on the voter's terminal and is also added to the cumulative total earned by the voter. Fifth, after learning the value of the incumbent's policy, each voter must decide whether or not to buy information about the

opponent's promise. If this information is purchased, that voter will have the fixed amount of 2 Franks deducted from his or her final payoff. Finally, if a voter indicates that he or she wishes to buy information about the challenger, the value of the challenger's promise is revealed to that voter. Thus, if information is purchased, the value of the challenger's promise along with the value of the incumbent's policy are revealed to the voter. But if no information is purchased, only the value of the incumbent's policy is revealed.

At this point, the period will end, and a new election will take place. The voters must then decide whether to keep the incumbent in office for another period, or to elect the challenger. If the incumbent is reelected, he or she must again choose a policy. Whether this policy is the same as their previous policy is up to the incumbent. And if the challenger is elected, the challenger's policy need not correspond to that person's prior promise to the electorate -- again, no constraints are placed on the candidates' policies once they become incumbents.

Summarizing, an election period consists of (1) voting, (2) announcement of the winner; (3) selection of policies by the incumbent and the challenger; (4) revelation of the value of the incumbent's policy to the voters; (5) the voter's decision whether or not to purchase information about the challenger; (6) revelation of this information to those who purchase it. We note that a voter's payoff in a given period does not depend on who you that person voted for, but only on the policy adopted by the current incumbent. Thus, if 1 is elected, and if 1 adopts a policy which gives you 4.50, then you receive that amount regardless of whether you voted for 1 or 2 -- less, of course, the cost of information if you chose to purchase information. Your vote can affect who is elected, but once that candidate is elected, you have no further control over your payoff. Your payoff in a given period depends entirely on the policy that the incumbent decides to adopt after he or she is elected.

One important rule of this experiment is that once we begin, no one is allowed to talk to anyone else. The only communication permitted is the communication that occurs over your computer terminals. The candidates are paid for their participation in the experiment on the basis of the number of elections that they win. Thus, each time a candidate is elected as the incumbent, he receives a fixed payoff. This will be displayed on the candidate's screen, and added to his cumulative total. Also note that in each period, the candidates, on their screens, learn only the total vote, not which voter voted for which candidate. Nor do candidates learn who or even how many voters purchased information. The payoffs of voters, as we have already stated, depend on the policy of the incumbent. No one including the candidates will be told, however, the relationship between policy and payoffs. And different voters may have different functions relating payoffs to policy. Thus, a particular policy may greatly benefit one voter but be strongly disliked by another. Voters will also not be told the maximum payoff possible in a period. The process we have described will continue for a fixed number of periods, or until a given time has elapsed. At this point, the first experiment will end, and you should each record your cumulative payoff on the sheet provided. Then, a second experiment will begin, which will be similar to the first, except that the function relating each voters' payoff to the incumbent's policy may be different than in the first experiment. Between each experiment voters will be shuffled and the candidates may be relabeled. So, for example, if candidate 1 wins many more elections in the first experiment, you should not suppose that letting 2 win more in the second will equalize payoffs --it may benefit the same person.

After two experiments, the session will end and you will all be paid on the basis of the earnings you have accrued. To compute your total payment, add the amount you have earned from each of the two experiments, and multiply by the exchange rate, which is listed on your record sheet. Enter this amount in the final column of your record

sheet, and submit it to me to receive your payment. Before beginning the actual experiment, we will have a 5-period practice session. Also, the sample sheet before you gives you some idea what a voters terminal screen looks like. Various messages will appear at the bottom of the screen prompting you to vote or to decide whether or not to purchase information. The columns denote the period number, the vote totals of the two candidates, the winner, your payoff from the winner's policy (entered under either the column for 1 or for 2), and, if you purchased information in that period, the value of the promise of the challenger. You will not be paid for the trial session, although the payoffs you would receive if this were a real session will be computed. After the trial session, please ask questions. I would like to emphasize at this point that the number of rows on the screen should not be interpreted as limiting the number of periods in an actual experiment since data on the screen can scroll up and off the screen to make room for as many periods as we choose.

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