Using Polls to Measure Pork

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Abstract

Polls are naturally used by politicians and businesspeople to form expectations over political outcomes, but every poll comes with its margin of error. We suggest that the shock to expectations caused by voting realizations provides us with an 'innovation' which can be used to perform an event study. In particular, forward-looking business behavior prior to the election is optimized relative to expectations formed through polls, and immediately after the election it adjusts according to the new information revealed by the election itself. If there is no reaction to the surprise vote, business behavior is not a function of political behavior, and so we infer that no credible promise of pork has been made. Variation in the magnitude of this response across electoral districts therefore gives us a way of identifying the local-level characteristics that make it both desirable and credible for politicians to use pork. Using data from Ugandan microfinance investors, we test the discontinuous response across counties of Uganda to the 2001 presidential election. Controlling for local variation in violence, threat-making, and explicit promises of pork, we find that both incumbent and opposition politicians are highly credible.

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1. Introduction.

Electoral politics has long been viewed as a strategic interaction between voters and politicians. Voters delegate governance with an expectation that the public and private goods provided will increase their welfare. Politicians, on the other hand, likewise seek the benefits they can accrue through office, both as a citizen-candidate and from the ability to extract rents. As such, voters choose candidates based on their estimations of politicians' policy promises according to mechanisms of selection (Fearon 1999a) and sanctioning (Ferejohn 1986). In turn, politicians court voters in an attempt to minimize the distribution required to obtain office (Alesina 1998; Dixit and Londregan 1996). In this paper we build on this common conception of the electoral game in two ways—one theoretical, the other empirical.

Theoretically, our model highlights the importance of the credibility problem that both politicians and voters have in promises to exchange votes for pork (see also Robinson and Verdier 2003). As Keefer (2004) points out, most analyses either assume that politicians' promises are universally credible (cf. Cox and McCubbins 1986; Lindbeck and Weibull 1987; Dixit and Londregan 1996) or that they never are (cf. Ferejohn 1986; Alesina 1987; Besley and Coate 1997). Along with a few recent attempts to reassess these assumptions, we argue that between these two extremes is a much more common set of intermediate cases in which politicians' credibility varies according to the nature of their relationship with voters.

We model the circumstances in which both voters and politicians possess the attributes that will make possible credible patronage promises, and suggest an empirical test based on event studies from the finance literature. The test is similar in spirit to Fisman (2001), but the use of the surprise vote makes the technique generally applicable and we do not need special data on the connections between politicians and specific firms. The idea behind this test is that forward-looking business behaviors (such as stock prices, business borrowing, housing starts, or investor confidence indexes) will respond in a discontinuous fashion to the revelation of new information which alters economic expectations. The gap between local voting outcomes as predicted by polls and the actual vote on election day reveals new information about the extent of local support for national politicians. The margin of error in election polls generates an

informational shock which is (in theory) randomly distributed and observable for numerous electoral units within a country. If any link exists between voting and distribution (Finan (2004) shows that such a link exists in Brazil, and Miguel and Zaidi (2003) in Ghana), then business behavior will respond. Since we can use the discontinuous shift in economic behavior observed just after the election, the statistical test laid out in this paper is both well-identified and new to the literature.

If politicians cannot credibly commit to redistribute on the basis of voting, then business will not respond, and so an election won by an incredible politician sees no alteration in economic behavior as a result of the surprise vote share. Our model agrees with both Keefer (2004) and Robinson and Verdier (2003) that the primary determinants of such two-sided credibility are the extent of repeated interaction between politician and voter and the propensity for collective action on the part of voters. For Keefer this is achieved either through investments aimed at building national (and thus long-lasting) parties or by 'buying' into the credibility that underlies existing clientelistic networks. The existence of such networks is the primary way in which voters can coordinate—patrons provide the coordination on the use of a retrospective voting rule.[1]

The focus on clientelism, as usually defined, presents empirical problems because these networks are not in general observable to researchers. Given that these networks are relatively well-understood by locals, however, this suggests that we should be trying to infer from the way that *locals* respond to new political information whether they foresee patronage. In this paper we suggest a definition of 'pork' (namely, any political transfer that is contingent on voting behavior) which is imprecise in terms of channels but which opens up a variety of empirical tests. If a politician is credible in any part of the country during an election, we can use cross-sectional variation to measure the *magnitude* of the response to the surprise vote share. This allows us to test for the local-level characteristics which make the electorate able to credibly contract with politicians. Using this cross-sectional variation, we suggest and implement

[1] Similarly, Robinson and Verdier (2003) argue that the groups with whom "politicians can credibly exchange with will be determined by the social network of individuals whose behavior they can observe relatively well—perhaps because they interact socially with them." They likewise conclude that clientelistic relationships are the most frequently-used mechanism for engaging in such credible exchanges.

tests for such diverse questions as whether politicians target core or swing voters, which ethnicities believe a particular politician, and whether winning specific sub-regions matters to politicians in a majoritarian system.

We proceed to specify the model and empirics in greater detail. In Section 2 we derive from a theoretical model an array of empirical tests that can be performed using the response in forward-looking business behavior to this surprise vote share. Using data which span the 2001 Ugandan presidential election as described in Section 3, we carry out these empirical exercises in Section 4 and find significant mean effects using a variety of metrics. Section 5 provides a discussion of robustness of the estimators described in the empirical section, and Section 6 concludes with some suggested directions for future research using this way of measuring pork and campaign credibility.

2. Model.

As investors approach an election, they attempt to incorporate the likely results of that election into their intertemporal investment behavior. Hence information (such as a pre-election poll) that allows them to forecast political outcomes is used to make forward-looking investments prior to the election. A politician approaching this election has a similar informational problem—whether and how to make contingent promises directly to constituents in order to drive up votes; that is, whether to use pork. In assessing where promises of pork are most useful, the politician also will use poll results to gauge how responsive the electorate has been to recent campaign promises in different regions of the country. In this sense a strategic dance takes place between a national politician and local voters in which poll outcomes give both sides the most direct source of information over the uncertainties posed by the election.

Following voting models as described by Lindbeck and Weibull (1987) and Stromberg (2004) we model elections as a two-period game between voters and a politician in which the first period is prior to the election wherein the politician makes promises over post-election pork which maximize his expected

utility.[2] In this period opinion poll outcomes are observed, and the equilibrium in the pre-election period is the point at which voters, businesspeople, and the politician have optimized their behavior conditional on their best guess of how others will play. In the second period the election outcome has been observed, and the error between the poll and the observed election outcome represents a stochastic shock to which equilibrium behavior may adjust. The locals understand the behavior of the politician well enough to know what his *ex post* optimal payment will be, and so the politician may face a credibility constraint that causes promises to be ignored by voters.

If politicians could contract with individuals, they would do so. Because most modern voting systems are anonymous at the individual level, however, we assume that politicians attempt to contract with voters at the smallest unit at which they can observe outcomes (hereafter the county).[3] Therefore the politician makes offers of 'pork' to counties as a contract contingent upon voting behavior. We define pork broadly to include any form of distribution (whether patronage, subsidy, favoritism, or local public good) which is offered strategically to voters in a given electoral district in order to influence their voting.[4] The short-term, strategic use of pork as defined here is contrasted with promises regarding issue positions and programmatic policies that are less malleable in the short term (Dixit and Londregan 1996). The model is similar in spirit to Stromberg (2004) and Ferraz & Finan (2005) in that politicians balance the benefits of diverting money against spending it to influence elections, however we focus explicitly on whether promises that the politician wishes to make *ex ante* will be credible *ex post*. Hence we focus our attention on expectations in period 1 over the political transfers that will be made in period 2.

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[2] Payments to voters prior to the election cannot be time-consistent in the absence of contracting; voters will simply take the money and then vote for whichever politician they prefer (Robinson and Verdier 2003).

[3] In systems such as Kenya's where elections line up voters to be counted in public, there is no reason to think that contracting with individuals will not exist. Anecdotes of the more colorful kind of individual contracting mechanism abound, from the failure to oil one of the levers in the old voting machines in Tammany Hall (Popkin) or the use of camera cell phones to transmit an image of the completed ballot in Mexico (Molinar).

[4] For relevant discussions regarding the role of clientelism in such behavior, see Keefer (2003) and Robinson and Verdier (2003).

Full Credibility.

We begin by modeling the simple case where the politician has a commitment device available in period 1 which makes him able to make time-consistent promises over period 2 behavior.

2.1. The Politician:

If elected, the politician has a pool of resources denoted by *K* at his disposal which can be diverted to private uses or used to influence the behavior of voters. The politician maximizes utility from *K* by balancing the utility from diversion against the increased vote share that can be achieved by spending on pork (cf. Olson 1993). In order to affect the outcome of the election, this pork must be promised (explicitly or implicitly) prior to the election in the form of campaign promises.

The politician has a period 1 net present valuation Ω_1 of being in office in period 2 (and 0 if not in office) and uses transfers t_c to each county c to increase the probability of re-election, $F(t_1,..,t_N)$. For simplicity, we model a majoritarian system where counties have the same number of voters so that we can write this total probability as $F(.)=\frac{\sum_c f(t_c)}{N}$, where $f(t_c)$ is the vote share received in county *c*. Voters in a given county *c* have a distribution of preferences towards the politician such that, in the absence of any transfers, their voting outcome will be $f_c(0)$.[5] If there exists a large mass of voters who are ambivalent between the pork-paying politician and the opponent, or if voters are easily bought, then we will see a high slope of the function $f(.)$ as the politician directs transfers towards a county. This derivative with respect to transfers to any given county is denoted by $f'(t_c)$, and we assume $f'>0$ and $f''<0$. This implies that higher transfers to a county increase the vote share from that county, but that there are diminishing marginal returns to pork. The justification for this is that a rational politician will do first those things that are most effective but as an increasing amount of pork is directed to a given region it is pushed into projects to which voters are less responsive. Even under full credibility, voters know they

[5] This way of modeling the problem is consistent with the presence of a second politician making fixed offers of patronage.

will only receive the transfer if the politician is elected (which the voters expect to occur with probability $\hat{F}^V$), and so the expected value of a promised transfer is $\hat{F}^V t_c$. The actual number of votes achieved by a promise of t_c is thus $f(\hat{F}^V t_c)$.[6] We assume that if $\hat{F}^P$, the politician's estimate of his own probability of victory, differs from $\hat{F}^V$, the voters', that the true value F lies weakly between these estimates.

Given a linear utility from diversion $\omega * \left(K - \sum_c t_c \right)$ and a utility of 0 if not elected, the politician's problem is to:

$$\underset{t_c}{Max} \quad \frac{\sum_c f(\hat{F}^V t_c)}{N} \left[\Omega_1 + \omega(K - \sum_c t_c) \right],$$

which has the first-order condition $\frac{\hat{F}^V f'(.)}{N} \left[\Omega_1 + \omega(K - \sum_c t_c) \right] = \hat{F}^P \omega$, meaning that the expected marginal benefit of promising a transfer to a county will equal the expected marginal opportunity cost of the transfer. This expression can be rearranged as:

$$f' = \frac{\hat{F}^P}{\hat{F}^V} * \frac{\omega N}{\Omega_1 + \omega(K - \sum_c t_c)} = \frac{\hat{F}^P}{\hat{F}^V} * \frac{\omega N}{\text{benefit of office}}.$$

$\frac{\hat{F}^P}{\hat{F}^V}$ is the ratio of the politician's to the voters' predicted probability of victory in the national election. This term might be called politician's 'confidence'. Where $\frac{\hat{F}^P}{\hat{F}^V} > 1$, the politician is overconfident and thinks that the voters are more responsive to pork than they actually are. When $\frac{\hat{F}^P}{\hat{F}^V} < 1$ the politician is underconfident and promises 'too many' transfers, being too pessimistic about his chances and underestimating how easily votes can be bought.

[6] For simplicity we assume that the number of counties N is large enough that we can ignore the marginal effect of increased promises to a given county on voters' estimated probability of overall victory in the national election, $\hat{F}^V$. The additional term substantially complicates the derivative and adds little to the intuition of the local response to promises of pork.

A primary role played by polls is the coordination of expectations, though the transparency of the polling process and freedom of the media in general obviously affects the extent of coordination between candidates, voters and businesspeople.[7] In any election conducted with commonly observed national polls we should see $\frac{\hat{F}^P}{\hat{F}^V} = 1$, leaving us with a first-order condition of: $f' = \frac{\omega N}{\Omega_1 + \omega(K - \sum_c t_c)}$.

The right hand side is a function only of the politician's parameters, and so is a constant across counties which we will denote by W_1. Hence the interior solution to this problem is $f' = W_1$, and so any county that receives transfers will have the same slope on the 'voting production function'.

Figure 1 illustrates this, where counties 1 and 2 are at interior equilibria, and county 3 receives no transfers in equilibrium. If counties exist where even the first unit of transfers have lower probability effects than the opportunity costs (such as county 3 in Figure 1), then these counties will receive no transfers in equilibrium. The optimal transfers are thus:

$$(1) \qquad \begin{array}{ll} f'(.) = W_1 & \forall\ c \text{ s.t. } f'(0) \geq W_1 \\ t_c = 0 & \forall\ c \text{ s.t. } f'(0) < W_1 \end{array}.$$

2.2 Investors:

Politics affects business outcomes both through the provision of transfers and through the direct effects of policy itself. The polls are used by investors to form expectations over both of these distinct processes—to understand how local voting will alter the level of transfers and to predict the outcome of the election itself. We begin by forming rational expectations for voters given the model of provision of local-level transfers outlined above.

Investors understand the local environment well enough to infer the function $f(\hat{F}t_c)$ and engage in some intertemporal investment behavior which is increasing in t_c. Businesses read the curve that relates pork to probability of election in reverse, by using polls to forecast the extent of pork that their region will

[7] Diaz-Cayeros, Magaloni and Weingast (2003b) argue that state control of the media was important in maintaining the PRI's political dominance in Mexico "not because it brainwashed people, but because it portrayed a strong PRI and a weak opposition", thus increasing the value of the PRI's promises of pork.

be provided (shown in Figure 2). We refer to the business metric as $\pi_c\left(F, f^{-1}(vote_c)\right)$, where π_c is an increasing function of $t_c = f^{-1}(vote_c)$ and F indicates the outcome of the national election.[8] We write the surprise vote share as ε_c, where $vote_c = poll_c + \varepsilon_c$.

Figure 3 illustrates the linear case using best responses to the model of pork developed in Section 2.1; because counties 1 and 2 receive positive transfers in equilibrium then $\frac{d\pi_c}{d\varepsilon_c}$ gives us the slope of $\pi_c(f_c^{-1})$ in those counties, and in county 3 there is no slope because there were no transfers in equilibrium. For a county that does not expect to and does not receive pork, $\frac{d\pi_c}{d\varepsilon_c} = 0$. For some counties, however, the surprise vote share switches it from one which expected to receive pork to one that did not, or vice versa. In this case $\frac{d\pi_c}{d\varepsilon_c} > 0$, but it will also be true that $\frac{d\pi_c}{d\varepsilon_c} < \pi_c f_c^{-1}$ because f_c^{-1} is a convex function, meaning that the slope of the line which connects $f_c(0)$ and the optimal transfer has a less steep slope than $(f_c^{-1})'$ evaluated at the optimal transfer. The 'switchers', then, have slopes on their response functions which are intermediate between the zero slope of the no-pork counties and the slope of a pork county.

The second distinct use of the polls is to forecast the outcome of the national election $\hat{F}$. Because the outcome is binomial, the variance of the electoral outcome is $\hat{F}(1-\hat{F})$. The mean and variance fully characterize the binomial distribution, and so we model investment response to $\hat{F}$ in a mean-variance form: investment prior to the election is based on the expected outcome, and is depressed by the variance of the outcome. An investor who is indifferent to the outcome of an election displays no response to the variance. Let $\phi_c = \pi\left(F=1, f^{-1}(poll_c)\right) - \pi\left(F=0, f^{-1}(poll_c)\right)$ be the difference in optimal outcomes that arises solely as a result of 'policy' effect, holding transfers constant. From here we can model variance

[8] Metrics of π which are readily observable and could be used include business borrowing, local investor confidence indices, local housing starts, and stock prices of companies based on the location of their headquarters.

as entering the investor decision by suppressing pre-election investment through a term $-\rho_c|\phi_c|\hat{F}(1-\hat{F})$, where ρ_c is a county-level coefficient of risk aversion.[9]

Immediately before the election, then, we will observe an outcome based on the expected mean and variance of optimal behavior under the winner and the challenger:

$$\pi\left(\hat{F}, f^{-1}(poll_c), \rho_c, \phi_c\right) = \left[\hat{F}\pi\left(p=1, f^{-1}(poll_c)\right) - (1-\hat{F})\pi(p=0, f^{-1}(poll_c))\right] - \rho_c|\phi_c|\hat{F}(1-\hat{F}).$$

If there is no risk aversion, the outcome observed prior to the election is a simple weighted average based on optimal behavior under the two candidates and the information in the polls. As ρ_c increases we see this pre-election investment outcome depressed by the uncertainty induced by the election.

Immediately after the election we see an outcome which maximizes profits given the realization: $\pi\left(F, f^{-1}(vote_c)\right)$. From here we can form the discontinuous change in outcomes seen around the election as:

$$\Delta\pi_c = (post-election\ equilibrium) - (pre-election\ equilibrium)$$

$$\Delta\pi_c = \pi\left(F, f^{-1}(vote_c)\right) - \pi\left(\hat{F}, f^{-1}(poll_c), \rho_c, \phi_c\right)$$

By adding and subtracting the outcome that would have been observed after a victory if the outcome exactly equaled the poll, $\pi(F=1, poll_c)$ and collecting terms, we get:

$$\Delta\pi_c = \Delta\pi_c(F=1, \varepsilon_c) + \phi_c(1-\hat{F})(1+/-\rho_c\hat{F}) \qquad for\ \ c\ \ s.t.\ \ \phi_c <\!/\!> 0$$

$$= \varepsilon_c \frac{d\pi(f_c^{-1}(.))}{d(vote_c)} + \phi_c(1-\hat{F})(1+/-\rho_c\hat{F})$$

The first term on the RHS shows the jump in investor outcomes that can be attributed to ε_c *and not* to the surprise in the overall election, whose effect is given by $\phi_c(1-\hat{F})(1+/-\rho_c\hat{F})$. The intuition for the ambiguous sign of the variance effect is that investors who preferred the challenger ($\phi_c < 0$) have their

[9] This way of expressing risk aversion is consistent with a story wherein fixed investment decisions are influenced by political concerns, and so businesses refrain from sunk investment until the outcome of the election is known.

outcomes distorted by risk in the *same* direction as the effect of the discovery of the election outcome, while those who preferred the victor see outcomes distorted in the *opposite* direction by risk.

As $\hat{F} \to 1$ there is no overall 'surprise' present in the election, and as $\phi_c \to 0$ outcomes for business are identical under the two candidates. In these cases only the local-level shock is informative and no meaningful innovation is contained in the outcome of the national election. In other words, we can decompose the surprise generated by the election into two components: 'who won?' and 'did we back the winner?'.

This decomposition of the change in investment is useful because it clarifies the orthogonality assumptions that must made to draw causal inference from the correlation between ε_c and $\Delta\pi_c$. In an election with a heavy favorite, $\hat{F} \to 1$ and we can isolate the effects of ε_c. In this case for an incremental $\partial\varepsilon_c$, $\frac{d\pi_c}{d\varepsilon_c} = \frac{\partial(\pi_c(f_c^{-1}(.))}{\partial(vote_c)}\Big|_{poll_c}$. Consequently, the change in business behavior over the surprise vote share allows us to measure the marginal responsiveness of business to the local vote.[10] In such an election, the correlation between $\Delta\pi_c$ and ε_c allows us to ask the following question: do investors alter their behavior in regions that unexpectedly backed the winner of the election?

If we have $\hat{F} < 1$ and $\phi_c \neq 0$, there will be some discrete adjustment on average to the announcement of election results. In this case, use of the correlation between $\Delta\pi_c$ and ε_c requires the additional assumptions that $\varepsilon_c \perp \phi_c$ (the surprise vote share is orthogonal to the local benefit from victory) and that $\varepsilon_c \perp \rho_c\phi_c$ (the surprise vote share is orthogonal to the product of local-level risk aversion and local benefit). Wlezien & Erikson (2006) characterize the error in a poll as arising from three components: sample design mistakes, a bias in the 'house' that conducts the poll, and random sampling error (including errors in the questionnaire). Our polls come from a single surveyor, and so by demeaning

[10] Note that a poll which is inaccurate at the county level generates more variation in ε_c than an accurate poll, and so improves the statistical identification of this relationship!

ε_c we can remove the house effect. The random error is orthogonal to county-level characteristics, and so the assumption required in elections that contain 'surprises' can be stated as the following: the error that arises from sample design flaws must be uncorrelated with variation in investor political preferences and in risk aversion.

We have a simple test for whether the election itself was not a shock to outcomes: if the term $E\left(\phi_c(1-\hat{F})(1+/-\rho_c\hat{F})\right)$ is non-zero, we will observe a discontinuous shift in outcomes across all units. This suggests that we test for the presence of a mean discontinuity around the time of the election. In section 5.1 we perform this test and find no such discontinuity.[11] Section 5.3 conducts tests to verify the randomness of ε_c. Given Museveni's large lead in the polls during the 2001 Uganda presidential election, and the absence of a mean shock during the election, we proceed to investigate the influence of credibility in this three-sided game under the assumption that our tests are correctly isolating a the effect of ε_c.

2.3. Credibility in the absence of a commitment device.

In the absence of a commitment device, behavior by both voters and the politician must be time-consistent. Appendix A3 discusses the circumstances under which the voters will tell the truth in polls. We argue that this will prove a dominant strategy. Credibility then becomes a function of two factors: first, the politician's period 2 benefit from actually paying transfers, and second, the ability of voters in a specific county to punish the politician in future elections for not carrying through on promises. We refer to this second characteristic as the 'memory' of voters.

[11] In developed-country two-party systems we might expect the primary political cleavage to be labor vs. capital, in which case investors are likely to share preferences over political outcomes, and so ϕ_c has the same sign for most agents. In this environment if $\phi_c \neq 0$ we should see a *mean* shift in outcomes as, for example, markets react to the election of a pro-business president. In an African country, where cleavages follow less strictly socio-economic lines, it is more likely that the average national effect has a zero-sum component, where the responses of the winners and the losers to the election balance on average. Thus failing to find any average shock to outcomes cannot be considered to have eliminated the possibility of a correlaction between ϕ_c and ε_c in the Ugandan context.

Voters' memory refers to their ability to ensure that elected politicians are held accountable for their performance in government. Accountability, of course, is a much-studied topic in political science. In the context of our analysis, voters' memory may be affected by many factors. First, the frequency of interaction between voter and politician directly affects the latter's future payoffs insofar as repeated interaction can create reputational costs if promises are not kept (cf. Alesina 1987). Second, in order to credibly commit to retrospective sanctions voters must find ways to overcome collective action problems. As in other areas, issues such as group size (Olson 1965), density (Bates 1981) and identity or ethnicity (Fearon 1999b) might all affect the credibility of voters' threats to act collectively. As noted in the introduction, many authors argue that voters are severely constrained in their ability to credibly threaten to punish ex post (Fearon 1999a; Alesina 1987; Besley and Coate 1996).

Third, the importance of pork for voters (either individually or as a group) can be significant. In keeping with a long academic tradition, voters are commonly perceived as accruing both ideological and material or particularistic benefits from governmental policies (Cox and McCubbins 1986; Lindbeck and Weibull 1987; Dixit and Londregan 1996). The relative weights of these two components will likely affect the extent to which voters can credibly threaten to sanction the failure of politicians to deliver pork. Many authors show how income levels affect the relative importance of pork—a bag of rice matters more for poor voters than for rich ideologues (Dixit and Londregan 1996; Calvo and Murillo 2004; Keefer 2003). Similarly, Stokes (2005) argues that with regard to core supporters ideology typically dominates and attention to material benefits is minimal. As a result, threats from the core to switch to another politician in response to promises of increased pork are incredible. Finally, Wantchekon (2003) finds that pork matters more than ideology or other public goods for voters with only provincial interests.

The relative salience of pork is closely related to a fourth potential factor—the existence of attractive second-best alternatives. In general, when failing to carry out threatened sanctions involves significant opportunity costs, such threats are increasingly credible. Diaz-Cayeros, Magaloni and Weingast (2003a) demonstrate this intuition in analyzing the support of the PRI in the various states of Mexico. Those

regions with close economic ties to the United States had a credible exit option from PRI support and the resulting pork.

The previous examples identify a few of the factors that might affect voters' memory or ability to credibly threaten sanctions ex post. Each is theoretically plausible and is therefore a matter of empirical analysis. We conduct several such tests in Section 4. We now wish to add county-level heterogeneity in this characteristic into our theoretical model.

Once the election has passed the only reason to make the transfers is the need to retain the credibility of promises regarding such transfers for future elections. Once in office (period 2), the politician has a net present valuation for future office equal to $\Omega_2 < \Omega_1$, and a discount factor δ over the interval until the next election. Reputation is achieved by making transfers as promised subsequent to the election, and the benefit of maintaining a reputation for truthfulness is that pork promises will remain effective in future elections.

We write the probability effect of period 2 transfers on future voter behavior as $h(t_c)$. Voters in a given county 'remember' only a share α_c of the transfer made in period 2, and so $h_c(t_c) = f_c(\alpha t_c)$. This implies that in the absence of any transfers that $h_c(0) = f_c(0)$ (meaning that political preferences are stationary between elections), and that $h' = \alpha_c f'$. In a county with a perfect memory $\alpha_c = 1$ and $h_c(.) = f_c(.)$. Counties with no memories have $h_c(.) = h_c(0) \; \forall \, t_c$.[12] The politician's utility-maximizing transfer in the second period is the largest transfer that is credible in the first period. This is given by:

$$(2) \quad f'(t_c) = \frac{\omega N}{\alpha_c[\delta\Omega_2 + (K - \sum_c t_c)]} \equiv \frac{W_2}{\alpha_c} \quad \geq \quad \frac{\omega N}{\Omega_1 + (K - \sum_c t_c)} \equiv W_1.$$

[12] As indicated previously, we can also think of α_c as representing the ability of political organizers in county c to overcome the collective action problem inherent in threatening to hold the behavior of politicians against them in upcoming elections. Once the next election arrives voters may want to revert to their preference-based voting outcome $h(0)$, but an 'organized' county has $\alpha_c \to 1$, and is thus able to credibly threaten politicians with repercussions of failing to pay pork.

Define t_c^* as the optimal transfer to a county based on the election result in the full-credibility equilibrium given by (1), and the optimal transfers given by (2) as $\hat{t}_c$. This says that $\hat{t}_c \leq t_c^*$ (or $\frac{W_2}{\alpha_c} > W_1$), meaning that the credible transfer without a commitment device is smaller. This is true for several reasons. The first is that discounting pushes up the opportunity costs of diverting funds ($\delta < 1$).[13] Secondly, $\Omega_2 \leq \Omega_1$, since the former is a NPV of future office with one fewer potential terms in office than the latter. These relationships are common to many modeling environments, and show credibility to be decreasing in the presence of impatient politicians with a probability of overthrow or politicians under term limits (in the absence of credible parties) (Keefer 2003; Wantchekon 2003). These two sources of heterogeneity pertain to the politician, and so their influence on the difference between $\hat{t}_c$ and t_c^* will be constant within a given election.

Only α_c creates county-level differentiation between the ex-post optimal transfers with and without a commitment device. Under limited credibility, variation in counties' ability to threaten the politician with repercussions means that even counties receiving non-zero transfers may have different slopes in the voting production function. Those counties with low α receive a lower transfer (because f' has to be driven up by decreasing transfers). This smaller transfer relative to the full-credibility transfer causes the marginal returns to pork to be increasing in the credibility constraint. Because business is responding to the inverted function h^{-1}, this means that *ceteris paribus* $\frac{d\pi_c}{d\varepsilon_c}$, the response of the business outcome to the surprise vote share, will be smaller in counties that make less credible threats to hold politicians accountable in future elections.

This county-level credibility constraint also has an extensive margin, because there may exist counties in which $f'(.)|_{t=0} \geq W_1$ but in which $\alpha_c f'(.)|_{t=0} < W_2$, and so while these counties would have

[13] The politician does not discount over the discontinuous period from before to after the election, but having won the election is now considering the multi-year period between election cycles when deciding whether to make transfers.

received positive transfers under full credibility, they receive none when credibility is limited. Figure 4 shows the differential slope that will be observed across counties depending on the degree to which the politician is credibility constrained in making promises to them. This constraint differs fundamentally from the 'yield constraint' concerning the basic efficacy of pork which defines (1), and for sufficiently small α we see $\frac{d\pi_c}{d\varepsilon_c} = 0$ under limited credibility because no promises are credible.

This theoretical model generates several quite general propositions:

P1. A finding that $\frac{d\pi_c}{d\varepsilon_c} = 0 \ \forall \ c$ indicates that no pork transfers are credible.

P2. A finding that $\frac{d\pi_c}{d\varepsilon_c} = k > 0 \ \forall \ c$ indicates that the politician has a commitment device, making county-level characteristics unimportant to the slope in equilibrium.

P3. If $\frac{d\pi_c}{d\varepsilon_c} > 0$ in some counties and varies across *c*, then the politician has limited credibility. As a result, characteristics which are associated with a steeper slope of the response describe the attributes that make an electoral unit credible.

We now proceed to test these propositions with the Ugandan data.

3. Data.

There are four distinct sources of data brought together in this paper. The first is data from the client records of FINCA/Uganda, a major microfinance agency. From these records, we have the following data: loans are for a 16-week term, to be repaid weekly at an 87% effective annualized rate, and fluctuations in borrowing can be seen as clients' subjective short-term forecasts of the business environment. We see savings in FINCA; these are physically held at Standard Chartered Bank in group accounts (all FINCA lending is done in groups of 30) and are *not* demand deposits. As these savings are

seized by FINCA in the event of failure to repay and are usually only available at the end of the borrowing cycle, they are one of the less liquid forms of saving available to these clients.

Our second source of data is county-level election outcomes, including votes cast for each of the candidates and the total vote. We use the percentage of the vote in each county cast for the incumbent Movement candidacy of Yoweri Museveni and the total percentage cast for the opposition candidates combined (Kizza Besigye, along with Moody Awori, Chapaa Karuhanga, Kibirige Mayanja, and Francis Bwengye). We have accounting data for FINCA clients located in 71 out of Uganda's 214 counties.

Third, we have a newspaper survey which covers the months of November 2000 through July 2001. Uganda has two major newspapers—the New Vision, owned by the Movement government, and the Monitor, owned by the Aga Khan's Nation Media Group. Only events which could be located at a specific place and time were included in order to focus the identification. Summary statistics of the events taken from the newspaper survey can be found in Tables 1 & 2, and a graphical summary of the data on pork promises and threats is found in Figure 5. We see sharply divergent patterns for pork promises made by Museveni and the opposition politicians; while the Movement made such promises throughout the political spectrum, the opposition made such promises only to their own base.

The final source of data is a pair of opinion polls released by the New Vision on February 3rd and February 28th 2001. These polls report outcomes only at the district level (districts are comprised of several counties), and results exist for only 19 of Uganda's 56 districts. FINCA groups exist in 22 districts, of which only 11 overlap with those covered by the opinion polls. From these we have constructed a county-level prediction of voting based on that in found in the district as a whole, or that in the closest district for which data exist. While this measure is very imperfect, it also represents the best information available to voters in the run-up to the election as to likely voting outcomes; no other polls existed, and it is not unreasonable to think that voters were using polls to form expectations in a similar manner.

3.1. Changes in the Outcomes.

We use two outcomes, each observed at the individual level for 21,050 small business-women during the months around the election (April 2000-July 2001). The first is the scale of business-lending; since all loans are on a four-month cycle this gives us a forward-looking measure of business expectations. As expectations of business opportunities expand, we expect to see business lending increase, giving us a metric of the investment opportunities as perceived by the businesswomen of FINCA for the four months following the date of the loan. Our second outcome is the savings which clients choose to leave with FINCA; these are partially collateralized savings which are not easily withdrawn during the four-month cycle and thus represent the businessperson's willingness to take liquidity 'out of the mattress' and place it beyond their reach for a four-month period. Loan quantities can be linked to savings; we often see these two move together but our results show that in circumstances where citizens are directly threatened by politicians credit demand increases and savings falls, meaning that households are trying to provide themselves with the maximum short-term liquidity. In general, then, the loan volume is a metric of business expectations and the savings volume gives a measure of perceptions of short-term financial security. A shock that is positive both in terms of business expectations and short-term security will see both credit demand and willingness to save increase.

The FINCA data is a rolling panel in which each individual features only ever four months (or more) as they 'recapitalize' their loans. For this reason, it is difficult to interpret discontinuous changes between time periods because they consist of different individuals. The temporal sorting process is non-random, a fact which is easily verified by picking 'placebo' discontinuity dates, of which nearly 50% are significant. In order to minimize differences between cohorts we difference outcomes. The dependent variable used throughout the study is $\ddot{\pi}_{ct} = \pi_{it} - \overline{\pi}_i$; individuals for whom only one observation are available were dropped from the study. We thus analyze how explanatory variables relate to deviations from individual mean outcomes for each individual controlling for the strongly increasing and concave pattern that both loans and savings display.

4. Empirics.

4.1 The mean response to the surprise vote share.

Because our dependent variables are defined in differences from individual means, the random effects estimator is identical to pooled OLS. The use of fixed effects in this context would only demean the RHS, and hence the FE estimate is very similar to POLS in all specifications. We therefore use pooled OLS with standard errors clustered at the county level to estimate

$$\ddot{\pi}_{igct} = \beta_0 + \delta_t + \beta_1 \chi_{igct} + \sum_{s=1}^{8} \beta_s S_{sct} + \gamma \varepsilon_{ct} + u_{igct}$$

for the differenced outcome $\ddot{\pi}_{igct}$ for individual *i* in group *g* and county *c* at time *t*. χ_{igct} is a vector of controls (loan cycle number and cycle squared) that vary at the individual level over time, and S_{ct} is a set of eight dummies equal to one in the month after new political events are revealed to have occurred in a county. Given the time dummies, these indicator variables measure how outcomes in counties in the month after each kind of shock has occurred differ from the average differenced outcomes in that month. ε_{ct} equals the surprise vote share in a county in the month after the election and zero all other times, so γ measures the extent to which county-specific shocks to economic outcomes in the month after the election are correlated with the surprise vote share.

The additional eight covariates, taken from the newspaper survey, measure the following:

1. *Acts of violence* (any attacks, beatings, shootings, etc. which are politically motivated).
2. *Threats against the opposition*, which includes the following categories from the newspaper survey: Government threats to opposition candidates, Government Arrests opposing supporters, Government threats to opposing officials, Government supporters threaten opposition, and Physical attack on opposition officials/supporters.
3. *Threats against the Movement*, includes Opponent supporters threaten government officials, Citizens threaten government, and Physical attack on government official/supporters.
4. *Threats against citizens*, includes Opponent supporters threaten citizens, Government/police attack/arrest citizens, Citizens struggle with citizens, and Increased security (included because it was an implied threat, and a response to violence).
5. *Election results contested.*
6. *Election was close*: This is a dummy which switches on in the month after the election that equals one if the poll was between 45% and 55%; this variable captures whether the response to the election is a function of local-level *uncertainty* over the outcome.

7. *Movement promise of pork* made to a county by a Movement politician.
8. *Opposition promise of pork* made to a county by any opposition politician.

Table 3 gives these estimates (non-political variables are suppressed in the table). Both threats and promises have measurable effects on borrower behavior. There is some evidence that threats against the Movement drive up loan sizes, but threats against citizens have a stronger effect; here we see increased borrowing and sharply *decreased* savings as households maximize their short-term liquidity. The response to a pork promise is positive, significant, and resembles the effects of the surprise vote share in significance although the coefficients indicate that the surprise vote share would have to be on the order of 20-40% to rival the quantity effects of a direct promise of pork. Intriguingly, the effects of pork promises by opposition politicians seem to be larger in both magnitude and significance than those made by the Movement. It seems difficult to argue that they would have been more credible, and so it may be instead that they were more unexpected, causing a bigger discontinuous shift. There is strong support here for the idea that changes in business outcomes are related to the surprise vote share.

Figure 6 plots the response to the surprise vote share as we increase the length of the post-election window used to measure the 'shock'. Supporting the theory that this informational shock is both immediate and transitory we see the largest effect using a two-week window and find that by the time the window has expanded to two months the measured effects become insignificant. Since we are seeing different clients show up to take loans over this time interval and that those coming to FINCA between two and four months after the election are taking their first post-election loan, the implication is that they have already come up with other ways of responding to the new information provided by the election and hence do not respond differently.

4.2. Heterogeneity in the response to surprise vote share.

Now we wish to measure the conditional distribution of $\frac{\Delta \pi_c}{\Delta \varepsilon_c}$, which can be accomplished through the use of interactions. For some fixed (and demeaned) county-level characteristics X_c, we can use OLS

to estimate $\ddot{\pi}_{igct} = \beta_0 + \delta_t + \beta_1 \chi_{igct} + \sum_{s=1}^{8} \beta_s S_{sct} + \beta_2 X_c + \gamma \varepsilon_{ct} + \eta(X_c * \varepsilon_{ct}) + u_{igct}$, where η measures the slope of $\frac{d\pi_c}{d\varepsilon_c}$ across X_c. Assuming that the responsiveness of π_c to pork does not differ across counties, differences in the slope of $\frac{d\pi_c}{d\varepsilon_c}$ across X_c give the determinants of α_c.

Table 4 shows that strong differentiation in slopes exists across X_c. The counties that show a strong response to the surprise vote share are educated, sparsely populated, and female. This implies that educated rural counties with a high percentage of females have the values of α_c closest to 1, and so have the punishment capabilities which make pork credible. Conversely, dense, poorly educated areas with a high concentration of males show the smallest response to the surprise vote share which implies that political redistribution conditional on voting behavior is least credible in these areas.

Given the strongly ethnic nature of many African electoral contests, another interesting dimension along which to examine the conditional distribution of $\frac{d\pi_c}{d\varepsilon_c}$ is ethnicity. Using the language spoken in group meetings as the best proxy we have for ethnicity we again find strong differentiation in slopes. Table 5 presents the results of interactions performed in the same way as described above where we dummy out seven language categories, leaving Kiswahili-speakers as the omitted category. Swahili is the language of trade in East Africa but is not widely spoken in Uganda, and so these groups are likely to be ethnically heterogeneous and to be trading over larger distances than other groups.

We find that neither English-speaking groups (which may be the most highly educated) nor the major ethnicities of southern Uganda (the Baganda and Basoga) have slopes which differ from Kiswahili speakers. However, two kinds of ethnicities have sharply divergent responses. On one hand ethnicities from the war-torn north of Uganda, where Museveni has struggled to establish authority, show much lower response to the surprise vote share. On the other hand the four small, non-northern ethnicities show much larger responses. The implication is that Museveni is incredible in the North but that he is

strongly credible among small ethnic groups. The former conclusion makes sense to anyone familiar with the past decade in Uganda and the latter may be evidence for the fact that a small ethnic group can more easily overcome the collective action problem inherent in trying to credibly threaten to hold politicians accountable for current pork distribution in future elections.

We can also use individual-level characteristics to measure interaction effects:

$$\ddot{\pi}_{igct} = \beta_0 + \delta_t + \gamma\varepsilon_{ct} + \beta_1\chi_{igct} + \sum_{s=1}^{8}\beta_s S_{sct} + \beta_2 X_{igc} + \eta(X_{igc} * \varepsilon_{ct}) + u_{igct}\,.$$

These results are given in Table 6. Despite the strong differentiation in effects at the county and ethnicity level, we see very little evidence that individual characteristics matter for the response to the surprise vote share. The only significant characteristic is the response to the survey question (asked several months before the election) 'Do you think that your local economic climate is worsening (-1), unchanged (0), or improving (1)?' This variable is itself measuring a locality effect rather than an individual effect (unless we consider the answer to come from idiosyncratic optimism). Since we have modeled heterogeneity in the response to the surprise vote share as arising from local-level credibility, it is unclear what individual heterogeneity in the response would mean, unless it arose from characteristics which make a given individual more likely to receive pork directly. The fact that we fail to find idiosyncratic differences in slopes is seen as confirmation of the fact that it is indeed heterogeneity at the level of α_c (and not π_{igct} or X_{igc}) that determines the slope of the response function.

Using the machinery developed up to this point, we can also test several of the fundamental hypotheses in the literature on elections.

4.3. Core versus Swing voters.

A major theoretical debate has taken place over whether politicians will target pork to core or swing constituencies. Cox and McCubbins (1986) assume risk-averse incumbents, concluding that they will steer transfers disproportionately to their core supporters in order to maintain coalitional stability. Dixit and Londregan (1996), on the other hand, argue that the incumbent's core supporters will only benefit when

the incumbent has an organizational advantage in directing favors to the core. Otherwise, welfare transfers will be directed to voters whose value for material utility is high relative to their ideological persuasions. By definition these voters are typically swing voters. Lindbeck and Weibull (1987) reach a similar conclusion. Empirical support for these formal models is mixed. Case (2001), Miguel (2003), Levitt and Snyder (1995), Stokes (2005), and Magaloni, Estévez and Diaz-Cayeros (2000) find evidence supporting transfers to the core. The evidence of Dahlberg and Johansson (2002), Kehmani (2003) and Kasara (2005), all support the Dixit-Londregan swing model.

The test suggested here provides a new way of addressing this question. In terms of our model, this is a debate over the responsiveness of voters to pork, and hence relates to $f'(\hat{Ft}_c)$. On both the intensive and extensive margins, we will see less business response if politicians are making fewer credible promises of pork to a given county, and so we can ask whether the slope $\frac{d\pi_c}{d\varepsilon_c}$ differs across core Movement, core opposition, and swing counties. We define dummies that define core Movement and swing counties (Movement vote percentages of >60%, and >40% and <60%, respectively), and interact these dummies with the surprise vote share variable, using the same specification as above.

The last two rows of Table 7.1 shows these interactions. We find that Movement core counties show a significantly larger response in loans to a surprise vote share of a given size than opposition core counties while swing counties do not respond differently. The savings response does not seem to be different across counties, which is reinforced by Table 7.2 which partitions the counties according to their status and runs three separate sets of regressions. In all three groups we see a savings response which is positive but marginally significant with very similar point estimates. The loan response is highly divergent, however, with a strongly significant positive response in Movement core counties and highly insignificant responses in the other groups. The implication is that while the security effects of a positive vote share (measured by savings) are not divergent across core and swing counties, the discontinuous improvement in business opportunities engendered by credible pork is strictly limited to Movement core counties.

4.4. Majoritarian voting.

A clear upshot of the theory on majoritarian systems is that since the total vote share is what matters, wining or losing in specific counties is not important. If this is the case then a surprise vote of a given magnitude should have the same effect whether or not the surprise tipped the county to a surprise majority change. In other words, $\frac{d\pi_c}{d\varepsilon_c}$ should be invariant to whether the surprise crosses 50%. Because this variable is itself an informational shock (unlike swing/core status), we test for it in a fashion similar to Section 4.1. We use the same specification outlined above and add the trichotomous variable m_{ct} which switches on in the month after the election and equals 1 if the surprise makes Museveni the county winner and -1 if an opposition politician takes the county by surprise. We also define separate dummies for the move in each direction.

When one of these variables is used to explain $\Delta\pi_c$ (controlling for the surprise vote share), it tests for whether the magnitude of $\Delta\pi_c$ differs for counties that tip allegiances. If it is interacted with the surprise vote share, the interaction $(m_{ct} * \varepsilon_{ct})$ measures whether the slope of $\frac{d\pi_c}{d\varepsilon_c}$ differs across m_c. Table 8 reports the results of this regression (suppressing shocks already reported above). Using the trichotomous variable (which measures symmetric positive effects in counties that tip to the Movement and negative effects in those that tip away), as well as the dummies for the change in each direction, we find no differential shocks across counties that tip.

The results in Table 8 support the theory—the interaction term is insignificant whether included as a separate shock or in interaction with the surprise vote share. Both the magnitude of the response (conditional on the surprise vote share) and the sensitivity of businesses to surprises are similar in counties that tipped allegiance by surprise and those that did not. In order to make this confirmation of the theory more complete, it would be useful to perform similar tests in countries with electoral colleges to verify whether such surprises do indeed demonstrate differential responses.

5. Robustness Checks.

5.1. Is the election outcome itself a shock?

If the election itself were causing huge swings in the outcomes that we measure, it would be more difficult to argue that the cross-county differences in these shocks were related to nothing but the surprise vote share. We assumed in section 2.2 that $F(.)$, the probability of re-election, is close to one, but if the outcome of the national election itself is a surprise then our identification relies on the assumption that the local business response to the resolution of this uncertainty is orthogonal to the surprise vote share. As way of testing whether the result itself was an economic shock, we generate smoothed pictures of how the de-meaned outcome used in the analysis changed in the weeks around the election. The results are presented in Figures 7 and 8. We see that the week prior to the election had savings and borrowing volumes that were below the mean, but that this difference was just significant and that there were no significant departures from trend on average in the four weeks after the election. Hence we conclude that the outcomes were not responding as if the result of the election itself was a major shock.

5.2. Is the surprise vote share endogenous?

If an unobserved shock drives down the incumbent vote share and drives down economic expectations we will observe a positive correlation as a result of unobserved shocks. The absence of such correlated shocks at the moment of the election is an identifying assumption and so cannot be tested directly, but we can test for whether any correlation exists between the changes in economic outcomes in the month *before* the election and the surprise vote share.

We include a variable which equals the surprise vote share only in the month before the election, and we continue to include the surprise vote share in the month after the election. Conditional on the results of the election, this tests whether correlation exists between unexplained changes in outcomes in the month prior to the election and the surprise vote share that occurred at the end of that month. The

results, presented in Table 9, are not consistent with endogeneity; in all specifications ε_c is not significantly correlated with $\Delta\pi_{ct-1}$ and is significantly correlated with $\Delta\pi_{ct}$. Hence we conclude that there is no evidence of reverse causality, and so we can identify off of ε_c.

5.3. Is the surprise vote share a randomly distributed shock?

If the polling firm made systematic mistakes in sampling, then it would be the case that $Cov(\varepsilon_c, X_c) \neq 0$. Having established that ε_c is exogenous, we can also ask whether it is randomly assigned according the distribution of observables prior to the election. An example of how this could arise from sampling errors by the polling firm would be that the polls under-represent the poor and the poor have anti-incumbent preferences. Counties with high percentages of uncounted poor are slower-growing, and so we would see $\varepsilon_c < 0$, relative to other counties we see $\Delta\pi_c < 0$, and so the correlation between X_c and $\Delta\pi_c$ biases the estimate. This effect only biases the estimate of γ if (a) ε_c is correlated with X_c and (b) X_c is correlated with $\Delta\pi_c$. We can't check (b) separate from the treatment effect, but we can test for (a). If it is indeed a classical error term, the surprise vote will be uncorrelated not only with pre-election covariates, but with pre-election outcomes as well.

To test this, we calculate these averages, and use them to explain the surprise vote share in each county. ε_c is found to be orthogonal to pre-election average outcomes, and so there should be no mean reversion in the estimates. Using a wide variety of specifications on the covariates, we find insignificant relationship with respect to pre-election averages. The exception is that district-level education is strongly and negatively related to the surprise vote share. In other words, educated counties swung more strongly than expected against the Movement. Whether this correlation arises from random variation, is a result of mis-sampled polling, or arises because of real last-minute changes in political preferences, its existence moves us from the realm of experimental to quasi-experimental identification. Because $Cov(\varepsilon_c, X_c) \neq 0$

with respect to education, we must now assume that $Cov(\Delta\pi_c, X_c) = 0$ across the discontinuous change that we use to identify the impact of the surprise vote share.

While we cannot test for this in the month of the election, we can test for it in the months prior to the election. We accomplish this by interacting the month dummies for the three months prior to the election with district-level education. In no case are these interactions significant. Furthermore, their signs flip from positive to negative. Consequently we conclude that changes in the outcomes in the months prior to the election are not related with district-level education. This confirms that $Cov(\Delta\pi_c, X_c) = 0$ and indicates that the correlation found between education and the surprise vote share is not causing bias in our response parameters.

6. Conclusion.

Using a novel measurement technique we find that Ugandan politicians, incumbent and opposition alike, are able to make credible threats and promises. We use a theoretical model to suggest why electoral districts may differ in their ability to hold politicians to promises. We then test for the determinants of credibility at the local level. We find that educated, sparsely populated districts which are predominantly female have the most credibility. Small ethnic groups similarly are more credible. Ethnicities from the North of Uganda are uniquely unable to strike credible deals with Museveni. Unlike group attributes, individual-level characteristics have very little impact over this two-sided credibility game. We find evidence that pork is targeted at the core rather than at swing voters and we confirm that in a majoritarian electoral system politicians pay no attention to winning specific districts, focusing instead on maximizing the total number of votes.

The 2001 Ugandan presidential contest featured an incumbent who, according to the constitution of his country, was facing his last election. Despite the country's 'no party' Movement system, Museveni had been the only president since the end of the country's long civil war; the party arguably had little credibility

apart from its leader. Standard game theoretic models would predict promises made by such a candidate to be largely incredible. Using the correlation between business response and the local surprise vote share we find the reverse to be true. Using different metrics we find that a positive surprise in the local vote for Museveni of between 15 and 40 percent had the same effect on local business behavior as a direct promise of pork from Museveni.

Several explanations for this surprising level of credibility are possible. The simplest is that Museveni, whether through local social capital networks or hard budgeting, had access to a commitment device. Failing this, the usual explanation would be that the Movement as a party was perceived to have sufficient longevity as to make future transfers credible. A more intriguing possibility exists, however, given that subsequent to the election studied here the constitution was altered to allow Museveni to run for a third term. Our theory places great stress on equilibrium expectations, and if voters (correctly) perceived that Museveni would alter the constitution in this way, they would ignore the document. In other words, tension exists between the credibility of the constitution and credibility of the president in the election studied here.

A primary attraction of the empirical method suggested in this paper is its general applicability. In any election for which polling outcomes and forward-looking business behavior are observable at a sub-national level, we can use this technique to test both for the presence of pork and for its cross-sectional determinants. We need not be able to observe pork or patronage directly; instead we assume that local businesspeople are well informed, and we use their responses to informational innovations to test for the presence of a link between politics and business. The resulting ability to use micro data and sub-national variation provides a new angle from which to test theories of electoral competition.

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Appendix.

Table 1. Frequency of Political Events, by Event (Unit: county/month).

	Event:	Frequency	Percent
0	No Event	963	78.48
1	Government Threats to Opposition Candidates	16	1.3
2	Opponent Supporters Threatens Citizens	2	0.16
3	Opponent Supporters Threaten Government Officials	11	0.9
4	Government Arrests Opposing Supporters	27	2.2
5	Government Threats to Opposing Officials	53	4.32
6	Government / Police Attack / Arrest Citizens	2	0.16
7	Peaceful Demonstrations	4	0.33
8	Citizens Struggle with Citizens	4	0.33
9	Promises of Pork by Government	10	0.81
10	Promises of Pork by Opposition	12	0.98
11	Government Supporters Threaten Opposition	4	0.33
12	Large-scale defections to Movement	2	0.16
13	Citizens Threaten Government	5	0.41
14	Physical Attack on Government Official/supporters	15	1.22
15	Physicial Attack on Opposition Official/supporters	39	3.18
16	Physical Attack by Government Official/supporters	8	0.65
17	Physical Attack by Opposition Official/supporters	1	0.08
18	Increased security	14	1.14
19	Election results contested	27	2.2
20	Government arrests/threatens Government Official/supporters	8	0.65

Table 2. Frequency by Month

month	Frequency	Percent
Nov. 2000	2	0.57
Dec. 2000	22	6.25
Jan. 2001	103	29.26
Feb. 2001	95	26.99
Mar. 2001	116	32.95
Apr. 2001	9	2.56
May. 2001	4	1.14
Jul. 2001	1	0.28
Total	352	100

Table 3

Discontinuous Effects.

OLS, SEs clustered at county level	Loans	Saving
Violence	-1.328	-0.726
	(-0.43)	(-0.70)
Threats against Opposition	0.879	0.828
	(0.41)	(1.06)
Threats against Movement	8.813	1.884
	(1.74)	(1.22)
Threats against Citizens	4.819	-2.301
	(1.68)	(-3.31)
Election Results Contested	1.065	-0.932
	(0.27)	(-1.20)
Vote was Close (outcome >45% & <55%)	1.996	2.445
	(0.93)	(1.79)
Movement Promises Pork	4.548	0.695
	(1.64)	(1.14)
Opposition Promises Pork	7.131	2.849
	(2.51)	(2.61)
Surprise Vote Share	0.099	0.047
	(2.03)	(2.23)
nobs:	58,731	58,729

(t-statistics in parentheses)

Table 4.

District-level interactions with Surprise Vote Share

OLS, SE clustered at county level	Loans	Saving
District Area	0.001	0.002
	(0.58)	(1.62)
Average distance from Trading center	0.374	0.144
	(2.58)	(2.52)
% with Secondary Education	2.958	1.465
	(2.08)	(2.29)
Population	-0.043	-0.018
	(-2.38)	(-1.87)
Sex ratio (M/F)	-0.038	-0.024
	(-1.92)	(-2.66)
nobs:	53,875	53,873

(t-statistics in parentheses)

Table 5.

Language Group interactions with Surprise Vote Share

	OLS, clustered SEs		# in group:
Omitted group: Kiswahili-speakers	Loans	Saving	2098
Small groups (Kakua, Lugbara, Lulamogi, Kinubi)	1.065	0.260	83, 729, 445, 30
	(4.81)	(2.49)	
English	0.169	0.074	595
	(0.80)	(0.75)	
Luganda	0.317	0.068	25089
	(1.42)	(0.58)	
Lunyoro	0.014	-0.021	1536
	(0.05)	(-0.29)	
Northern (Lango, Luo/Acholi)	-0.725	-0.290	343, 1486
	(-2.52)	(-1.89)	
Lusoga	0.236	-0.008	11971
	(1.03)	(-0.07)	
nobs:	37,635	37,635	

(t-statistics in parentheses)

Table 6.

Individual & Group-level interactions with Surprise Vote Share

	OLS, SE clustered at county level	Loans	Saving
Individual characteristics	Education	0.053	0.021
		(1.10)	(1.30)
	Date of Birth	0.000	0.000
		(1.55)	(0.53)
	Household Size	0.021	0.004
		(0.90)	(0.52)
	Per capita household expenditures	0.165	0.072
		(1.11)	(1.04)
	Assessment of Economic climate	0.216	0.063
		(2.79)	(2.09)
Group characteristics	Members in group	0.017	0.004
		(1.92)	(1.20)
	Group is urban	(0.02)	(0.02)
		(0.57)	(0.90)
	Ethnic homogeneity of group	(0.03)	(0.05)
		(0.24)	(1.46)
	Group runs internal ROSCA	0.184	0.656
		(1.52)	(1.22)
	Group pre-existed FINCA	0.030	0.038
		(0.27)	(0.65)
	nobs:	9,408	9,408

(t-statistics in parentheses)

Table 7.1.

Response in Core versus Swing counties.

OLS	Loans	Saving
Surprise Vote Share	-0.328	0.013
	(-1.51)	(0.19)
Movement Core county	1.885	0.075
	(1.85)	(0.18)
Swing county	2.000	0.407
	(1.98)	(1.00)
Movement Core * Surprise Vote	0.492	0.037
	(2.16)	(0.51)
Swing * Surprise Vote	0.252	0.041
	(1.09)	(0.52)
nobs:	58,731	58,729

(t-statistics in parentheses)

Table 7.2.

Partitioned Data.

	Movement Core		Swing		Opposition Core	
OLS	Loans	Saving	Loans	Saving	Loans	Saving
Surprise Vote Share	0.188	0.040	-0.031	0.062	-0.138	0.061
	(3.01)	(1.70)	(-0.44)	(1.62)	(-0.52)	(1.01)
nobs:	30,098	30,096	24,790	24,790	3,843	3,843

(t-statistics in parentheses)

Table 8.

Response in counties that tip allegiance by surprise.

OLS, no interaction	Loans	Saving
Surprise Vote Share	0.110	0.028
	(1.33)	(0.74)
Vote crosses 50% (-1,0,1)	-0.537	0.936
	(-0.14)	(0.51)

OLS, with interaction	Loans	Saving
Surprise Vote Share	0.030	0.052
	(0.35)	(1.09)
Vote crosses 50% (-1,0,1)	0.637	0.591
	(0.18)	(0.31)
Vote crosses 50% * Surprise Vote	0.097	-0.029
	(1.43)	(-0.92)
nobs:	58,731	58,729

(t-statistics in parentheses)

Table 9.

Robustness Check

OLS, SEs clustered at county level	Loans	Saving
Violence	-1.238	-0.699
	(-0.41)	(-0.69)
Threats against Opposition	1.186	0.919
	(0.52)	(1.14)
Threats against Movement	9.404	2.059
	(1.73)	(1.30)
Threats against Citizens	5.902	-1.98
	(1.71)	(-2.44)
Election Results Contested	1.97	-0.665
	(0.45)	(-0.78)
Vote was Close (outcome >45% & <55%)	2.241	2.517
	(1.01)	(1.83)
Movement Promises Pork	4.775	0.762
	(1.70)	(1.24)
Opposition Promises Pork	7.384	2.924
	(2.54)	(2.66)
Surprise Vote Share, month before election	0.069	0.02
	(0.94)	(1.08)
Surprise Vote Share, month after election	0.102	0.048
	(2.05)	(2.27)
nobs:	58,731	58,729

(t-statistics in parentheses)

Figure 1.

The 'voting production function':

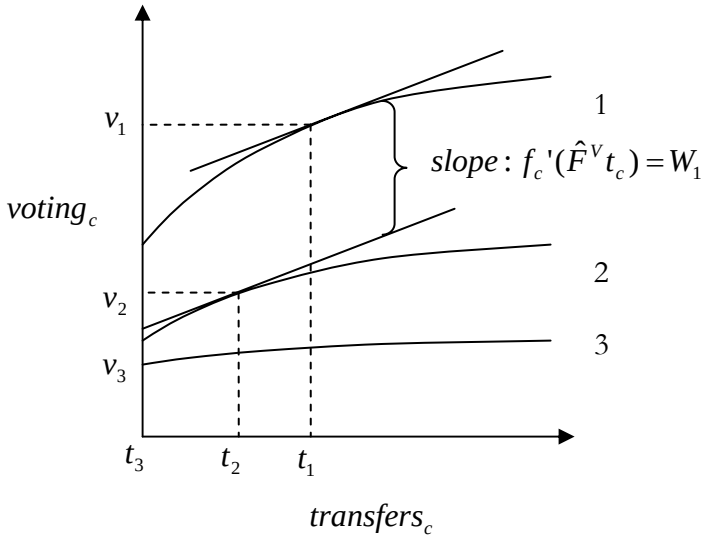


Figure 2.

The inverse function, as read by local businesses:

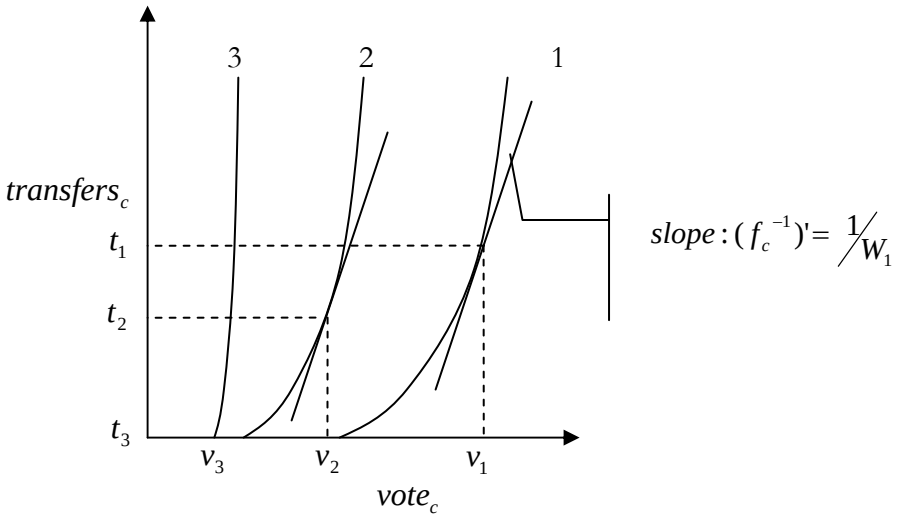


Figure 3. Business response to the surprise vote share with a commitment device:

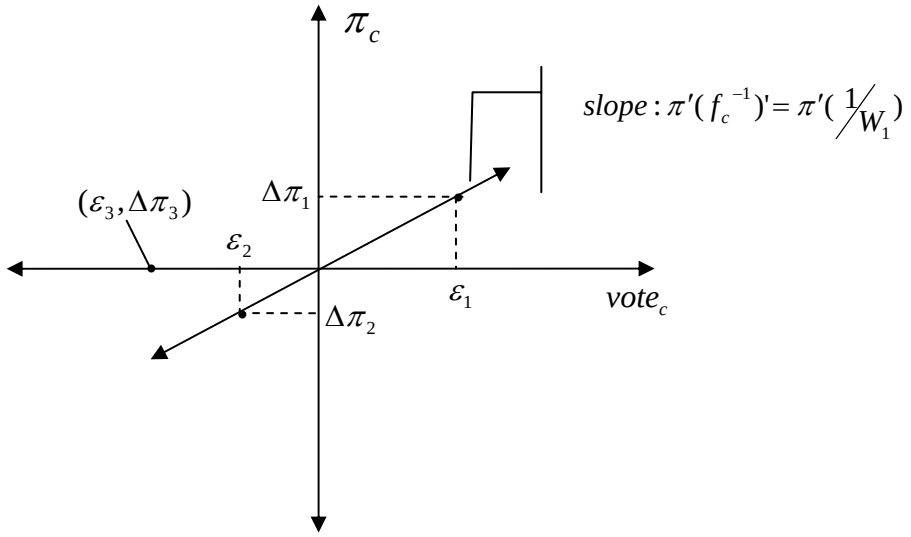


Figure 4. County-level heterogeneity under limited credibility:

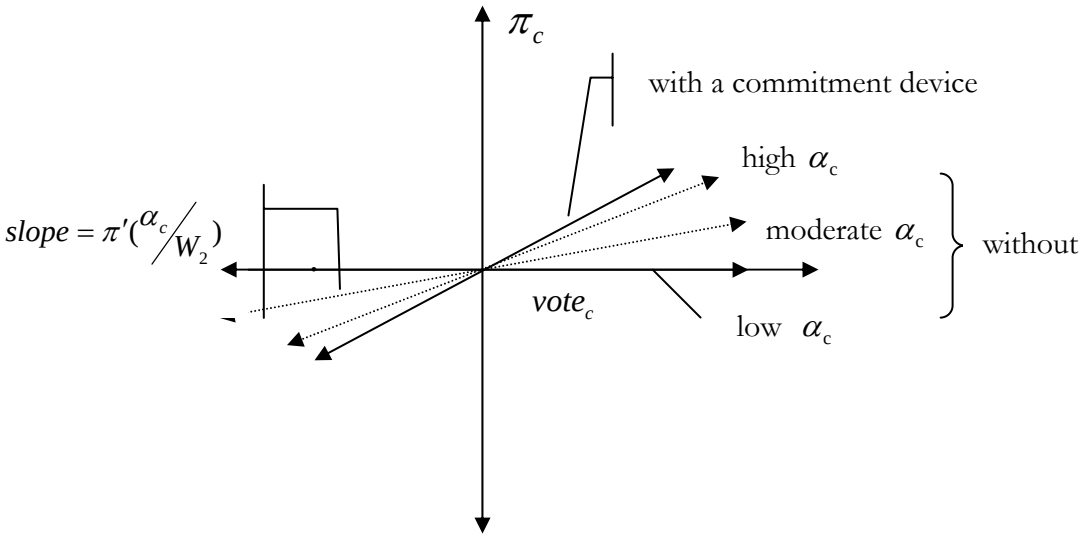


Figure 5. Pre-election events.

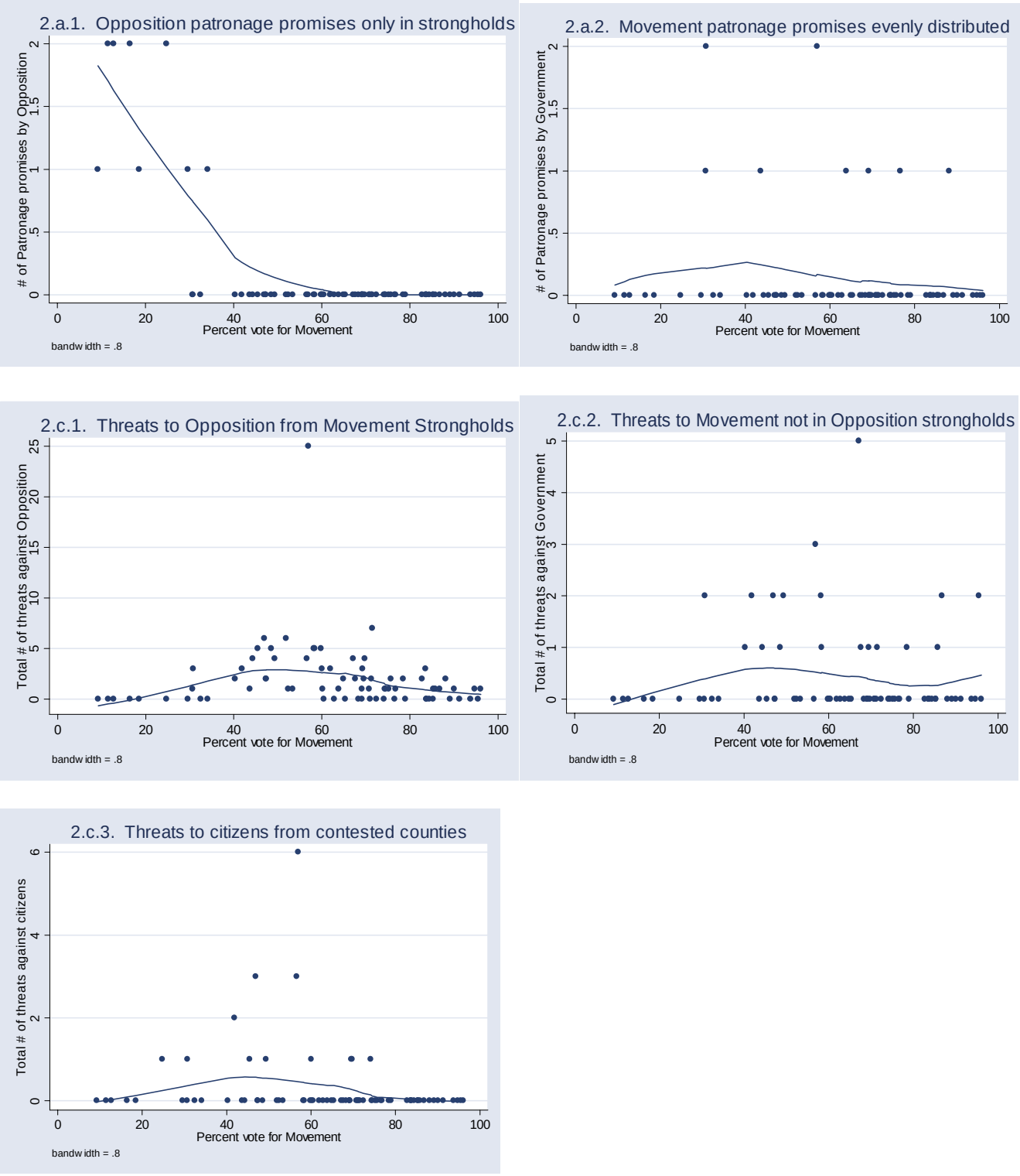


Figure 6. Effects over time.

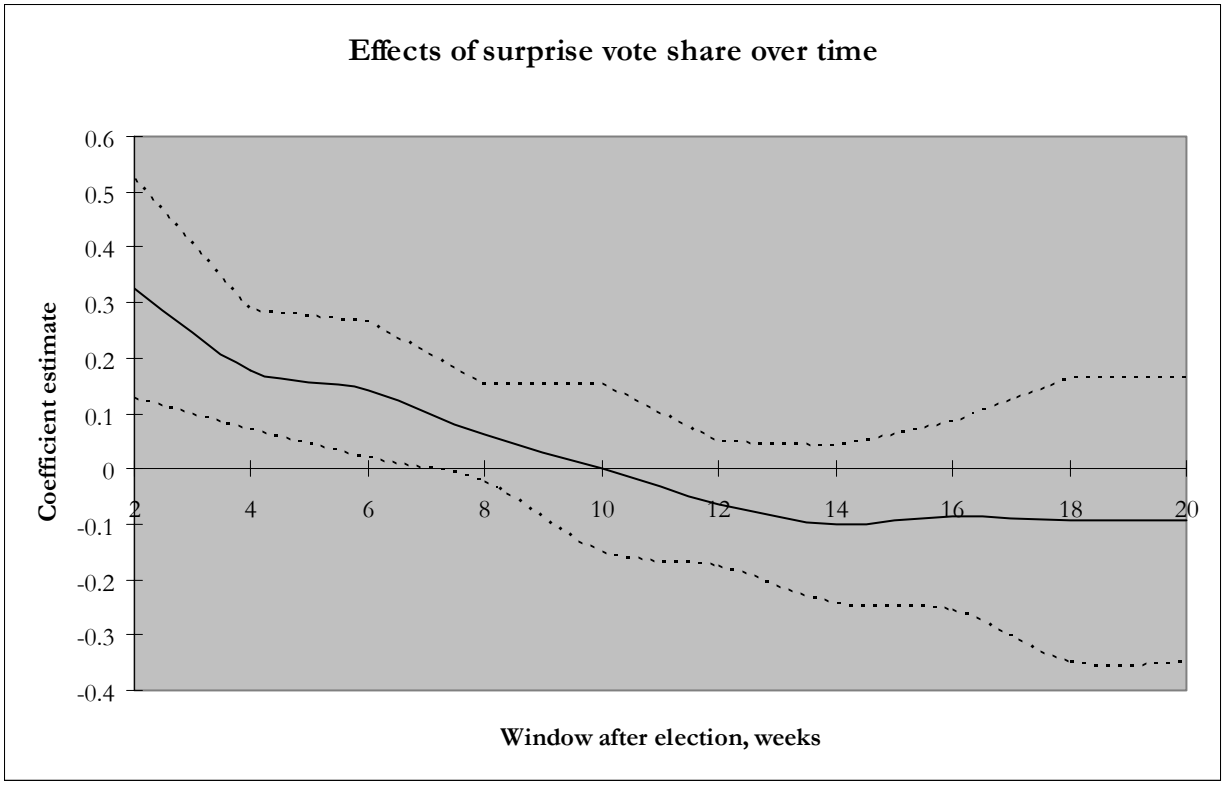


Figure 7.

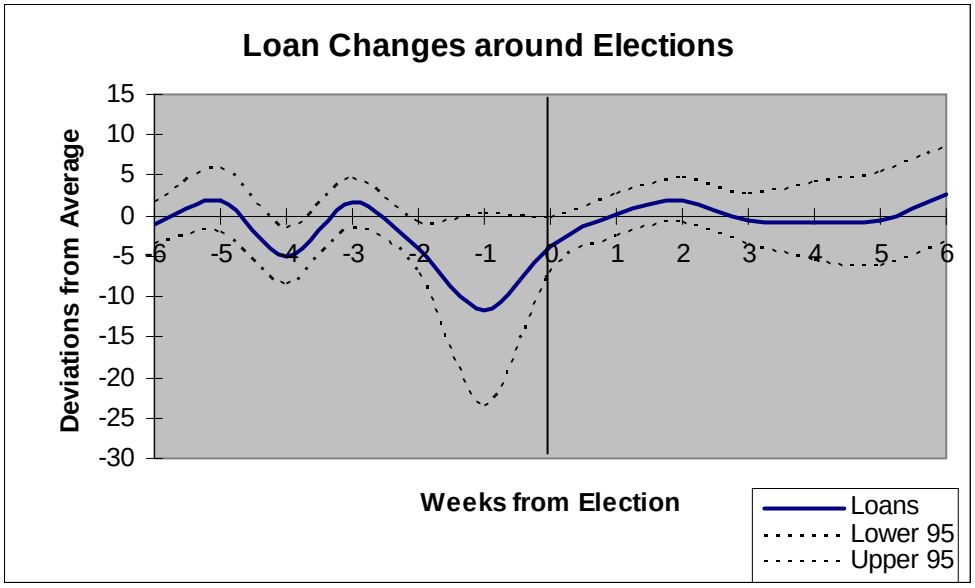
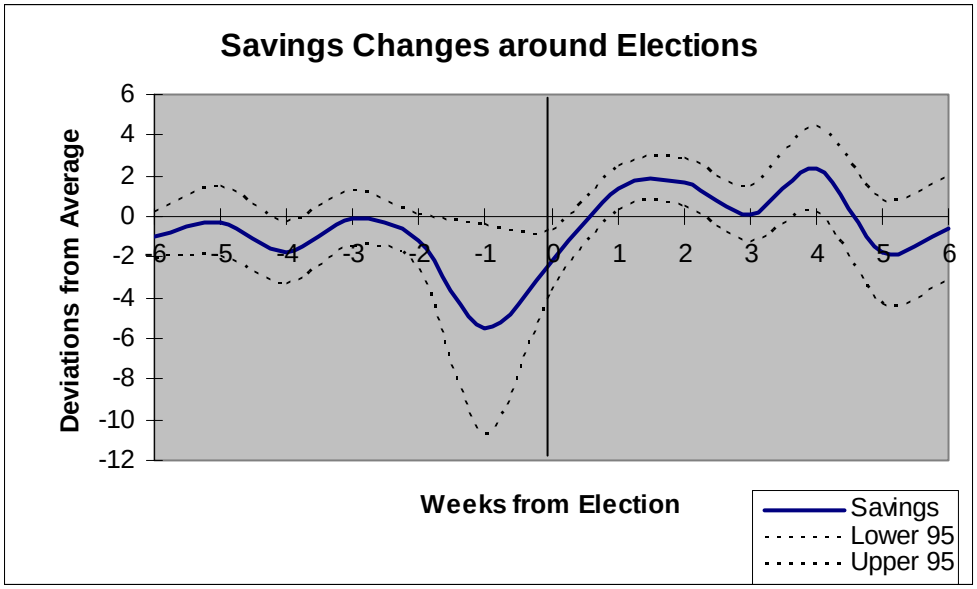


Figure 8.



A3. Truth-telling and the meaning of polls.

The incentives that exist for the polled to answer honestly can be seen as a two-sided truth-telling game. Voters who have no strategic region to answer a particular way lie by answering randomly, and so ε_c is a random variable:

		Politician	
		Lie	**Truth**
Voters	**Lie**	(A) $Cov(\Delta\pi_c, \varepsilon_c)=0$ for two reasons; poll is random and locals do not respond to local surprise vote share.	(B) Locals lie in polls in order to receive pork. Strategy fails because actual vote is observable by the time patronage will be paid.
	Truth	(C) Polls are meaningful but politicians can't credibly promise, and so there is no respose to the surprise local vote share.	(D) Polls and local surprise are meaningful; if patronage has any effect on local behavior, a discontinous response to the surprise vote share will be observed.

For the moment we assume that that voters can perfectly predict whether the politician is lying, and so never respond to incredible promises, although the more formal model below allows these expectations to be probabilistic. In keeping with a large theoretical literature (CITE), we assume that identity politics causes voters to derive some direct utility from self-identifying in polls.

Politician lies: If the politician is lying, then no pork can be credible, and hence whether or not the poll is accurate the locals receive the same transfers. There is thus no indirect benefit caused by polls, the only difference between A and C for a voter is the utility of self-identification, and so polls will be accurate but because pork is not forthcoming, there will be no response to the surprise vote share.

Politician tells truth: A truth-telling politician will deliver the pork *ex post* that was promised *ex ante*. Politicians gain valuable information from being able to target pork using polls, and so would rather face an electorate that tells the truth than one that is lying. Hence the politician will never actually pay pork in period two based on the poll results, because this gives the electorate an incentive to lie in polls, saying that they will vote for the incumbent in order to receive pork, and then not in fact doing so. To avoid this, the politician has easy recourse to the fact that since both polls and the vote are observed by the time pork is

paid, the actual vote tally is used to apportion payments rather than the poll. This means that lying in the poll achieves nothing for voters when the politician is telling the truth, and so identity politics again causes the voters to tell the truth. Since Cell (C) yields higher utility for voters than (A), and (D) than (B), truth-telling is a dominant strategy for voters. The politician will be believed in pork promises when they are credible and not otherwise, and so $\Delta\pi_c$ will only respond to ε_c in environments where the politician is believed.