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ABSTRACT

During the contest for Kansas attorney general in 2006, an organization sent out 6 pieces of mail criticizing the incumbent's conduct in office. We exploit a discontinuity in the rule used to select which households received the mailings to identify the causal effect of mail on vote choice and voter turnout. We find these mailings had both a statistically and politically significant effect on the challenger's vote share. Our estimates suggest that a ten percentage point increase in the amount of mail sent to a precinct increased the challenger's vote share by approximately three percentage points. Furthermore, our results suggest that the mechanism for this increase was persuasion rather than mobilization.

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1. Introduction

Understanding how partisan campaign activity affects vote choice and voter turnout is a central issue in both political science and economics. If campaign activity has an independent effect on election outcomes, then the policy preferences of election-oriented politicians may not perfectly reflect those of the median voter (Baron 1989, Grossman and Helpman 1994). Estimates of the effect of campaign activity are also essential to predicting the impact of reforms to the political process such as restrictions on campaign spending.

Yet, despite an extensive empirical literature devoted to this topic, the magnitude of the impact of campaign activity on voters is still unresolved. For several reasons simple correlations between campaign activity and vote share do not represent a causal effect. Campaign activity and vote share are both outcomes of a complex process that depends on many aspects of candidates and elections that are difficult to measure. For example, if more able candidates attract both more campaign resources and more votes, and candidate ability is not fully observable, then regression estimates of the effect of campaign spending on vote share will inevitably reflect some combination of the true causal effect and unobserved heterogeneity.

In this paper, we use a regression discontinuity (RD) approach to identify the effects on campaign activity on turnout and vote share. Although previous papers have sought to address the endogeneity of campaign activity, none has used RD to do so. During the contest for Kansas attorney general in 2006, an organization sent out 6 pieces of mail criticizing the Republican incumbent's conduct in office. We obtained a complete record of which households received the mailings as well as the algorithm used to select

the households for inclusion in the universe of households that received the mail. We also obtained precinct-level candidate vote totals, the lowest level of aggregation at which candidate choice is observed, and individual level voter turnout records. We exploit our knowledge of the selection rule to isolate a discontinuity in the targeting algorithm which resulted in substantially different amounts of mail in otherwise similar precincts. Our identification strategy compares precinct-level vote shares and individual turnout decisions in similar precincts that received substantially different amounts of mail as result of this discontinuity. We find that the 6 piece mail campaign had no effect on turnout but caused a sizable increase in the vote share of the Democratic challenger.

The remainder of the paper proceeds as follows. Section 2 reviews the previous literature on the topic. Sections 3 and 4 present the study design and data respectively. Section 5 describes our econometric model and presents results. Section 6 provides additional analysis to flesh out the mechanism behind our results in Section 5. Section 7 concludes.

2. Previous Literature

Jacobson (1985) was the first to use statistical methods to estimate the effect of campaign spending. Using the then-newly-available FEC data on candidate expenditures in US House and Senate races, he finds large positive correlations between challenger spending and challenger vote share, but little relation between incumbent spending and incumbent vote share.

This provocative finding spurred researchers in political science and economics to search for ways to deal with the problem of the endogeneity of campaign activity. One

approach seeks to identify the effect of campaign spending from races in which the same candidates face each other on repeated occasions (Levitt 1994). Although fixed-effects models like this hold constant the characteristics of candidates and areas, they remain vulnerable to bias if spending adjusts to changes in political conditions. Another approach seeks to use instrumental variables that induce variation in spending but are uncorrelated with the characteristics of candidates and elections (Green and Krasno 1988, Gerber 1998, Erikson and Palfrey 2000). Complementing these studies on the effects of campaign spending are several recent papers employing natural experiments to measure the effects of mass media communication on voter turnout and candidate choice (e.g. Gentzkow (2006), George and Waldfogel (2007), DellaVingna and Kaplan (2007)). However, each of these studies is vulnerable on the familiar grounds that the exclusion restrictions or modeling details justifying the instrumental variables approach are not valid.

A third approach uses randomized field experiments to assess how voters respond to campaign activity. Most of this work has focused on estimating the effects on turnout (Gerber and Green 2000; Green and Gerber 2008). According to meta-analysis of dozens of studies of each of the alternative methods of voter mobilization, door-to-door canvassing prior to the election often has a large effect on voter turnout, raising turnout by approximately eight percentage points in a typical election, while phone calls and mailings have more modest effects. A live phone call from a commercial firm raises turnout around 0.5 percentage points, a call from a volunteer, two and a half percentage points, and several pieces of campaign mail boosts turnout by approximately one percentage point (Green and Gerber 2008).

Substantially fewer field experiments have been employed to study the effects of campaign activity on vote share. Although randomized experiments are free from the biases inherent in the use of observational data, they have other limitations when applied to the study of voter choice. First, the secret ballot means that individual voters' candidate choices, unlike their turnout, are not a matter of public record. One response to this is to randomize mailings at the household-level and measure the effect on vote share using post election surveys. Gerber (2004) uses this method to study the effect of campaign activity in a collection of local elections from 1999 and 2000 and finds that incumbent spending was ineffective while challenger spending was effective. These and similar studies tend to be small due to the cost of administering post-election surveys and vulnerable to bias due to high rates of survey non-response.[1] An alternative response is to randomize campaign activity at the precinct level. Gerber (2004) uses this approach to study the effects of an incumbent's direct mail campaign in a 2002 US House primary and general election, and finds large effects on vote share in the primary and no effects in the general election. While potentially quite promising due to the availability of vote share at the precinct level, the application of this method is limited by the fact that few campaigns, particularly those in competitive electoral environments, are willing to remove a substantial number of precincts from the campaign's communications efforts.

In this paper, we use regression discontinuity (RD) methods to avoid some of these limitations. While we are not the first to apply RD to politics (see, among others, Lee 2001, Petterson-Lidbom 2006, Ferraz and Finan 2008), we are the first to use RD to assess the effect of campaign activity on voter choice. Although field experiments

[1] There are additional concerns that the treatment may affect survey responses, but not vote-choice, or vice-versa.

require fewer identifying assumptions, RD enjoys several potential advantages in this context. First, RD is less obtrusive. Campaigns may be reluctant to alter their campaign plans to produce control groups, while RD merely requires the campaign to keep track of the rules (and cutoffs) used to determine the campaign targets. This suggests that RD might be applied to a larger and potentially more representative sample of campaigns. Second, RD can be applied historically if campaign records can be obtained. Many campaigns are currently being conducted and treatment assignment rules are held secret. If the selection rules for the mailings and other campaign activity are preserved, these campaigns can be a source of valuable information about the effects of campaign activity once there is no longer a need to maintain secrecy. This holds for all contests, including competitive races, where political actors might be especially resistant to setting aside control groups.

3. Study Design

Our study focuses on the effects of a 6 piece direct mail campaign in the highly competitive Kansas state attorney general race in the 2006 midterm election. This election featured a Republican incumbent against a Democratic challenger. The mail, which was sponsored by the advocacy group Kansans for Consumer Privacy Protection, informed constituents about the group's concerns regarding the incumbent's conduct in office, suggesting that the incumbent was violating citizens' privacy by "snooping" around peoples' medical records rather than fighting crime. The mailings, which featured pictures of a variety of dogs to illustrate the snooping theme, are included in the appendix. The mailings were sent every two or three days in the final two weeks before

the election to a selected set of households with at least one registered voter in a subset of Congressional districts. Households received either one set of mailings or no mailings, regardless of how many registered voters resided there. We consider a voter to have received mail if anyone in his or her household received mail.

The vendor sent mail to a voter according to a function of three groups of variables: the background characteristics of each of the registered voters in the household, including party registration, gender, voting history, time since registration, and other demographic characteristics; responses to a phone survey designed to exclude from the mailing registered voters who stated that they were committed to voting for a specific candidate; and the voter's location in a census block in which more than 5.08131 percent of the households had incomes greater than $150,000. We observe the variables in the first and third group, but do not observe responses to the phone survey.

Simple least-squares estimates of the Democratic challenger's vote share on the proportion of registered voters in a precinct receiving mail are likely to be inconsistent. The probability of receiving mail is positively correlated with voters' background characteristics, phone survey responses, and census-block income levels, all of which might also be correlated with voting behavior. However, the pseudo-random experiment created by the discontinuity in the targeting rule allows us to estimate the causal effect of mail by comparing precincts that contain census blocks with incomes just above the income threshold with those precincts that contain census blocks just below the income threshold.

We develop an instrumental variable (IV) specification based on a fuzzy regression discontinuity design (Hahn, Todd, and Vand der Klaauw, 2001) to identify the

causal effect of mail on election outcomes. Because census block demographic discontinuities are common in political targeting formulas, we generate a specification that can be applied generally to problems of this type. Although census block and precinct boundaries are drawn independently, the geographic clustering of mail in certain census blocks also resulted in the clustering of mail in certain precincts. The share of voters in a precinct receiving mail thus varied depending on the weighted average of the census-block incomes for all census blocks contained in the precinct. The fuzzy regression discontinuity design takes advantage of the fact that living in a precinct containing a census block above the income threshold increases the probability of receiving mail; however, unlike with a sharp discontinuity, it does not determine treatment status. We use the fact that the vendor informed us whether they would have sent each voter mail, had the voter lived in a census block that met the income threshold. We refer to this variable as the *mail eligibility* of a voter. The first stage of our IV regression estimates the effect on the share of voters in a precinct receiving mail of an increase in the share of voters residing in census blocks just above the income threshold, holding constant the share of voters in the precinct who are mail eligible. The second stage regression then estimates the effect of this variation in the share of voters receiving mail on the vote share of the Democratic challenger.

The exclusion restriction necessary to ensure the consistency of the IV estimate is that precincts containing census blocks with incomes just above the income threshold are similar to precincts containing census blocks just below the income threshold, but for their probability of receiving mail given the control variables in the second stage. To

enhance the validity of this exclusion restriction, we restrict our analysis to those precincts that contain census blocks in a small window around the income threshold.

Because we are using a quasi-experiment rather than a randomized experiment, we can not be certain that the probability of receiving mail is orthogonal to unobservable determinants of vote share. We therefore develop an estimation framework that attempts to isolate the effect of mail from any pre-treatment differences in those precincts containing census blocks just above versus just below the income threshold. We use a difference-in-difference-in-difference (DDD) specification to allow for the possibility that unobserved determinants of support for the Democratic challenger were somehow correlated with the receipt of mail.

To implement our DDD specification we first take the difference in the vote share of the 2006 Democratic attorney-general candidate in those precincts with voters in census blocks just above versus just below the income threshold. We then compare this difference to the difference in the same precincts in the 2002 attorney-general race. This is designed to capture pre-treatment differences in the support for Democratic attorney-general candidates in precincts containing census blocks just above versus just below the income threshold. We also estimate the same difference-in-difference for a "control" race that was not subject to the income threshold used in the attorney-general race. This control difference-in-difference is design to capture changes in the general tendency to vote Democratic across time in precincts with voters just above and just below the income threshold. We then take the difference in the difference-in-difference in the attorney-general and the "control" race to get our DDD estimate of the effect of mail.

4. Data

We use two sources of data. We use precinct-level election returns from the state attorney general, gubernatorial, and secretary of state races from the 2006 election. We also obtain precinct-level returns from the state attorney general, gubernatorial, and secretary of state races from the 2002 election to control for the pre-treatment political preferences of each precinct's registered voters.

Second, we match to this file to precinct-level summaries of an individual-registrant level voter file obtained directly from the mail vendor. The voter file contains all of the variables used by the vendor to select households to receive mail and the turnout of each household's registered voters in the 2006 election. As discussed above, the vendor sent mail according to the income of the voter's census block and the voter's mail eligibility.

Table 1 reports descriptive statistics for a selected set of these characteristics. The Secretary of State's office reports election results for 2,711 precincts in the targeted Congressional Districts. After eliminating precincts because the precincts used in the vendor's voter file did not match those used by the Secretary of State's office, because precinct boundaries changed over time, or because individual vote history was unavailable, we are left with a final sample of 1731 precincts.[2]

Table 2 illustrates the identification strategy and our basic result. The first row of the table reports vote shares from precincts that had at least 10 percent of voters that were

[2] We exclude 293 precincts, covering 8.1 percent of registrants, because the precincts in the vendor's file did not match those used by the Secretary of State's office. We exclude 383 more precincts, covering 25.8 percent of registrants, because precinct boundaries change between 2002 and 2006. We exclude an additional 109 precincts, covering 2.8 of registrants, because we don't have either 2002 or 2006 vote history. Finally in 265 precincts we need to consolidate two or more precincts into a single observation in order to match between the vendor's file and the Secretary of State's files. These 265 precincts are collapsed down to 70 observations.

mail eligible and from census blocks with 5.08131-8.08131 percent of households earning more than $150,000.[3] In the 77 precincts that met these conditions, 16.8 percent of voters received mail. The second row of the table reports vote shares for precincts that had at least 10 percent of voters that were mail eligible and from census blocks just *below* the income threshold -- that is, from census blocks with 2.08131-5.08131 percent of households earning more than $150,000. In the 337 precincts that met these conditions, 7.5 percent of voters received mail.

Column (1) in Table 2 reports the vote shares in the 2006 attorney general election in precincts just above and just below the income threshold. It shows that in the 77 precincts just above the income threshold, the Democratic candidate received 55.0 percent of the vote compared to 54.5 in 337 precincts just below the income threshold. If we assume that there were no pre-treatment differences in precincts just above and just below the income threshold, we can estimate the percentage point change in the level of the Democratic attorney-general candidate's vote share caused by the receipt of mail, by taking the difference in the vote share received in precincts just above and just below the income threshold. The single-difference estimate of the effect of mail is thus

$D = 55.0 - 54.5 = 0.5$ percentage points (standard error 1.4).

Of course, precincts are not randomly assigned to be just above or just below the income threshold; therefore there may be some pre-treatment differences in the likelihood of choosing the Democratic attorney-general candidate.[4] Column (2) shows the vote

[3] In this section we will refer to these precincts as precincts just above or below the income threshold, even though technically the income threshold is a property of a census block, and not a precinct.

[4] Later in the paper we compare the party registration, mail eligibility, and the gender of registrants for the 414 precincts that are used to construct Table 2. There are important differences in precincts above and below the income threshold; for example, precincts getting more mail had substantially more Republicans and fewer Democrats.

share of Democratic attorney-general candidate in *2002, when no mail was sent*, for precincts above and below the income threshold. We use the 2002 results to calculate a 2002 analog to *D*, which captures the pre-treatment "placebo" effect of mail. We find in 2002 that the Democratic attorney-general candidate received 46.9 percent of the vote in precincts just above the income threshold, which is 1.6 percentage points less than the 48.5 percent of the vote received in precincts just below the income threshold. If we assume that these differences in the support for the Democratic attorney-general candidate in precincts above and below the income threshold are time invariant then *DD*, the difference-in-difference estimate of the causal effect of mail, is:

$$DD = ((55.0 - 54.5) - (46.9 - 48.5)) =$$

2.1 percentage points (standard error 1.3).

The difference-in-difference estimate allows voter preferences for Democratic attorney-general candidates in the precincts just above and just below the income threshold to differ. However, it assumes that any differences in preference for Democratic attorney general candidates in precincts just above and just below the income threshold are constant across time. This assumption may be problematic if precincts just above the income threshold became more or less Democratic in general between 2002 and 2006 than those just below the income threshold. The remainder of Table 2 explores the possibility that precincts above and below the income threshold differed in their general tendency to vote Democratic across time. In columns (3) and (4) we investigate how precincts just above and just below the income threshold differed in their voting patterns in the 2002 and 2006 gubernatorial race. To do this, we calculate the difference-in-difference effect of mail in the *gubernatorial* race, *which was not subject to*

a mailing with an income discontinuity in either 2002 or 2006. Under the assumption that the differential preference for Democratic candidates across time in precincts just above and just below the income threshold is constant across offices, the gubernatorial race provides a counterfactual of the change in Democratic attorney-general vote share in precincts just above and just below the income threshold absent any mail. Contrary to what we would expect if precincts just above the income threshold became more Democratic between 2002 and 2006, we find that the trend in gubernatorial vote shares declined by -0.3 percentage points (standard error 0.9).

The final row of the table presents the DDD effect of mail: the difference in Democratic votes shares in precincts just above versus just below the income threshold, in the 2006 versus the 2002 elections, for the attorney general versus governor. To estimate the difference-in-difference-in-difference effect of mail we subtract the placebo difference-in-difference effect we observe in the governor race from the difference-in-difference effect we observe in the attorney-general race. The DDD estimate of the effect of mail is thus:

$$DDD = ((55.0 - 54.5) - (46.9 - 48.5)) - ((54.6 - 57.2) - (51.0 - 53.4))$$

$$= 2.4 \text{ percentage points (standard error 1.1).}$$

This effect is quite substantial in political terms. Given that the difference in the share of households receiving mail in the two sorts of precincts is 9.3 percentage points (= 0.168 - 0.075), this implies the average treatment-on-the-treated effect of mail was about 25.8 percentage points. This suggests that a moderately intensive down ballot direct-mail effort can persuade enough voters to alter the result of a close election.

Table 3 suggests that the DDD estimate of the effect of mail from Table 2 is not an artifact of voter preferences for the particular "control" office we selected to construct the DDD. This might occur, for example, if the relationship between income and preference for Democratic candidates were stronger in up-ballot races such as governor as compared to down-ballot elections like attorney-general or secretary of state. In both the 2002 and 2006 elections, the vote shares for the Democratic secretary of state were smaller in the precincts just above versus just below the income threshold. Moreover, there was greater relative support for the Democratic secretary of state candidate in "control" precincts than "treatment" precincts in 2006 than 2002. This suggests using vote shares from the election for secretary of state, rather than vote shares from the election for governor, would not change our results. We explore this hypothesis in greater detail in the econometric models that follow.

5. Econometric Model and Results

Model

The results above suggest that receipt of mail may have had large effect on voting behavior in the 2006 Kansas attorney-general race. In this section we develop an econometric model to formalize the assumptions necessary to identify this effect. We model the share of votes received by the Democratic candidate in race r = (attorney general, governor) or (attorney general, secretary of state) and precinct p = (1, …, P) at year t = (2002, 2006), S_{rpt}. Vote share S_{rpt} depends on (race × year)-specific constant terms[5]; the share of voters in the precinct who received mail, m_p; a vector function $g_p(\Omega)$

[5] α_0, β_0, δ_0, and φ_0 are each five-element vectors: a constant term and dummy variables for the home county and home media markets of the Republican and Democratic candidates We include fixed effects for the

of the characteristics Ω of voters and census blocks contained in a precinct[6]; interactions between race, year and m_p and $g_p()$; and an error term ε_{rpt}:

$$\begin{aligned} S_{rpt} = {} & \alpha_0 + \alpha_1 m_p + g_p(\Omega)\alpha_2 + \\ & AG_{rt}(\beta_0 + \beta_1 m_p + g_p(\Omega)\beta_2) + \\ & YEAR2006_t(\delta_0 + \delta_1 m_p + g_p(\Omega)\delta_2) \\ & AG_{rt} * YEAR2006_t(\phi_0 + \phi_1 m_p + g_p(\Omega)\phi_2) + \varepsilon_{rpt} \end{aligned} \tag{1}$$

For each of the N_p voters in a precinct, the matrix Ω contains 5 variables: whether the voter is mail eligible, registered Democratic, registered Republican, female, and the percentage of households in the voter's census block with incomes above $150,000. The vector function $g_p(\Omega)$ returns 14 variables defined at the precinct level: the share of voters who are mail eligible, registered Democratic, registered Republican, and female, X_p; a cubic polynomial in the estimated proportion of residents in a precinct who have incomes above $150,000, I_p, with $I_p = \frac{\sum_{\substack{census \\ blocks\ b}} R_{pb} * I_b}{\sum_b R_{pb}}$, with R_{pb} the number of registered voters in precinct p who live in census block b; interaction terms between X_p and this cubic polynomial; and two additional variables $g1_p$ and $g2_p$. The variables $g1_p$ and $g2_p$ partition the precinct level variable *mail eligible* into sections defined by census-block income to capture nonlinearities in the relationship between census block income and mail eligibility. They measure the share of voters from high- and moderate-income census blocks who are mail eligible (the omitted category is of voters from low-income census blocks who are mail eligible):

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home counties and media markets to account for home-town candidate preferences that may cause differentials within precincts in Democratic vote shares across the two races.

[6] $g_p(.)$ is a function of voter-level variables because the determinants of the Democratic candidate's vote share can not be written as a function of precinct-level summaries of the elements of Ω; the vendor's targeting rule depended on interactions *at the voter level* between mail eligibility and census-block income.

$g1_p$ = share of voters who are from a census block with more than (5.08131 + s) percent of residents with incomes above $150,000 AND are mail eligible

and

$g2_p$ = share of voters who are from a census block with percent of residents with incomes above $150,000 between (5.08131 - s) and (5.08131 + s) AND are mail eligible

where s = 1, 2, or 3.

For the reasons discussed above, simple OLS estimates of (1) are likely to be inconsistent: m_p explicitly depends on I_p and implicitly on ε_{rpt} (because of the unobserved phone survey). We therefore write m_p as a function of a constant term θ_0, a vector function $h_p(.)$, and an error term ω_p:

$$m_p = \theta_0 + h_p(\Omega)\theta_2 + \omega_p .$$

To estimate (1) by instrumental variables, we impose the exclusion restriction that mail-eligible voters from census blocks with just more than the threshold share of households earning above $150,000 have the same propensity to vote for a Democratic candidate as mail-eligible voters from census blocks with just less than the threshold share of these households. We implement this identification strategy with the following specification of $h(.)$:

$$h(\Omega) = [g(\Omega) \mid h1_p] ,$$

where

$h1_p$ = share of voters who are from a census block with percent of residents with incomes above $150,000 between 5.08131 and (5.08131 + s) AND are mail eligible

The vector functions $g(.)$ and $h(.)$ are specified to make this exclusion restriction as weak as possible. By including a cubic polynomial of I_p in $g(.)$, we attempt to capture any possible direct effects of census-block income on voters' propensity to choose

Democratic candidates. Interacting the cubic polynomial of I_p with X_p in *g(.)* allows the effects of the covariates, most importantly mail eligibility, to vary with the income-level of the precinct.

The inclusion of $g1_p$ and $g2_p$ in *g(.)* allows for the possibility that mail eligibility and census-block income interact to affect the probability of voting Democratic in some way that is not captured by the precinct-level interactions between I_p and X_p. By including $g1_p$ and $g2_p$, identification is reduced to the condition that after controlling for X_p, the cubic polynomial of I_p, and the interaction of X_p and the cubic polynomial of I_p, the interactive effect between census-block income and mail eligibility does not change over the interval (5.08181 – s, 5.08181 + s). The exclusion of $h1_p$ from *g(.)* only assumes that mail-eligible voters from census blocks with between 5.08131 and 5.08131 + *s* percent of households earning more than $150,000 have the same propensity to vote Democratic, conditional on observables, as voters from census blocks with between 5.08131 - *s* and 5.08131 percent of high-income households. We vary the size of the window *s* to investigate the sensitivity of our results to the scope of our exclusion restriction.

Results

Table 4 presents IV estimates of the effect of the proportion of registered voters receiving mail on Democratic vote shares for attorney general and the control race in 2002 and 2006. The table reports estimates of four parameters: the effect of mail on the vote share in the control race in 2002, α_1; the differential effect of mail on the attorney general race in 2002, β_1; the differential effect of mail on the control race in 2006, δ_1; and the differential effect of mail on the attorney general race in 2006, φ_1. The regressors

underlying the first three of these parameters are meant to capture heterogeneity in voter preferences correlated with a precinct receiving mail but not due to the causal effect of mail: indeed, there can be no causal effect of the mail on the any of the 2002 elections, because the mail was not sent until four years later. The parameter of interest in this model is φ_1, which is the IV analogue of the DDD estimate from Table 2. Standard errors allowing for arbitrary within-precinct correlation of the error term ε_{rpt} are in parentheses.

Table 4 shows that the IV estimate of the effect of mail is, if anything, *larger* than the DDD effect in Table 2. Column (1) of the Table presents estimates from the IV model that is the closest analogue to Table 2 -- one that uses the governor's race as a control and uses a window of 3 percentage points around the income discontinuity (s = 3). In that specification, a 10 percentage point increase in the share of voters receiving mail leads to a 3.34 percentage point increase in difference in the share of votes for the Democratic candidate for attorney general versus the Democratic candidate for governor, in the 2006 versus the 2002 election (standard error 1.97 percentage points). Substituting the secretary of state race for the gubernatorial race (column (2)) gives a similar estimate of 3.62 percentage points, but does lead to a slight decline in its standard error (standard error 1.58 percentage points).[7]

Columns (3) - (6) of Table 4 present results from models that use a smaller window around the income discontinuity. By using a smaller window, these models impose a weaker exclusion restriction; they reduce the income range of precincts that are assumed to be otherwise conditionally similar in their preference for the Democratic candidate for attorney general in 2006. Shrinking the window used to identify the IV

[7] The standard errors in the Secretary of State's race are likely lower because the same two candidates race in both 2002 and 2006.

effect leads it to decline slightly in magnitude and statistical significance, but does not alter our basic result. The smallest estimated effect of mail that we obtain, based on a model with a window size of $s = 1$ and the secretary of state as the control race (column (6)), still implies that a 10 percentage point increase in the share of voters receiving mail leads to a 2.49 percentage point increase in the differential vote share for candidate of interest (standard error 1.42).

The fifth row of the table presents the *p*-value of the test of the hypothesis that the effect of mail on the *level* of the Democratic vote share for attorney general in 2006 equals zero (i.e, h_0: $\alpha_1 + \beta_1 + \delta_1 + \varphi_1 = 0$). Each of the coefficients β_1, δ_1, φ_1 represent an effect of mail on some difference in vote shares, either over time or across races; the sum of α_1, β_1, δ_1, and φ_1 represent the total effect of mail on the level of the Democratic vote share in the race of interest. Except in models that use the smallest window around the income discontinuity, the effect of mail on the level of vote share is positive and statistically distinguishable from zero.

6. Identifying the Mechanism for the Change in Vote Share

In the previous section, we showed that a direct mail campaign that criticized the Republican incumbent attorney general increased voters' propensity to vote for the Democratic challenger for that office relative to their propensity to vote for the Democratic candidate for governor. There could be two possible reasons for such a causal effect. The first is that receiving the direct mail persuaded individuals who were already going to turnout to switch for whom they voted. The second is that receiving the direct mail persuaded individuals who supported the Democratic attorney general

candidate to turnout to vote. In this section, we investigate whether there is any evidence for the second possibility.

Estimating the effect of mail on turnout is more straightforward than estimating the effect of mail on vote share because we observe at the individual level both whether an individual received mail and whether s/he turned out to vote. Formally, for each registered voter $j = 1, \ldots, N$ in precinct $p = (1, \ldots, P)$ at year $t = (2002, 2006)$ we specify turnout T_{jpt}, as a function of precinct-level fixed effects, λ_0^P and π_0^P; whether the voter received mail in the 2006 elections, m_{jp}; a vector function $g_{jp}(\Omega)$ of the characteristics Ω of voters and census blocks; and an error term ε_{jpt}:

$$T_{jpt} = \lambda_0^P + \lambda_1 m_{jp} + g_{jp}(\Omega)\lambda_2 + YEAR2006_t(\pi_0^P + \pi_1 m_{jp} + g_{jp}(\Omega)\pi_2) + \varepsilon_{jpt} \quad (2)$$

In this model, the vector function $g_{jp}(.)$ is defined analogously to the function $g_p(.)$ in the precinct-level model, except that all elements are returned at the individual or census block level. For the same reasons as in the precinct-level model of vote share, OLS estimates of the effect of mail on turnout are likely to be inconsistent. We therefore write m_{jp} as a function of precinct fixed-effects ρ_0^P, a vector function $h_{jp}(.)$, and an error term ω_{jp}:

$$m_{jp} = \rho_o^p + \theta_0 + h_{jp}(\Omega)\theta_1 + \omega_p$$

Analogously to the case before, to estimate (2) by instrumental variables, we impose the exclusion restriction that mail-eligible voters from census blocks with just more than the threshold share of households earning above $150,000 have the same propensity to vote for a Democratic candidate as mail-eligible voters from census blocks

with just less than the threshold share. We do this by defining the vector function *h(.)* analogously to the precinct-level model.

Table 5 presents IV estimates of the effect of mail on turnout, λ_1 and π_1 from equation (2). As in table 4, standard errors allowing for arbitrary within-precinct correlation of the residuals are in parentheses. In contrast to the estimates in table 4, the effect of persuasive mail on turnout is neither statistically nor politically significant. Using the smallest possible window around the discontinuity (column (3)), the point estimate of the effect of mail on turnout in 2006, over and above the "control" or "placebo" effect of mail in 2002, π_1, is 0.023, which implies that a 10 percent increase in the likelihood of receiving mail would have at most a 0.2 percent effect on turnout. A turnout effect of this magnitude can not explain the finding in the previous section. There, a 10 percent increase in the proportion of registered voters receiving mail led to an increase in vote share of approximately 3 percentage points -- more than ten times as much. Even assuming a turnout effect equal to the upper bound of the 95 percent confidence interval of the estimate from column (3) can explain only a very small portion of the effect of mail on vote share. At least for the experiment we evaluate here, the effect of mail on voting appears to come through persuasion of individuals who were already going to turnout to switch for whom they voted.

7. Conclusion

To explore the importance of campaign activity on political outcomes, we estimate the effect of a series of direct mailings on voter turnout and candidate choice in the 2006 Kansas attorney general race. We find that mailings sent criticizing the

Republican incumbent had both a statistically and politically significant effect on his vote share. Our estimates suggest that a ten percentage point increase in the amount of mail sent to a precinct increased the Democratic challenger's vote share by about three percentage points. Furthermore, we find no evidence that these mailings affected turnout. As a result, we conclude that these mailings persuaded individuals who were already going to turnout to switch for whom they voted.

These effects are quite large. By comparison, in a meta-analysis of field experiments, Green and Gerber (2008) find that several pieces of direct mail increase turnout by about one percentage point. There is only limited evidence on the persuasive effects of direct mail campaigns, but previous studies find much smaller effects than those reported in tables 2 and 4 (Gerber 2004). A number of factors might account for this difference. First, these are estimates and one standard deviation is approximately 40%-50% of the size of the measured effect. Although we reject the hypothesis that the true effect is zero, we can not reject the hypothesis that the true effect is materially lower than the point estimate. Second, the particular race we study is a down-ballot race; it was not the primary race mobilizing voters to the polls. Direct mail likely has a larger potential effect in such environment than in a presidential race where voters are much better informed about the issues. Third, the local-average treatment effect we estimate only applies to so-called "mail eligible" voters -- namely, those who were predetermined by the vendor to be particularly susceptible to persuasion; mail almost surely would be less effective in the population at large.

The magnitude of our estimate might also be evidence of spillover effects, or social interaction among voters. Our treatment effects are estimated at the precinct level

and may reflect changes in households that were not send mail but live in a precinct where the mailings were concentrated. Even if such spillover effects are only half as large as direct effects, each treated household influences one other household through social interactions, and for simplicity the effects of such additional communications are linearly additive and confined to the precinct, then social interactions would account for one-third of the magnitude of the total effect. Designing an experiment or formulating a structural model that could distinguish between direct and spillover effects of voter persuasion is an important area for future research.

Although our study is of a single direct mail campaign, the method we propose for analyzing campaign communications is general and can be applied to other situations where there exist geographic discontinuities in targeting formulas. Because this method can be applied retroactively, it may permit scholars to analyze elections that have already occurred. RD is likely to be acceptable to a broader set of campaigns than randomized experimentation, which requires setting aside randomized control groups. RD analysis may therefore represent a complementary methodology to field experiments in learning how and when campaign activity affects citizens' voting behavior.

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Table 1
Descriptive Statistics, Voting Precincts, 2002 and 2006

Variable	Mean	Std. Dev.
Percentage of voters:		
receiving mail in 2006	0.100	0.074
voting for Democratic Attorney General in 2006	0.533	0.093
voting for Democratic Governor in 2006	0.554	0.099
voting for Democratic Secretary of State in 2006	0.283	0.107
mail eligible	0.271	0.089
registered Democrat	0.256	0.088
registered Republican	0.516	0.124
female	0.534	0.031
voting for Democratic Attorney General in 2002	0.483	0.108
voting for Democratic Governor in 2002	0.532	0.105
voting for Democratic Secretary of State in 2002	0.282	0.101
Number of voters (unweighted)	381.44	485.66

Note: N = 1731 precincts. Observations weighted by total votes in 2006 Attorney General Race.

Table 2
Democratic Candidates' Vote Shares in 2006 and 2002
Precincts Just Above versus Just Below the Income Threshold for Receiving Mail

	Vote Share for Dem AG		Vote Share for Dem Gov	
	2006	2002	2006	2002
	(1)	(2)	(3)	(4)
Precincts Just Above Income Threshold for Receiving Mail in 2006	0.550	0.469	0.546	0.510
Share of voters receiving mail = 0.168 (N = 77)				
Precincts Just Below Income Threshold for Receiving Mail in 2006	0.545	0.485	0.572	0.534
Share of voters receiving mail = 0.075 (N = 337)				
Difference in Vote Shares	0.005	-0.016	-0.027	-0.023
	(0.014)	(0.019)	(0.016)	(0.017)
Difference in Difference in Vote Shares	0.021		-0.003	
	(0.013)		(0.009)	
Difference-in-difference-in difference effect of mail: difference in Democratic vote shares, precincts just above versus just below income threshold for mail, race for attorney general versus race for governor, 2006 versus 2002	**0.024**			
	(0.011)			

Notes: "Precincts just above income threshold for receiving mail" are those that have at least 10 percent of voters mail eligible AND from census blocks with 5.08131-8.08131 percent of households earning more than $150,000. "Precincts just below income threshold for receiving mail" are those that have at least 10 percent of voters mail eligible AND from census blocks with 2.08131-5.08131 percent of households earning more than $150,000.

Table 3: Other characteristics of Precincts Just Above and Just Below Income Threshold for Receiving Mail in 2006

	Precincts Just Above Income Threshold for Receiving Mail	**Precincts Just Below Income Threshold for Receiving Mail**
Percentage of registered voters:		
earning more than $150,000	0.049	0.028
mail eligible	0.252	0.293
registered Democrat	0.238	0.268
registered Republican	0.534	0.491
Female	0.525	0.537
voting for Democratic Sec'y of State in 2002	0.265	0.296
voting for Democratic Sec'y of State in 2006	0.268	0.309
# Precincts	77	337

Notes: "Precincts just above income threshold for receiving mail" are those that have at least 10 percent of voters mail eligible AND from census blocks with 5.08131-8.08131 percent of households earning more than $150,000. "Precincts just below income threshold for receiving mail" are those that have at least 10 percent of voters mail eligible AND from census blocks with 2.08131-5.08131 percent of households earning more than $150,000.

Table 4: IV Effect of Mail On Vote Share

	(1)	(2)	(3)	(4)	(5)	(6)
% registered voters receiving mail in 2006 (α_1)	0.347 (0.221)	0.352 (0.148)	0.214 (0.168)	0.218 (0.103)	0.076 (0.179)	0.185 (0.101)
% registered voters receiving mail in 2006*attorney general (β_1)	0.006 (0.151)	0.001 (0.192)	0.013 (0.115)	0.010 (0.151)	0.069 (0.114)	-0.039 (0.141)
% registered voters receiving mail in 2006* year=2006 (δ_1)	-0.184 (0.174)	-0.213 (0.119)	-0.174 (0.137)	-0.191 (0.091)	-0.325 (0.138)	-0.194 (0.099)
% registered voters receiving mail in 2006* year=2006 * attorney general (φ_1)	**0.334** **(0.197)**	**0.363** **(0.158)**	**0.274** **(0.152)**	**0.291** **(0.119)**	**0.380** **(0.177)**	**0.249** **(0.142)**
p-value testing effect of mail on 2006 level of vote share for attorney general = 0: h_0: $\alpha_1 + \beta_1 + \delta_1 + \varphi_1 = 0$	0.020	0.019	0.040	0.040	0.256	0.256
Control race	Governor	Sec'y state	Governor	Sec'y state	Governor	Sec'y state
Size of window around discontinuity	3 (large)	3 (large)	2 (med)	2 (med)	1 (small)	1 (small)
R-squared	0.490	0.811	0.522	0.824	0.542	0.829

Notes: N = 6924 = 1731 precincts * 2 races * 2 time periods. Observations weighted by total votes in each respective race. Standard errors calculated allowing for clustering at the precinct level.

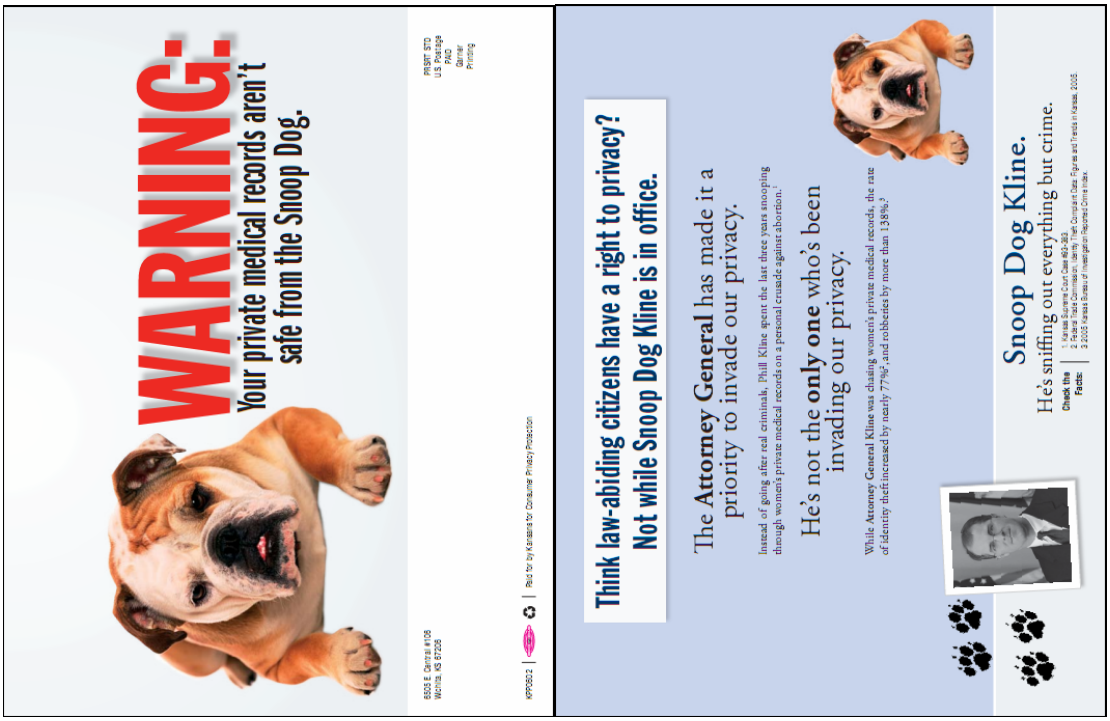
Table 5: IV Effect of Mail On Turnout

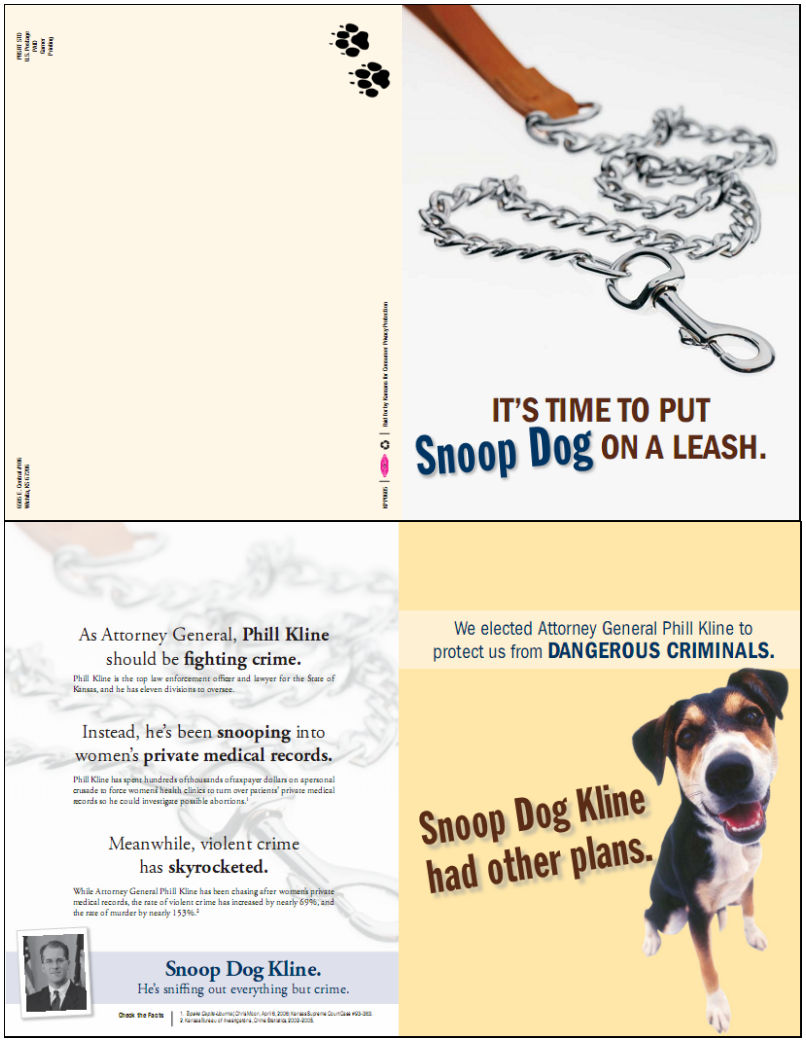
	(1)	(2)	(3)
Received mail in 2006? (1 = yes) (λ_1)	-0.035 (0.026)	-0.022 (0.025)	-0.029 (0.033)
Received mail in 2006 * year=2006 (π_1)	**0.018** **(0.019)**	**0.011** **(0.018)**	**0.023** **(0.022)**
p-value testing effect of mail on 2006 level of turnout = 0 h_0: $\lambda_1 + \pi_1 = 0$	0.497	0.616	0.831
Size of window around discontinuity	3 (large)	2 (medium)	1 (small)
R-squared	0.120	0.120	0.120

Notes: N = 564980 individuals. Standard errors calculated allowing for clustering at the precinct level (1731 precincts)

Appendix

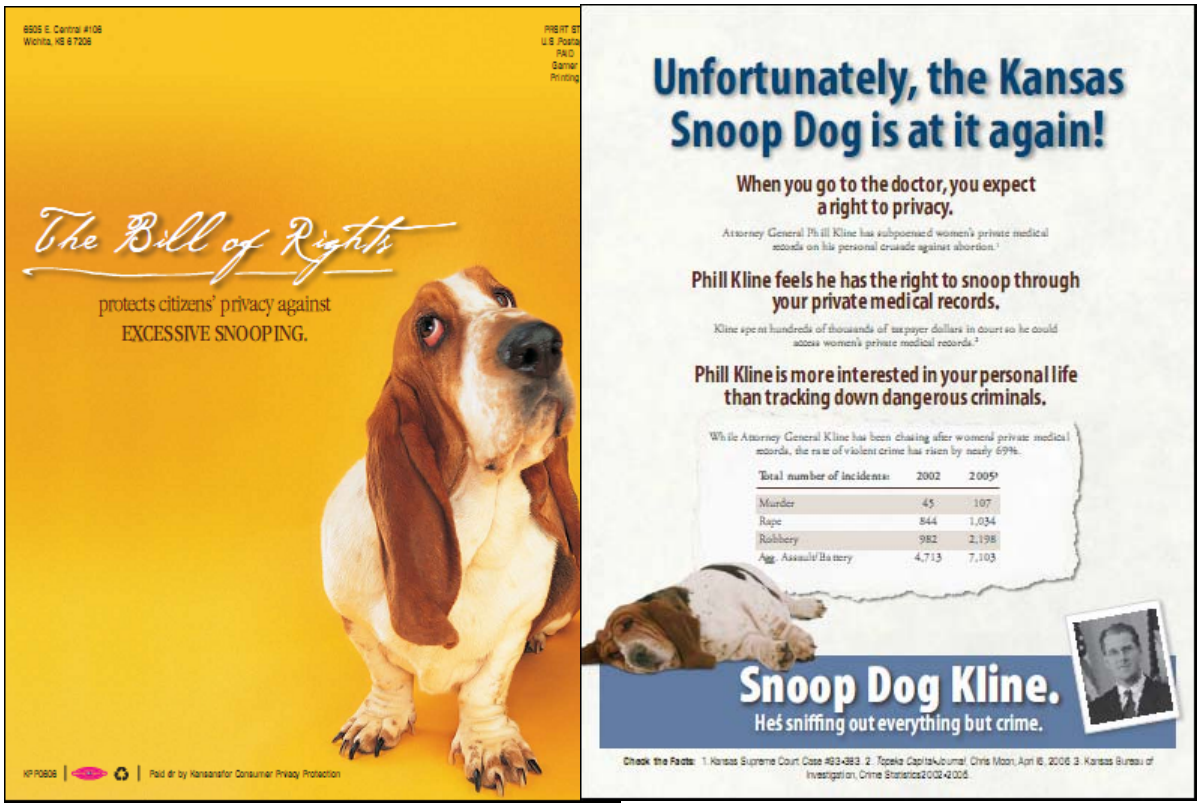
Each picture is the front and back of a postcard mailed by the vendor, displayed in their order of receipt.




IT'S TIME TO PUT
Snoop Dog ON A LEASH.
As Attorney General, Phill Kline should be fighting crime.
Phill Kline is the top law enforcement officer and lawyer for the State of Kansas, and he has eleven divisions to oversee.
Instead, he's been snooping into women's private medical records.
Phill Kline has spent hundreds of thousands of taxpayer dollars on a personal crusade to force women's health clinics to turn over patients' private medical records so he could investigate possible abortions.[1]
Meanwhile, violent crime has skyrocketed.
While Attorney General Phill Kline has been chasing after women's private medical records, the rate of violent crime has increased by nearly 69%, and the rate of murder by nearly 153%.[2]
Snoop Dog Kline.
He's sniffing out everything but crime.
Check the Facts
We elected Attorney General Phill Kline to protect us from DANGEROUS CRIMINALS.
Snoop Dog Kline had other plans.


Phill Kline
is barking up the wrong tree.
Protecting seniors and children is not the Kline priority.
Phill Kline voted against background checks for nursing home workers.
Phill Kline voted against cracking down on deadbeat parents.
Now, Phill Kline is snooping into private medical records instead of fighting real crime.
Snoop Dog Kline.
He's sniffing out everything but crime.
Instead of protecting seniors and children, he'd rather sniff around YOUR private medical records.


6605 E. Central #108
Wichita, KS 67206
PRSRT STD
U.S. Postage
PAID
BM&M
Snoop Dog Kline is on YOUR trail.
KP P0804 | Paid dr by Kansansfor Consumer Privacy Protection
Cracking down on deadbeat parents?
Attorney General Phill Kline is
MORE INTERESTED IN YOU.
Phill Kline voted against a bill that would have helped single parents collect child support.
Kansas Senate Bill 140 would have cracked down on deadbeat parents, but Kline said it was an "invasion of privacy."[1]
As Attorney General, he hasn't cracked down on dangerous criminals.
Meanwhile, the criminals run loose in the State of Kansas, with the rate of violent crime increasing nearly 69% and murder by nearly 153%.[2]
Instead, he's sniffing around your private medical records.
Attorney General Phill Kline spent the better part of three and a half years fighting to snoop through women's private medical records on his personal crusade against abortion.[3]
DOG
Snoop Dog Kline.
He's sniffing out everything but crime.
Check the Facts: 1. Kansas City Star, Michelle Burger, July 5, 1997 2. Kansas Bureau of Investigation Crime Statistics 2002-2005
3. Kansas Supreme Court Case #93-383


6605 E. Central #108
Wichita, KS 67206
The Bill of Rights
protects citizens' privacy against
EXCESSIVE SNOOPING.
KP P0805 | Paid dr by Kansansfor Consumer Privacy Protection
Unfortunately, the Kansas
Snoop Dog is at it again!
When you go to the doctor, you expect a right to privacy.
Attorney General Phill Kline has subpoenaed women's private medical records on his personal crusade against abortion.[1]
Phill Kline feels he has the right to snoop through your private medical records.
Kline spent hundreds of thousands of taxpayer dollars in court so he could access women's private medical records.[2]
Phill Kline is more interested in your personal life than tracking down dangerous criminals.
While Attorney General Kline has been chasing after women's private medical records, the rate of violent crime has risen by nearly 69%.
Total number of incidents: 2002 2005[3]
Murder 45 107
Rape 844 1,034
Robbery 982 2,198
Agg. Assault/Battery 4,713 7,103
Snoop Dog Kline.
He's sniffing out everything but crime.
Check the Facts: 1. Kansas Supreme Court Case #93-383. 2. Topeka Capital-Journal, Chris Moon, April 6, 2006. 3. Kansas Bureau of Investigation, Crime Statistics 2002-2005.