Econ 219B
Psychology and Economics: Applications
(Lecture 12)

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Outline

1. Attention: Introduction
2. Attention: Financial Markets
3. Attention: Voting
4. Media: Data
5. Media: Consumer Behavior

1 Attention: Introduction

- Attention as limited resource:
  - Satisficing choice (Simon, 1955)
  - Heuristics for solving complex problems (Gabaix and Laibson, 2002; Gabaix et al., 2003)

- In a world with a plethora of stimuli, which ones do agents attend to?

- Psychology: Salient stimuli (Fiske and Taylor, 1991)

1.1 Attention to Non-Events

- Remember Huberman and Regev (2001)?

- Timeline:

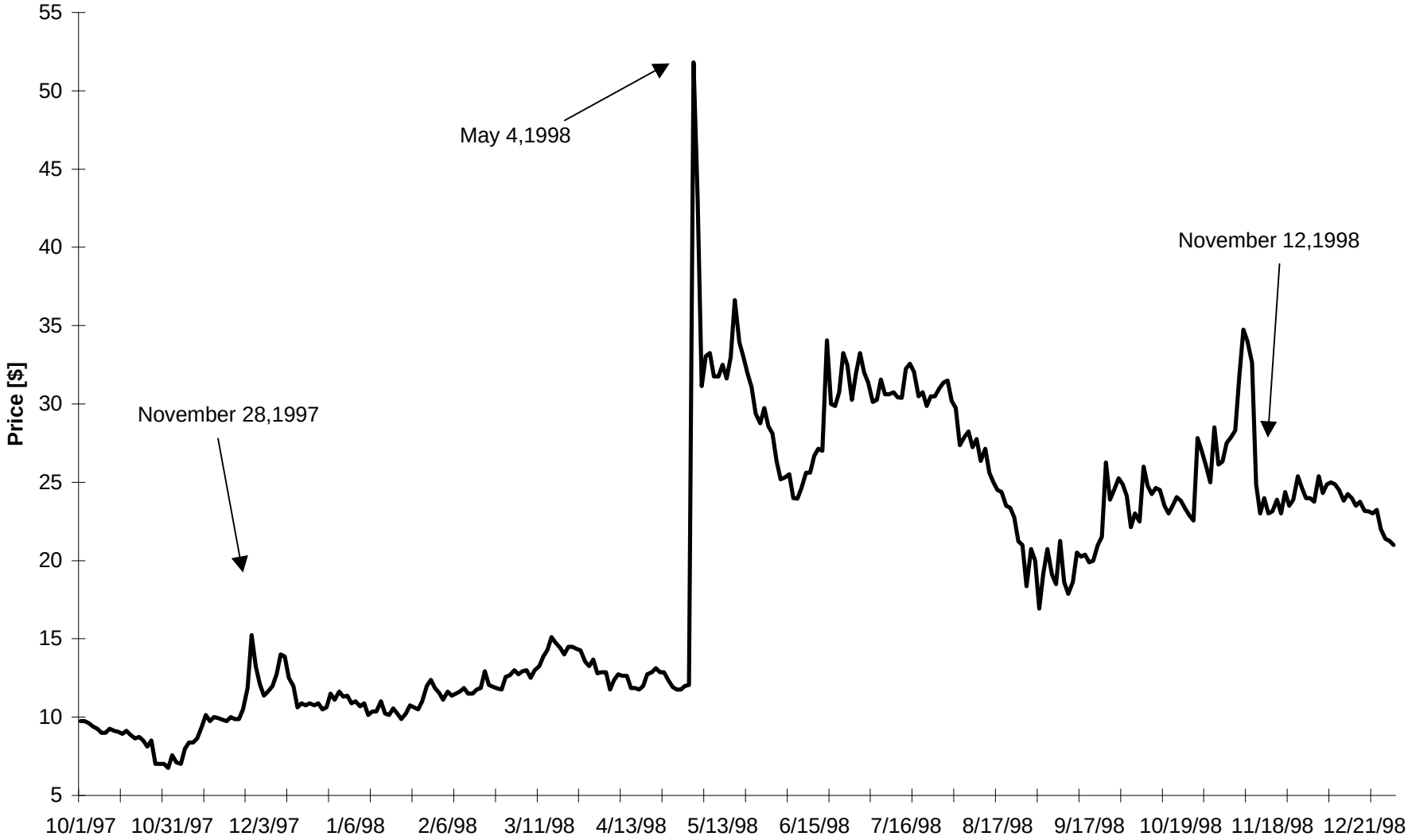
  - October-November 1997: Company EntreMed has very positive early results on a cure for cancer

  - November 28, 1997: *Nature* "prominently features;" *New York Times* reports on page A28

  - May 3, 1998: *New York Times* features essentially same article as on November 28, 1997 on front page

  - November 12, 1998: *Wall Street Journal* front page about failed replication

Figure 5: ENMD Closing Prices and Trading Volume 10/1/97-12/30/98



- In a world with unlimited arbitrage...

- In reality...

1.2 Theory of attention?

- Which theory of attention explains this?

- We do not have a theory of attention!

- However:
  - Attention allocation has large role in volatile markets
  - Media is great, underexplored source of data

- Suggests successful stategy on attention papers:
  - Do not attempt general model
  - Focus on specific deviation

2 Attention: Financial Markets

- Most basic prediction of limited attention:

  1. Pay (too) much attention to salient stimuli

  2. Pay (too) little attention to non salient stimuli

- Financial markets: Good place to test both

  1. Barber and Odean (2004)

  2. Hong, Torous, Valkanov (2002); Pollet (2003)

2.1 The Salient

- Barber and Odean (2004)

- Investor with limited attention
  - Stocks in portfolio: Monitor continuously
  - Other stocks: Monitor extreme deviations (*salience*)

- High-attention stocks:
  - demand increases
  - supply does not

- Heterogeneity:

  - Small investors have attention bias
  - Institutional investors less prone to attention bias

- Market interaction: Small investors are:
  - net buyers of high-attention stocks
  - net sellers of low-attention stocks.

- Measures of attention:
  - same-day volume
  - previous-day return
  - in the news?

- Alternative interpretations of results:

- Small investors own few stocks, face short-selling constraints

- If stock captures attention:
  - buy if positive news
  - do nothing otherwise

- If stock is not in news:
  - small trades
  - more likely to be a transaction on owned stock

- Large investors are not constrained

- Investigate stocks already owned

Figure 1: Simulated buy-sell imbalance.

We simulate 100,000 realizations of the economy in our model assuming the parameter values assumption that ϕ = 2, A = 2, m = 2, ψ = 2, and $\kappa = 0.5$. Realizations are sorted into partitions on the basis of period 1 return and period 2 trading volume. Buy-sell imbalance is calculated as noise trader buys minus sells divided by noise trader buys plus sells.

Figure 1a

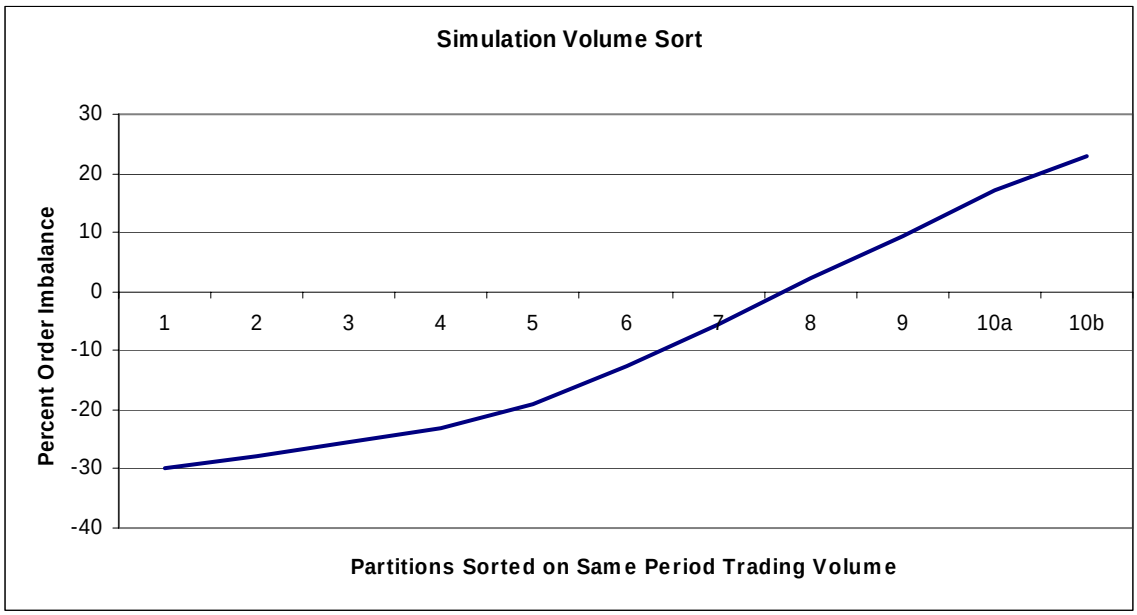


Figure 1b

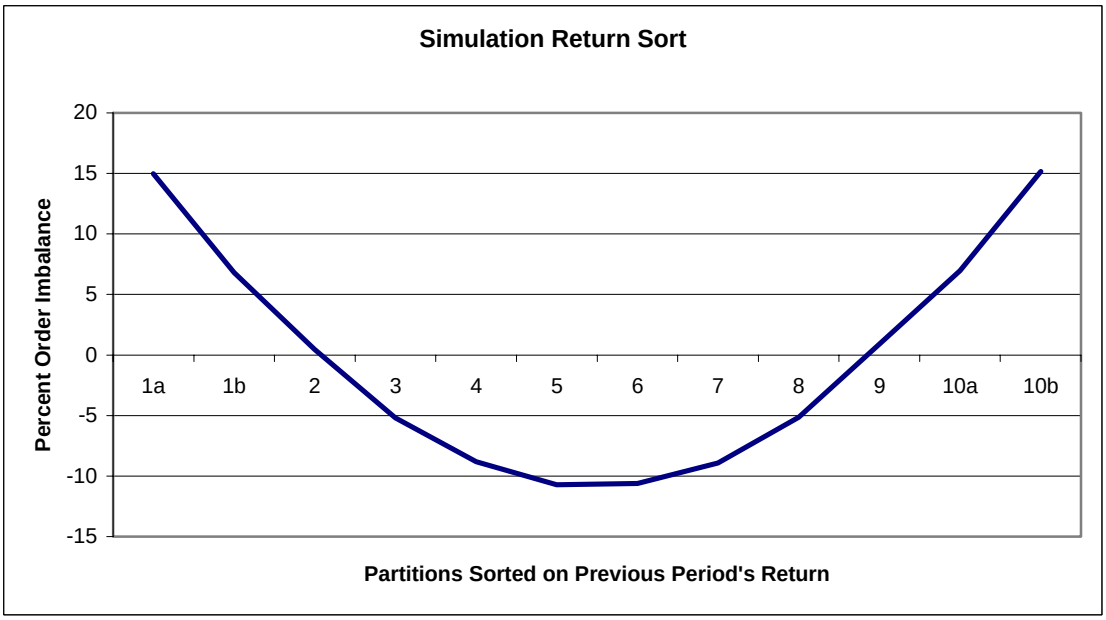


Figure 2: Buy-sell imbalance by Number of Trades for Stocks Sorted on the Current Day's Abnormal Trading Volume

Stocks are sorted daily into deciles on the basis on the current day's abnormal trading, The decile of highest abnormal trading is split into two vingtiles (10a and 10b). Abnormal trading volume is calculated as the ratio of the current day's trading volume (as reported in the CRSP daily stock return files for NYSE, ASE, and NASDAQ stocks) divided by the average trading volume over the previous 252 trading days. Figure 2a graphs buy-sell imbalances for investors at a large discount brokerage (1991-1996), investors at a large retail brokerage (January 1997 through June 1999), and investors at a small discount broker (January 1996 through June 15, 1999). Figure 2b graphs buy-sell imbalance for institutional money managers (January 1993 through March 1996) classified as following momentum, value, and diversified strategies. For each day/partition/investor group, we calculate number imbalance as number of purchases minus number of sales divided by total number of trades. The figure depicts the mean for each time-series of daily imbalances for a particular investor group.

Figure 2a

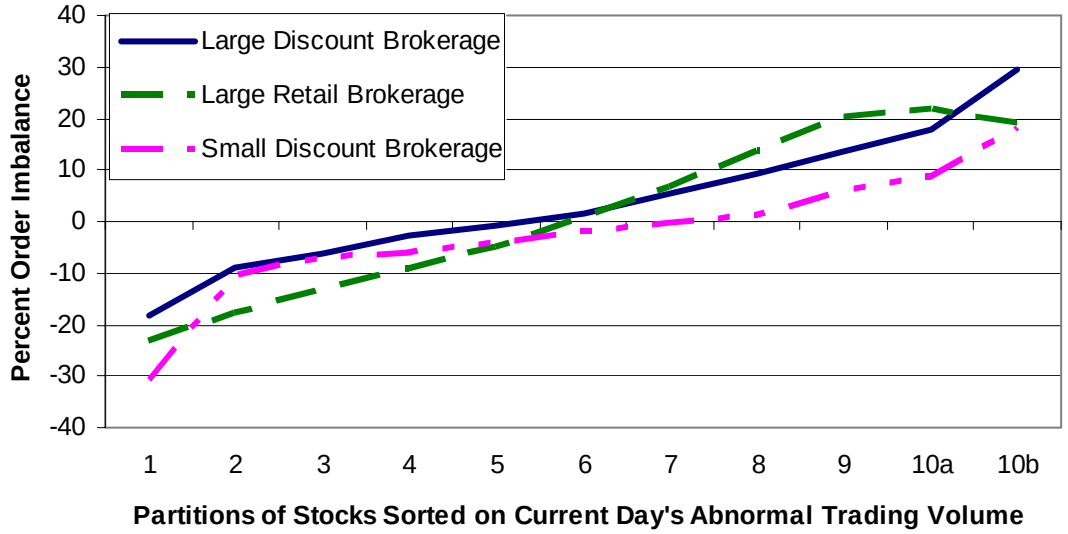


Figure 2b

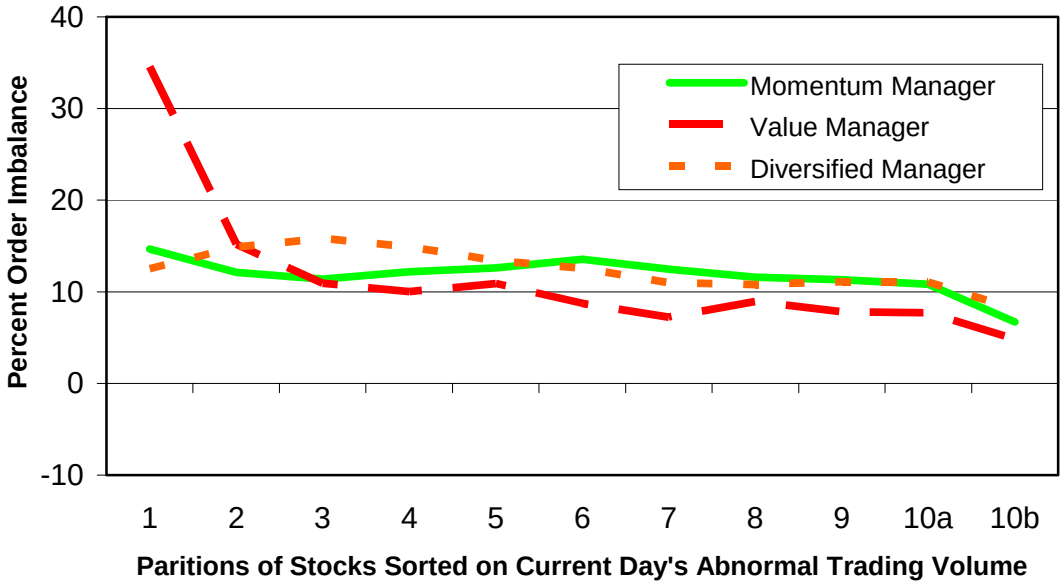


Figure 3: Buy-sell imbalance by Number of Trades for Stocks Sorted on the Previous Day's Return

Stocks are sorted daily into deciles on the basis on the previous day's return as reported in the CRSP daily stock return files for NYSE, ASE, and NASDAQ stocks. The deciles of highest and lowest returns are each split into two vingtiles (1a, 1b, 10a and 10b). Figure 3a graphs buy-sell imbalances for investors at a large discount brokerage (1991-1996), investors at a large retail brokerage (January 1997 through June 1999), and investors at a small discount brokerage (January 1997 through June 1999). Figure 3b graphs buy-sell imbalances for institutional money managers (January 1993 through March 1996) classified as following momentum, value, and diversified strategies. For each day/partition/investor group, we calculate number imbalance as number of purchases minus number of sales divided by total number of trades. The figure depicts the mean for each time-series of daily imbalances for a particular investor group.

Figure 3a

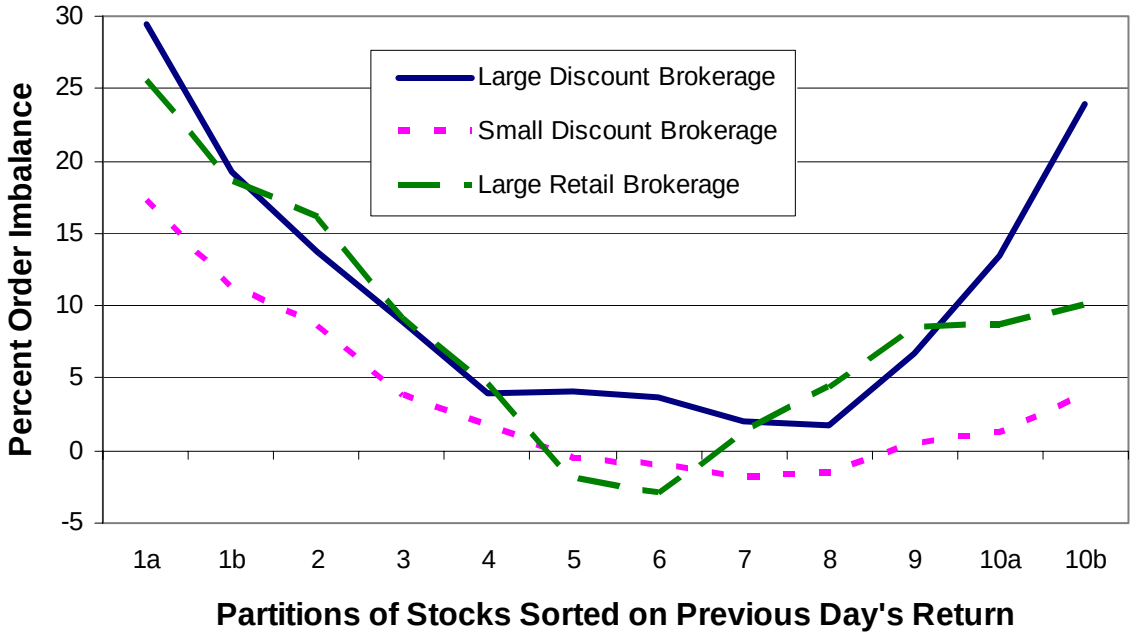
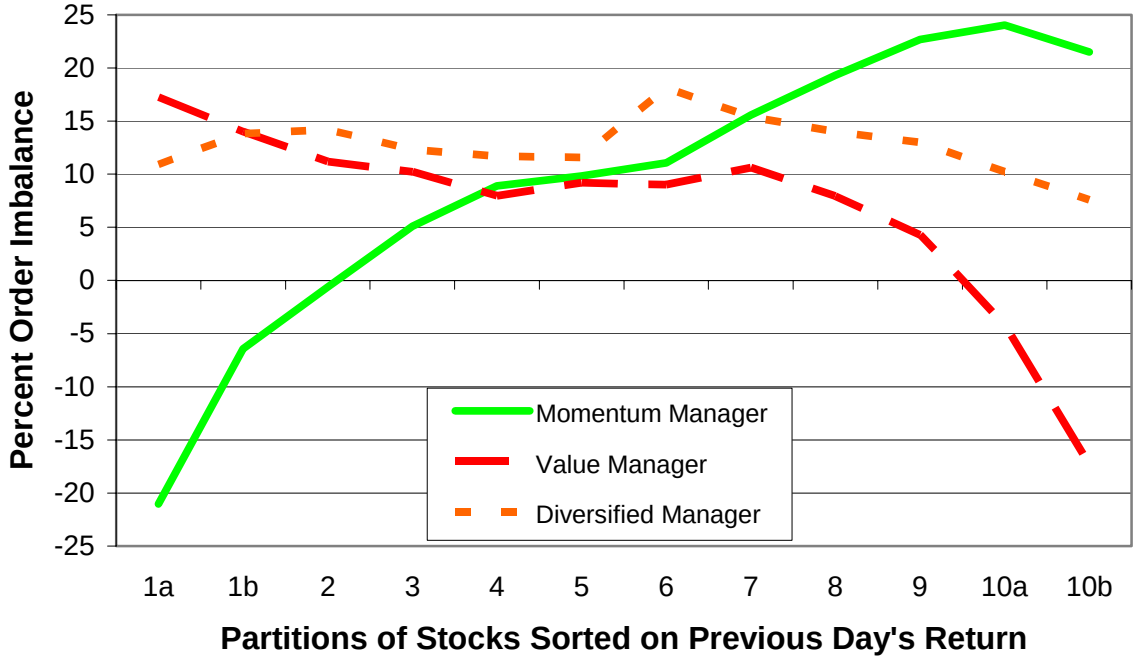


Figure 3b



2.2 The Non-Salient

- Hong, Torous, Valkanov (2002); Pollet (2003)

- Investors not good at handling indirect links
  - Stock market investors specialize in one sector
  - Shocks to other sectors are somewhat neglected
  - Indirect effects of industry-specific shocks neglected

- Example: forecasted increase in price of oil

- Oil industry reacts immediately

- Increase in oil prices will impact most sectors in economy negatively

- Null hypothesis: This information should be incorporated into market returns

- Alternative hypothesis: Information is not incorporated beacuse of inattention

- Hong, Torous, Valkanov (2002)

- Forecast market returns one month ahead using industry-level returns

- Specification:

$$MR_t = \alpha + \beta R_{s,t-1} + \varepsilon_t$$

- Regression run separately for each sector.

- Coefficient β should differ from 0 only about 5% of time

- Problems:
  - lack of of sign structure in prediction
  - industry itself is included in market returns – problem is returns are autocorrelated over time within industry

- Pollet (2002): Scandinavian stock market predicts US stock market one month ahead

- Oil industry predicts several industries one month ahead

3 Attention: Voting

- Principle-Agent relationship:
  - Voter-Politician
  - Shareholder-CEO
  - Employer-Employee

- Does the principal pay attention to the correct determinants of agent's pay?

- Are principals rewarding luck / noise?

3.1 Luck

- Side paper
- Bertrand-Mullainathan (2001)
- Examine questions for CEO pay
- Data on CEO pay (salaries + stock options)
- Data on company performance (accounting / stock returns)
- $w_t =$ pay at time t
- $y_t =$ performance at time t

- $X_t =$ set of controls

- $L_t =$ luck variables measured at time t

- Empirical specification:

- First stage:

$$y_t = \alpha + \beta_0 X_t + \beta_1 L_t + \varepsilon_t$$

- Obtain predicted performance based oin luck: $\hat{y}_t$

- Second stage:

$$w_t = \gamma + \delta X_t + \lambda \hat{y}_t + \varepsilon_t$$

- Coeffcient on λ should be zero according to standard principle/agent model

- Measures for L :

  1. price of oil on pay in 51 US oil companies: 1977-1994

  2. industry-specific exchange rate: 792 corporations (Yermack and Shleifer data)

  3. mean accounting return in 2-digit industry (excluding same company)

- Why is there pay for luck?

  – CEOs stealing, they do not care where money comes from

  - inattention
  - simple, suboptimal contract

- Does this result partly go away in better-managed firms?
- Proxy: number of large shareholder in board
- Check on actions of CEO
- New second stage:

$$w_t = \gamma + \delta X_t + \lambda \hat{y}_t + \lambda GOV * \hat{y}_t + \varepsilon_t$$

Table 1: Pay for Luck for Oil CEOs
(Luck Measure is log Price of Crude Oil)
Dependent Variable: Ln (Total Compensation)[a]

Specification:	General (1)	Luck (2)	General (3)	Luck (4)
Acc. Rate of Return	.82 (.16)	2.15 (1.04)	—	—
Ln(Sh. Wealth)	—	—	.38 (.03)	.35 (.17)
Age	.05 (.02)	.07 (.03)	.05 (.02)	.05 (.02)
$\text{Age}^2 * 100$	-.04 (.02)	-.05 (.02)	-.04 (.02)	-.04 (.02)
Tenure	.01 (.01)	.01 (.01)	.01 (.01)	.01 (.01)
$\text{Tenure}^2 * 100$	-.03 (.02)	-.03 (.01)	-.03 (.02)	-.03 (.02)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Quadratic	Yes	Yes	Yes	Yes
Sample Size	827	827	827	827
Adjusted R^2	.70	—	.75	—

[a]Notes:

1. Dependent variable is the logarithm of total compensation. Performance measure is accounting rate of return in columns (1) and (2) and the logarithm of shareholder wealth in columns (3) and (4). All nominal variables are expressed in 1977 dollars.
2. Summary statistics for the sample of oil firms are available in Appendix Table A1.
3. The luck regression (columns 2 and 4) instrument for performance with the logarithm of the price of a barrel of crude oil in that year, expressed in 1977 dollars.
4. Each regression includes firm fixed effects and a quadratic in year.
5. Standard errors are in parentheses.

Table 3: Pay for Luck[a]

Dep. Var.:	Cash Comp		Ln(Cash)		Ln(Tot Comp)		Ln(Cash)		Ln(Tot Comp)	
Spec.:	General	Luck	General	Luck	General	Luck	General	Luck	General	Luck
				Panel A: Luck Measure is Exchange Rate Shock						
Income	.17	.35	—	—	—	—	—	—	—	—
	(.02)	(.16)								
$\frac{Income}{Assets}$	—	—	2.13	2.94	2.36	4.39	—	—	—	—
			(.16)	(1.28)	(.28)	(2.17)				
Ln(Shareholder Wealth)	—	—	—	—	—	—	.22	.32	.31	.57
							(.02)	(.13)	(.03)	(.23)
Sample Size	1737	1737	1729	1729	1722	1722	1713	1713	1706	1706
Adjusted R^2	.75	—	.75	—	.58	—	.75	—	.59	—
				Panel B: Luck Measure is Mean Industry Performance						
Income	.21	.34	—	—	—	—	—	—	—	—
	(.02)	(.10)								
$\frac{Income}{Assets}$	—	—	2.18	4.02	2.07	4.00	—	—	—	—
			(.12)	(.53)	(.21)	(.86)				
Ln(Shareholder Wealth)	—	—	—	—	—	—	.20	.22	.25	.29
							(.01)	(.12)	(.02)	(.19)
Sample Size	4684	4684	4648	4648	4624	4624	4608	4608	4584	4584
Adjusted R^2	.77	—	.81	—	.70	—	.82	—	.71	—

[a] Notes:

1. Dependent variable is the level of salary and bonus in columns (1) and (2), the logarithm of salary and bonus in columns (3), (4), (7) and (8) and the logarithm of total compensation in columns (5), (6), (9) and (10). Performance measure is operating income before extraordinary items in columns (1) and (2) (in millions), operating income to total assets in columns (3) to (6) and the logarithm of shareholder wealth in columns (7) to (10). All nominal variables are expressed in real dollars.
2. In the luck regressions in Panel A, the performance measure is instrumented with current and lagged appreciation and depreciation dummies and current and lagged exchange rate index growth. First-stage regressions are presented in Appendix Table A2.
3. In the luck regressions in Panel B, the performance measure is instrumented with the total assets-weighted average performance measure in the firm's 2-digit industry (the firm itself is excluded from the mean calculation).
4. Each regression includes firm fixed effects, year fixed effects and demographic controls (quadratics in age and tenure).
5. Standard errors are in parentheses.

- Wolfers (2002)

- Voters
  - Null: Elect politicians that is able
  - Alternative: Inattention –> Luck also plays a role (and other factors will too)

- Data: Governor elections

- Performance measure: State-level output, employment gap, housing prices

- First specification (Table 2, Table 3):

$$\Delta Vote_{s,t} = \alpha \Delta Econ_{s,t}^{S-N} + \lambda \Delta Econ_{t}^{N} + \varepsilon_{s,t}$$

- λ is measure of luck

- Decompose relative stare performance on luck vs. non-luck component

- First stage. Run

$$\Delta Econ_{s,t}^{S-N} = \beta X_{s,t} + \gamma Instr_{s,t} + \eta_{s,t}$$

and decompose the dependent variable into $\Delta\widehat{Econ}_{s,t}^{S-N}$ and $\Delta Econ_{s,t}^{\text{Res } S-N}$

- Then run

$$\begin{aligned}\Delta Vote_{s,t} &= \alpha_0 \Delta\widehat{Econ}_{s,t}^{S-N} + \alpha_1 \Delta Econ_{s,t}^{\text{Res } S-N} + \\ &\quad + \lambda \Delta Econ_t^N + \varepsilon_{s,t}\end{aligned}$$

- Coefficients α_0 and λ capture luck.

- Instruments:
  - oil prices on economy
  - effect of national economy on state economy

- Interpretation:
  - Voters face low incentives in voting decision
  - Do primitive signal-extraction on performance of local economy

Table I: Effects of State Economic Conditions on the Vote for the Incumbent Party in State Gubernatorial Elections

Dependent Variable: Change in incumbent party's share of two-party vote

	Real Income per capita[a]	**Employment gap**[b]	**Real Housing Prices**[c]
Coefficient	.06	.27***	.19**
	(.07)	(.10)	(.08)
Adjusted R^2	-.001	.011	.025
Sample	636 elections 1947-97	636 elections 1947-97	185 elections 1982-97

Each column shows a separate regression of the change in the incumbent party's share against a specific indicator of the state's economic performance over the two years leading up to the election.
(Robust standard errors in parentheses.)
***, **, * denote statistical significance at 1%, 5%, and 10%, respectively.
[a] Two-year ended percentage change in state income per capita. Data from the Commerce Department, deflated using the Chain GDP deflator.
[b] Two-year ended change in the employment gap, measured in percentage points. Constructed from BLS non-farm payrolls data (see Appendix B for details). Results are coded so that a positive number denotes an improving economy (analogous to a declining unemployment rate).
[c] Two-year ended percentage change in real housing prices. Housing prices are measured from a repeat sales index provided by the Office of Federal Housing Enterprise Oversight, deflated using the Chain GDP deflator.

Table II: Do Voters Filter Out the Performance of the National Economy?

	Independent Variable (2 year-ended changes) (Each column is a separate regression)		
	ΔEmployment gap (%points) [a]	**%ΔHouse Prices**	**%ΔReal income per capita**
Panel A: OLS regression **Dependent Variable: ΔIncumbent *Party* Two Party Preferred Vote Share (OLS)**			
	(1)	**(2)**	**(3)**
α: Effects of competence ($\Delta State_{s,t}$-$\Delta National_t$)	.42***	.22***	.08
	(.13)	(.08)	(.08)
λ: Effects of luck ($\Delta National_t$)	.14	.09	.01
	(.12)	(.14)	(.12)
Test: α=λ	F(1,633) = 2.84*	F(1,182)=0.73	F(1,633)=0.33
Adjusted R^2	.013	.022	-.001
n (elections)	636 (*1947-97*)	185 (*1982-97*)	636 (*1947-97*)
Panel B: Probit regression[b] **Dependent Variable: Indicator =1 if incumbent *Governor* was re-elected; =0 otherwise**[c]			
	(4)	**(5)**	**(6)**
α: Effects of competence ($\Delta State_{s,t}$-$\Delta National_t$)	3.0***	1.2**	1.4**
	(1.0)	(0.5)	(0.6)
λ: Effects of luck ($\Delta National_t$)	1.6*	0.8	1.6
	(0.9)	(0.8)	(1.0)
Test: α=λ	$\chi^2(1)$=1.16*	$\chi^2(1)$=0.18	$\chi^2(1)$=0.02
Pseudo R^2	.026	.059	.016
n (elections)	356 (*1950-88*)	69 (*1982-88*)	356 (*1950-88*)

***, **, * denote statistically significant at 1%, 5%, and 10%, respectively.
(Robust standard errors in parentheses.)

[a] Employment gap is coded so that a positive number denotes an improving economy (analogous to a declining unemployment rate).

[b] Probit coefficients report the marginal change in the probability of re-election for a marginal change in economic outcomes, evaluated at cell means.

[c] Re-elected incumbents are coded to a value of 1. Incumbents who lost a primary race, a general election, or who decided not to run again are coded as 0. Incumbents who ran for higher office, or were barred from re-election by term limits are dropped from the sample.

Table III: Robustness: Relative Performance Evaluation

Dependent Variable: ΔIncumbent *Party* Two Party Preferred Vote Share (OLS)

	Employment gap[a] *(2-year ended Δ, %pts)*		**House Prices**[a] *(2-year ended %Δ)*		**Real income per capita**[a] *(2-year ended %Δ)*	
	α: ΔState-ΔNational *Competence*	**λ: ΔNational** *Luck*	**α: ΔState-ΔNational** *Competence*	**λ: ΔNational** *Luck*	**α: ΔState-ΔNational** *Competence*	**λ: ΔNational** *Luck*
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \varepsilon_{s,t}$						
Basic Specification (from Table 2)	.42***	.14	.22***	.09	.08	.01
	(.13)	(.12)	(.08)	(.14)	(.08)	(.12)
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \phi President's\ Party_{s,t} + \varphi(President's\ Party_{s,t} * \Delta National\ performance_t) + \varepsilon_{s,t}$						
+ Control for president's ec. performance (*President's Party*[b] * *ΔNational Performance*)	.41***	.13	.20**	.10	.09	.04
	(.13)	(.12)	(.08)	(.14)	(.07)	(.11)
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \phi President's\ Party_{s,t} + \sum_{t \in years} \varphi_t (President's\ Party_{s,t} * Year_t) + \varepsilon_{s,t}$						
+ Control for national partisan swings (*President's Party*[b] * *Year Fixed Effects*)[c]	.42***	.15	.21**	.06	.07	-.03
	(.14)	(.12)	(.08)	(.15)	(.07)	(.11)
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \phi President's\ Party_{s,t} + \sum_{t \in years} \varphi_t (President's\ Party_{s,t} * Year_t) + \sum_{s \in states} \mu_s State_s + \varepsilon_{s,t}$						
+ Control for *state fixed effects*	.41***	.15	.20*	.10	.09	-.04
	(.14)	(.13)	(.12)	(.21)	(.08)	(.12)
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \phi President's\ Party_{s,t} + \sum_{t \in years} \varphi_t (President's\ Party_{s,t} * Year_t) + \sum_{s \in states} \mu_s State_s + \kappa Party_{s,t} + \pi(Party_{s,t} * \Delta State\ performance_{s,t}) + \varepsilon_{s,t}$						
+ Control for economy-contingent preferences (*Party*[b] * *ΔState Performance*)	.40***	.15	.20*	.13	.09	-.04
	(.14)	(.13)	(.11)	(.21)	(.08)	(.12)
$\Delta Incumbent\ vote_{s,t} = \alpha(\Delta State\ performance_{s,t} - \Delta National\ performance_t) + \lambda \Delta National\ performance_t + \phi President's\ Party_{s,t} + \sum_{t \in years} \varphi_t (President's\ Party_{s,t} * Year_t) + \sum_{s \in states} \mu_s State_s + \kappa Party_{s,t} + \pi(Party_{s,t} * \Delta State\ performance_{s,t}) + \sum_{t \in years} \tau_t Year_t + \varepsilon_{s,t}$						
+ Control for national swings in pro- or anti-incumbent sentiment (*Year fixed effects*)	.47***	n.a.	.18	n.a.	.10	n.a.
	(.15)		(.12)		(.09)	
Sample (*n*)	*1947-97 (636 elections)*		*1982-97 (185 elections)*		*1947-97 (636 elections)*	

***, **, * denote statistically significant at 1%, 5%, and 10%, respectively. (Robust standard errors in parentheses.)

[a] Economic variables are measured as a percentage change over the two years leading up to the election. (Employment gap is a simple difference, measured in percentage points.)

[b] *President's Party* takes a value of +1 if the incumbent governor is of the same party as the President, and –1 otherwise. (Symmetric treatment reflects the fact that one party's loss is the other party's gain.) Similar logic underlies the coding of $Party_{s,t}$, which takes a value of +1 for Democrat incumbents, and –1 for Republicans.

[c] Note that the interaction of *President's* $Party_{s,t}$ with year fixed effects could equivalently be expressed as the interaction of $Party_{s,t}$ with year fixed effects.

Table IV: Relative Performance Evaluation – Testing for Asymmetries

Dependent Variable: ΔIncumbent *Party* Two Party Preferred Vote Share (OLS)

$$\Delta Incumbent\ vote_{s,t} = c + \alpha(\Delta State\ employment\ gap_{s,t} - \Delta National\ employment\ gap_t) + \lambda \Delta National\ employment\ gap_t + \varepsilon_{s,t}$$

	α: ΔState-ΔNational *Competence*	**λ: ΔNational *Luck***	**Adj. R^2**	***n***
Whole Sample	**.42*** (.13)**	**.14 (.12)**	.013	636
	Sample excluding Downturns			
ΔState> -10%	.47*** (.13)	.18 (.12)	.018	632
ΔState> -8%	.51*** (.14)	.21 (.12)	.019	622
ΔState> -6%	.54*** (.14)	.23 (.13)	.020	602
ΔState> -4%	.52*** (.15)	.25 (.15)	.017	544
ΔState> -2%	.41** (.19)	.20 (.18)	.006	434
ΔState> 0%	.41** (.22)	.20 (.24)	.004	283
	Sample excluding Booms			
ΔState<+10%	.44*** (.14)	.15 (.13)	.014	631
ΔState<+8%	.47*** (.15)	.17 (.13)	.014	615
ΔState<+6%	.40** (.16)	.08 (.14)	.008	587
ΔState<+4%	.36* (.19)	.14 (.17)	.004	535
ΔState<+2%	.23 (.22)	.00 (.21)	-.001	463
ΔState<0%	.27 (.29)	-.15 (.31)	.001	353

***, **, * denote statistically significant at 1%, 5%, and 10%, respectively.
(Robust standard errors in parentheses.)
Employment gap measured as the change over the years leading up to the election (percentage points)

Table V: IV Tests of Voter Rationality: State Responses to Oil Prices

Panel A: First Stage Regression (2-year ended changes, % points)		
	Instruments for ΔState Employment Gap – ΔUS Employment Gap	
	ΔLog Real Oil Price Interacted with state dummies	**ΔLog Real Oil Price Interacted with state industry shares (measured in the 1940s)**
	$U_{s,t}^{State-National} = \sum_{s \in states} \mu_s + \beta_s (State_s * \Delta P_{t-1}^{Oil})$	$U_{s,t}^{State-National} = \sum_{i \in industries} \theta_i (Ind.share_{s,i}^{1940s} * \Delta P_{t-1}^{Oil})$
Estimated coefficients	Average: 0 State Range: -.07 to +0.23	Average: 0 State Range: -.06 to +0.20
Most positive effects	AK, LA, OK, TX, WY	Construction, farm, finance, mining
Most negative effects	IN, MI, NV, TN	Manufacturing, services
Adjusted R^2	.15	.12
Sample (n)	2504 (1947-97)	2504 (1947-97)
Panel B: Explaining ΔIncumbent *Party* Vote Share (OLS) $\Delta Incumbent\ vote\ share_{s,t} = \lambda U_t^{National} + \delta \hat{U}_{s,t}^{State\text{-}National} + \alpha U_{s,t}^{Unexplained} + \varepsilon_{s,t}$		
$U_t^{National}$	.15	.15
Rule of thumb signal extraction (λ)	(.12) [.12]	(.12) [.12]
$\hat{U}_{s,t}^{State-National}$	1.13^{***}	$.89^{*}$
Sophisticated signal extraction (δ)	(.37) [.40]	(.53) [.57]
$U_{s,t}^{Unexplained}$	$.32^{**}$	$.38^{***}$
Returns to competence (α)	(.14) [.15]	(.14) [.14]
Adjusted R^2	.017	.014
Sample	636 (1947-97)	636 (1947-97)
Panel C: Explaining Whether Incumbent *Governor* is Re-elected (Probit[a]) $\Delta Incumbent\ re\text{-}elected_{s,t} = \lambda U_t^{National} + \delta \hat{U}_{s,t}^{State\text{-}National} + \alpha U_{s,t}^{Unexplained} + \varepsilon_{s,t}$		
$U_t^{National}$	1.7^{*}	1.6^{*}
Rule of thumb signal extraction (λ)	(0.9) [0.9]	(0.9) [0.9]
$\hat{U}_{s,t}^{State-National}$	4.3^{*}	3.9
Sophisticated signal extraction (δ)	(2.4) [2.7]	(3.1) [3.5]
$U_{s,t}^{Unexplained}$	2.8^{***}	2.9^{***}
Returns to competence (α)	(1.0) [1.1]	(1.0) [1.0]
Pseudo R^2	.027	.026
Sample	356 (1950-88)	356 (1950-88)

***, **, * denote statistically significant at 1%, 5%, and 10%, respectively.
(Robust standard errors in parentheses.) [Bootstrapped standard errors in square brackets.]
$U^{National}$ denotes the change in the national employment gap in the two years leading up to the election. $U^{State\text{-}National}$ denotes the change in the state employment gap less the change in the national employment gap. The first stage regression decomposes this state-specific cycle into $\hat{U}^{State-National}$, a component attributed to the instrument set, and a residual $U^{Unexplained} = U^{State} - U^{National} - \hat{U}^{State\text{-}National}$.
[a] Probit coefficients report the marginal change in the probability of re-election for a marginal change in the employment gap, evaluated at cell means. Re-elected incumbents are coded to a value of 1. Incumbents who lost a primary race, a general election, or who decided not to run again are coded as 0. Incumbents who ran for higher office, or were barred from re-election by term limits, are dropped from the sample.

3.2 Order

- Inattentive voters: Order of candidates may matter as well
- Ho and Imai (2004)
- Exploit randomization of ballor order in California
- Years: 1978-2002
- Data: 80 Assembly Districts

- Observe each candidate in different orders in different districts

- Compute abolute vote (Y) gain

$$E\left[Y\left(i=1\right)-Y\left(i\neq 1\right)\right]$$

and percentage vote gain

$$E\left[Y\left(i=1\right)-Y\left(i\neq 1\right)\right]/E\left[Y\left(i\neq 1\right)\right]$$

- Result:
  - Small to no effect for major candidates
  - Large effects on minor candidates

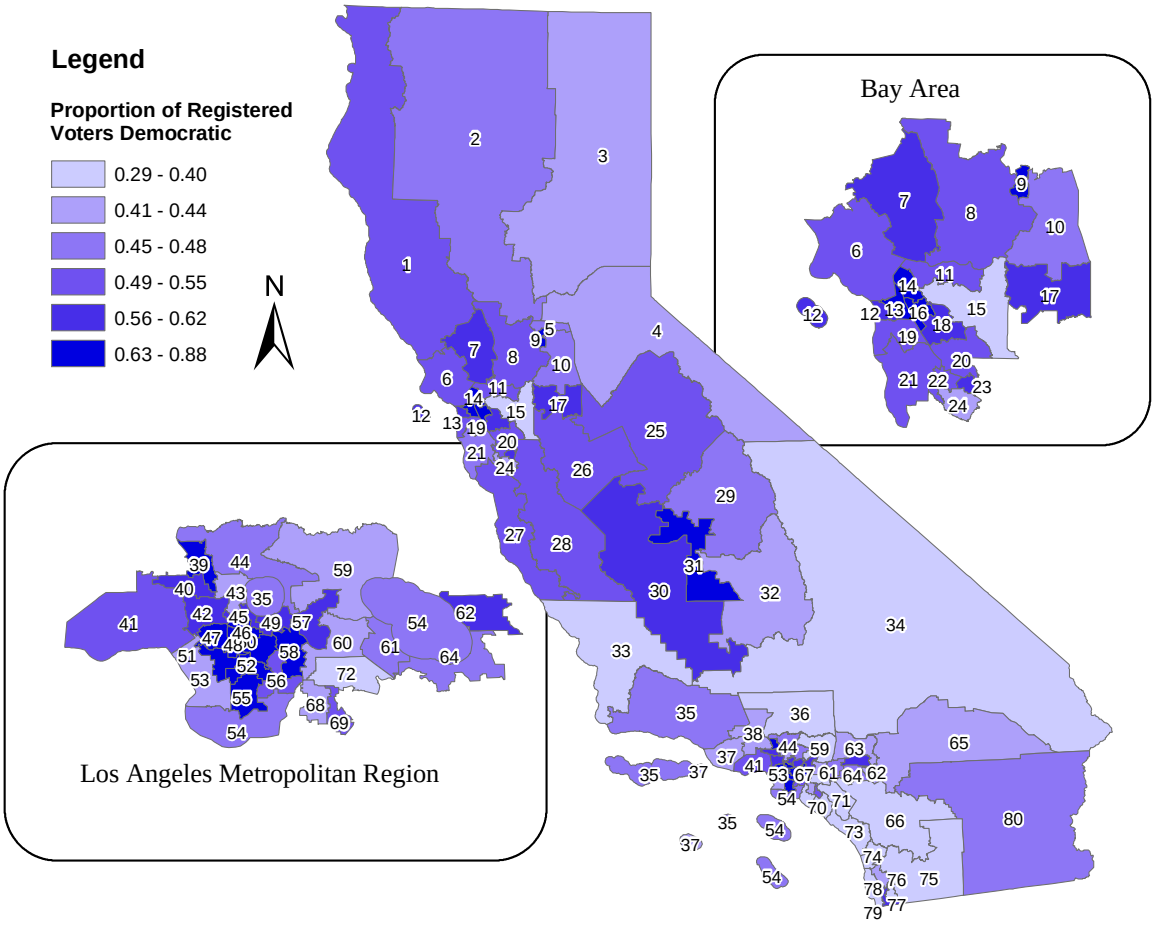


Figure 1: 1992 California Assembly Districts. The districts with darker color are those with a higher proportion of Democrats among registered voters.
Source: The California Spatial Information Library. Map created using Arcmap.

behavioral changes of candidates, making it difficult to isolate the direct effects of ballot order on voters. For example, candidates listed last on the ballot in a particular assembly district might campaign more intensely in that district, in fear of some ballot order effect. Or, candidates might be chosen to assure a higher ballot order in favorable districts (Masterman, 1964). However, such a scenario seems unlikely given that the randomized alphabet is drawn very late in the game.

All but write-in candidates must have declared candidacy and been certified by the time that the drawing of a randomized alphabet takes place, and even sample (non-randomized) ballots are printed before the drawing. Only minor adjustments, such as removal of a candidate from the ballot in the case of a death, occur after the drawing.[9]

[9]Even if there are candidate behavioral changes resulting from the drawing, this "intention-to-treat" effect may still be of important policy significance.

Year	Election	Randomized Alphabet																								
1982	Primary	S	C	X	D	Q	G	W	R	V	Y	U	A	N	H	L	P	B	K	J	I	E	T	O	M	F	Z
	General	L	S	N	D	X	A	M	W	V	T	O	F	I	B	K	Y	U	P	E	Q	C	J	Z	H	R	G
1983	Consolidated	L	C	P	K	I	A	U	G	Z	O	N	B	X	D	W	H	E	M	F	V	R	S	T	Y	Q	J
1984	Primary	W	M	F	B	Q	Y	T	D	J	U	O	V	I	K	R	H	S	N	P	C	A	E	L	Z	G	X
	General	V	W	I	H	R	Q	G	J	O	M	T	S	Y	C	A	F	U	X	K	B	P	E	Z	N	D	L
1986	General	Q	N	H	U	B	J	E	G	M	V	L	W	X	C	K	O	F	D	Z	R	Y	I	T	S	P	A
1988	Primary	W	O	K	N	Q	A	V	T	H	J	F	Z	L	B	U	D	Y	M	I	R	G	C	E	S	X	P
	General	S	W	F	M	K	J	U	Y	A	T	V	G	O	N	Q	B	D	E	P	L	Z	C	I	X	R	H
1990	Primary	E	J	B	Y	Q	F	K	M	O	V	X	L	N	Z	C	W	A	P	R	D	G	T	H	I	S	U
	General	W	F	C	L	D	I	N	J	H	V	K	O	S	A	R	E	Q	B	T	M	Y	U	G	Z	X	P
1992	Primary	U	R	F	A	J	C	D	N	M	K	P	Z	Y	X	G	W	O	H	E	B	I	S	V	L	Q	T
	General	F	Y	U	A	J	S	B	Z	G	O	E	Q	R	L	I	M	H	V	N	T	P	D	K	X	C	W
1994	Primary	K	J	H	G	A	M	I	Q	U	N	C	Z	S	W	V	R	P	Y	B	L	O	T	D	F	E	X
	General	V	I	A	E	M	S	O	K	L	B	G	N	W	Y	D	P	U	F	Z	Q	J	X	C	R	H	T
1996	Primary	G	E	F	C	Y	P	D	B	Z	I	V	A	U	S	M	L	H	K	N	T	O	J	Q	R	X	W
	General	J	Y	E	P	A	U	S	Q	B	H	T	R	K	N	L	X	F	D	O	G	M	W	I	Z	C	V
1998	Primary	L	W	U	J	X	K	C	N	D	O	Q	A	P	T	Z	R	Y	F	E	V	B	H	G	I	M	S
	General	W	K	D	N	V	A	G	P	Y	C	Z	I	S	T	L	J	X	Q	O	F	H	R	B	U	M	E
2000	Primary	O	P	C	Y	I	H	X	Z	V	R	S	Q	E	K	L	G	D	W	J	U	T	M	B	F	A	N
	General	I	T	F	G	J	S	W	R	N	M	K	U	Y	L	D	C	Q	A	H	X	O	E	B	V	P	Z
2002	Primary	W	I	Z	C	O	M	A	Q	U	K	X	E	B	Y	N	P	T	R	L	V	S	J	H	D	F	G
	General	H	M	V	P	E	B	Q	U	G	N	D	K	X	Z	J	A	W	Y	C	O	S	F	I	T	R	L
2003	Recall	R	W	Q	O	J	M	V	A	H	B	S	G	Z	X	N	T	C	I	E	K	U	P	D	Y	F	L

Table 1: Randomized Alphabets Used for the California Statewide Elections Since 1982.

3.2 Are Alphabets Really Random?

Election officials seem to have taken seriously their legal obligation of conducting the alphabet lottery. Given the evidence of manipulation of ballot order in other states (e.g., Darcy and McAllister, 1990), however, we conduct statistical tests to ensure empirically the accurate implementation of the randomization. Such tests often help discover unexpected implementation errors of randomization (Imai, 2004). As shown in Table 1, we collected the randomized alphabets used for 23 California statewide elections since 1982. We use this list to test whether the randomization procedure described above has in practice produced completely randomized alphabets not favoring any particular letters, and hence particular candidates.

We conduct a rank test under the null hypothesis that the alphabet is completely randomized. In particular, we compare the relative positions of all possible pairs of letters by calculating the

Election		President	Senate	Governor	Lt. Gov.	Atty Genl	Controller	Ins. Comm.	Sec. State	Treasurer	Supt Educ
1978	General	–		5							
1980	General	7	5	–	–	–	–	–	–	–	–
1982	General	–	5	5							
	Primary	–	19	20							
1984	General	5	–	–	–	–	–	–	–	–	–
1986	General	–	5	5							
	Primary	–	20	9							
1988	General	5	5	–	–	–	–	–	–	–	–
	Primary		6	–	–	–	–	–	–	–	–
1990	General	–	–	5							
	Primary	–	–	19							
1992	General	6	$5, 5^a$	–	–	–	–	–	–	–	–
1994	General	–	6	5							
	Primary	–		12							
1996	General	8	–	–	–	–	–	–	–	–	–
1998	General	–	7	7	7	5	7	6	7	6	2
	Primary	–	13	17	13	10	7	8	8	9	5
2000	General	7	7	–	–	–	–	–	–	–	–
	Primary	23	15	–	–	–	–	–	–	–	–
2002	General	–	–	6	7	5	5	6	7	6	2
	Primary	–	–	11	8	6	10	11	13	7	4

Table 2: Number of Candidates Running in All Races Examined. "–" indicates that no election was held for that office in a particular year. Blank cells represent races where election returns data were not available by assembly districts. The number of candidates in this table differs slightly from total number of candidates analyzed because of several uncontested party primaries.

[a]There were two senatorial elections in 1992 both of which had five candidates running.

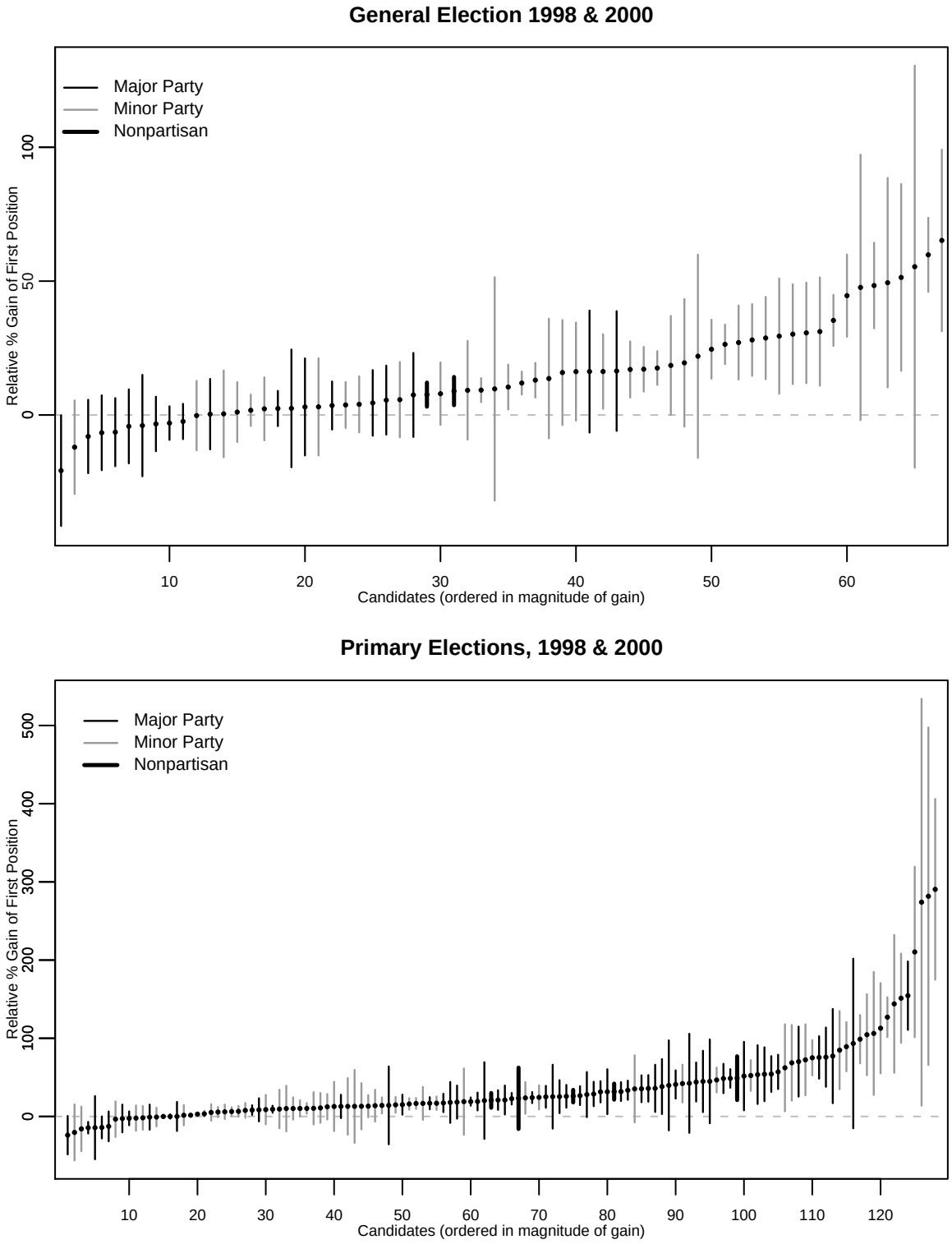


Figure 2: Candidate-Specific Average Relative Gain due to Being Listed in First Position on Ballots for 1998 and 2000 Elections. The top panel shows the results for general elections, and the bottom panel displays those for primary elections. Circles indicate point estimates for each candidate, and vertical bars represent estimated 95% confidence intervals. In general elections, only minor party and nonpartisan candidates are affected by the ballot order. In primary elections, however, major party candidates are also affected.

	General				Primary			
	Absolute		Relative		Absolute		Relative	
	ATE	SE	ATE	SE	ATE	SE	ATE	SE
Democratic	0.05	0.46	0.25	0.90	1.89	0.32	43.58	5.53
Republican	-0.06	0.53	-0.43	1.29	2.16	0.46	33.62	5.91
American Independent	0.16	0.02	20.83	1.39	2.33	0.15	26.76	3.55
Green	0.56	0.17	21.18	5.82	3.15	1.16	6.24	3.54
Libertarian	0.23	0.02	14.56	1.03	6.59	1.42	71.92	13.55
Natural Law	0.31	0.06	26.13	2.85	0.40	0.08	44.78	5.45
Peace and Freedom	0.28	0.03	25.49	2.15	6.31	0.53	14.75	1.43
Reform	0.26	0.07	19.57	2.23	4.11	1.56	48.45	9.66
Nonpartisan	1.95	0.30	9.21	3.31	3.44	0.78	19.42	4.05

Table 3: Party-Specific Average Causal Effects of Being Listed in First Position on Ballots Using All Races from 1978 to 2002. ATE and SE represent the average causal effects and their standard errors, respectively. For general and primary elections, the left two columns present the estimates of average absolute gains in terms of the total or party vote, respectively, while the right two columns show those of average relative gains. Each candidate-specific effect is averaged over different races to obtain the overall average effect for each party. In general elections, only minor party and nonpartisan candidates are affected by the ballot order. In primaries, however, the candidates of all parties are affected. The largest effects are found for nonpartisan candidates.

1.6% of the party vote. Given that primary races have a much larger number of candidates, it is notable that the absolute gain is larger than for general elections (see also Section 4.4).

Averaging over all the races from 1978 to 1992, Table 3 summarizes the estimated ballot effects for these 25 years.[17] The rough patterns of the 1998 and 2000 elections hold across all elections studied. In general elections, major party candidates exhibit no discernible ballot order effect, while the effect on minor party candidates is substantial. Minor party candidates typically gain from 15 to 30% of their baseline vote share in general elections. Given that minor party candidates generally receive only a small proportion of the vote, however, this amounts to an average absolute gain of roughly 0.2 to 0.6% of the total vote cast.

Testable propositions deriving from partisan cue theory would predict that cognitive biases such as ballot effects should be most prominent for nonpartisan races, independent candidates, and primary races, since party labels are least informative in such races. These predictions bear out consistently in our results. Independent and nonpartisan candidates gain 2.4% of the absolute

[17] In cases where multiple candidates from the same party or multiple nonpartisan candidates contested the election, such as in primaries or nonpartisan elections, the simple average of those candidate-specific point estimates and standard errors are used to obtain an estimate for each race, and these estimates are then averaged across elections with the number of candidates in each race as weights.

4 Media: Data

- Media deliver information:
  - TV
  - Radio
  - Newspapers
  - Internet

- Media data is fairly easily available:
  - Lexis-Nexis: Newspaper (TV) Content
  - Vanderbilt data set: TV news stories
  - Warren News: Cable channels

- Local monopolies in media markets:
  - Towns have 1 (rarely 2) newspapers (Genesove, 2000)
  - Towns have 1 (rarely 2) cable providers
  - Only two national papers (from late 80s): USA Today, NYT
  - Owners can spin news

- Media topics:
  - Effect of media on politicians (Besley and Burgess, 2002; Stromberg, 2004) – Skip this
  - Effect of media on consumption (George and Waldfogel, 2002; Dyck and Zingales, 2003)
  - Effect of media bias (Groseclose, 2004; Shleifer and Mullainathan, 2004; DellaVigna and Kaplan, 2004)

– Effect of advertisement

5 Media: Consumer Behavior

- Does media affect consumer behavior?

- Channels:
  - Persuasion. How easy is to convince people?
  - Attention. Focus attention on certain topics

- George and Waldfogel (2002): New York Times and voter behavior

- Gregorio

Does the New York Times Spread Ignorance and Apathy?

L. George and J. Waldfogel

Presentation by Gregorio Caetano

April 25, 2005

Presentation Structure

- Overview of the paper
- Results
- Estimation Strategy
- First Stage Relationship
- Reduced-form Relationship
- Tests of Robustness
- Critical Comments
- Suggestion for Future Research

Overview of the Paper

- Main questions:
  - (First Stage) Does NYT draw readers away from local newspapers?
  - (Reduced-form) Does NYT penetration in local areas reduce voting in local elections because of that?

Results

- One-for-one displacement of local papers sales by NYT sales.
- Each additional copy of the NYT sold in a MSA reduces political participation among individuals with a college degree by 4-5 votes (average is 3 readers for each newspaper).

Overview of the Paper

- Basic Story:
  - New York Times doesn't cover local issues like local newspapers do.
  - People may stop reading local newspapers <u>because</u> they are reading New York Times.
  - This may "distract" people from local affairs.
  - Eventually they could decrease the propensity to vote in local elections.

Estimation Strategy

- Pretty good data:
  - Zip code-level data for local newspaper circulation (1995 and 1998).
  - MSA-level data for NYT circulation (1995, 1997 and 1999).
  - Individual-level data for voting (CPS 1994, 1996 and 1998).
  - Zip code-level demographic data for 1990 Census.
  - Product-level data for local newspaper content.

Estimation Strategy

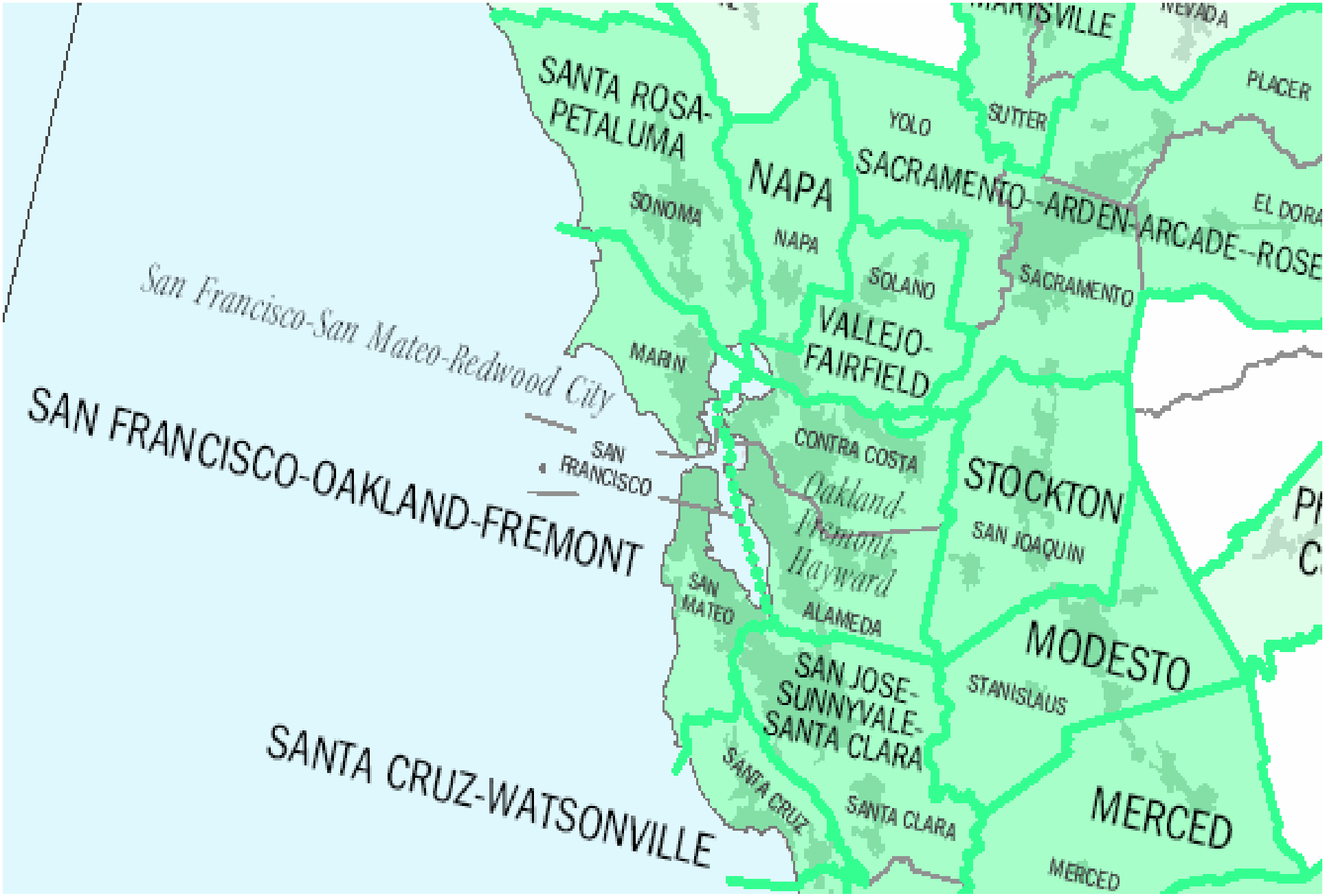
$$s_{zt}^{h} = \alpha_0^{h} + \alpha_1^{h} NYT_{Mt} + \mu_M^{h} + \varepsilon_{zt}^{h}$$
$$s_{zt}^{l} = \alpha_0^{l} + \alpha_1^{l} NYT_{Mt} + \mu_M^{l} + \varepsilon_{zt}^{l} \qquad (1)$$

$$s_{zt} = e_z s_{zt}^{h} + (1 - e_z) s_{zt}^{l} \qquad (2)$$

$$s_{zt} = \beta_0 + \beta_1 NYT_{Mt} + \beta_2 e_z + \beta_3 (e_z NYT_{Mt}) + \nu_{zt} \qquad (3)$$

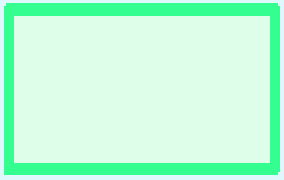
$$\nu_{zt} = \mu_M^{l} + \varepsilon_{zt}^{l} + e_z (\mu_M^{h} - \mu_M^{l}) + e_z (\varepsilon_{zt}^{h} - \varepsilon_{zt}^{l}) \qquad (4)$$

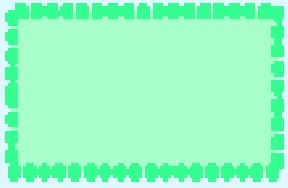
Why should we care about MSA fixed effects?



Legend

OLYMPIA  Metropolitan Statistical Area

Shelton  Micropolitan Statistical Area

Tacoma  Metropolitan Division

1st Stage: Effect of NYT on Readership of Local Newspapers

- Two approaches:
  - Cross-section:
    - Assume $\mu_M^h = \mu_M^l$: no unobservable effect in behavior related to readership of local newspapers between high educated and low educated people within a given MSA.

$$s_{zt} = \beta_0 + \beta_1 NYT_{Mt} + \beta_2 e_z + \beta_3 (e_z NYT_{Mt}) + \mu_M + v_{zt}$$

    - Identification: For a given year (1995 or 1998), difference in readership between high and low educated people across MSAs with different NYT penetration.

1st Stage: Effect of NYT on Readership of Local Newspapers

- Longitudinal:
  - Pooled data (1995 and 1998).
  - Allowing $\mu_M^h \neq \mu_M^l$ by adding education-specific MSA fixed effects ($e_z \mu_M$) .
  - Adding year fixed effects (μ_Y) and education-specific year fixed effects ($e_z \mu_Y$).

$$s_{zt} = \beta_0 + \beta_1 NYT_{Mt} + \beta_{2M} e_z + \beta_3 (e_z NYT_{Mt}) + \mu_M + e_z \mu_M + \mu_Y + (e_z \mu_Y)$$

  - Identification: Difference of changes over time and across MSAs of NYT penetration and local newspaper readership between high and low education groups.

Table 3: Does the *New York Times* Depress Local Newspaper Circulation?

	Cross Sectional Results		*Longitudinal Results*
	1995	1998	1995-1998
	(1)	(2)	(3)
Per Capita NYT (β_1)			1.5572
			(1.95)
Zip Fraction High Ed (β_2)	***0.1926***	***0.1875***	***0.1628***
	(6.57)**	***(7.04)*****	***(16.80)*****
1998 Year Dummy			-0.0117
			(9.53)**
Zip High Ed * 1998			0.0105
			(3.01)**
Zip Fraction High Ed * NYT (β_3)	***-4.6355***	***-5.2955***	***-10.9005***
	(1.64)	***(2.03)****	***(4.31)****
Zip Fraction Black	-0.0664	-0.0614	-0.0574
	(7.01)**	(7.55)**	(13.03)**
Zip Fraction Asian	-0.1129	-0.1418	-0.1032
	(3.07)**	(4.07)**	(4.30)**
Zip Fraction Native American	-0.0978	-0.0890	-0.0867
	(1.61)	(1.79)	(4.41)**
Zip Fraction Other Race	0.0001	0.0079	-0.0994
	(0.00)	(0.12)	(7.62)**
Zip Fraction Age>=65	0.2373	0.1299	0.1895
	(4.91)**	(2.68)**	(5.23)**
Zip Fraction Age<30	-0.1752	-0.2330	-0.2007
	(2.99)**	(4.39)**	(7.93)**
Zip Median Income ($1,000)	0.0002	0.0005	0.0003
	(0.61)	(1.59)	(2.45)*
Constant	0.1912	0.2180	0.2061
	(4.62)**	(5.74)**	(10.31)**
Fixed Effects	MSA	MSA	MSA x Education
Observations	8,993	8,990	17,983
MSA's	259	259	259

Notes: Dependent variable is *per capita* local newspaper sales in the zip code. All specifications are population-weighted, with standard errors clustered by MSA for cross-sectional specifications and MSA x year for longitudinal specifications. T-statistics in parentheses: * significant at 5% level; ** significant at 1% level. Constants in fixed effects regressions represent the average value of the fixed effects.

Reduced-Form: Effect of NYT on Voting of Local Elections

- Individual-level data for voting.
- Linear Probability Models.
- Three approaches**:**
  - *Cross-section* (similar to first stage):

$$V_i = \gamma_0 + \gamma_1 NYT_M + \gamma_2 ed_i + \gamma_3 (NYT_M ed_i) + \mu_M$$

  - *Longitudinal* (MSA fixed effect <u>for each year</u>):

$$V_i = \gamma_0 + \gamma_1 NYT_M + \gamma_2 ed_i + \gamma_3 (NYT_M ed_i) + \mu_M + \mu_Y + ed_i \mu_Y + \mu_Y \mu_M$$

  - *Longitudinal* (MSA fixed effect <u>for each education group</u>):

$$V_i = \gamma_0 + \gamma_1 NYT_M + \gamma_3 (NYT_M ed_i) + \mu_M + \mu_Y + ed_i \mu_Y + ed_i \mu_M$$

Table 5: Does the *New York Times* Depress Voting among the College Educated?

	Voting Probability Non-Presidential Elections				
	1994	1998	Pooled (94, 98)	Pooled (94, 98)	Pooled (94, 98)
	(1)	(2)	(3)	(4)	(5)
Per Capita NYT (γ_3)	-	-	***-9.818***	***-1.562***	***-5.647***
	-	-	***(1.81)***	***(0.71)***	***(1.21)***
High Ed (γ_2)	0.147	0.136	0.169	0.156	-
	(6.18)**	(4.29)**	(7.07)**	(6.68)**	-
NYT*High Ed (γ_5)	***-4.568***	***-3.531***	***-4.108***	***-3.824***	***-22.054***
	(3.26)*	***(1.89)***	***(3.78)****	***(3.43)****	***(2.69)****
1998 Year Dummy	-	-	-0.067	-0.110	-0.048
	-	-	(1.40)	(2.04)*	(0.94)
High Ed*1998	-	-	-0.046	-0.023	-0.115
	-	-	(1.24)	(0.63)	(4.07)**
Black	0.041	0.097	0.069	0.066	0.067
	(2.69)**	(5.21)**	(5.45)**	(5.21)**	(5.36)**
Asian	-0.163	-0.186	-0.177	-0.166	-0.175
	(6.13)**	(5.73)**	(7.73)**	(6.40)**	(7.70)**
Indian	-0.072	-0.086	-0.080	-0.077	-0.079
	(3.02)**	(2.71)**	(4.06)**	(3.80)**	(4.00)**
Hispanic	-0.069	-0.043	-0.054	-0.056	-0.058
	(4.00)**	(3.04)**	(5.02)**	(5.07)**	(5.52)**
Sex	-0.017	-0.020	-0.018	-0.018	-0.018
	(4.22)**	(4.61)**	(6.25)**	(6.20)**	(6.16)**
Age <30	-0.223	-0.233	-0.229	-0.228	-0.228
	(28.68)**	(33.74)**	(43.87)**	(43.71)**	(43.90)**
Age 65+	0.200	0.218	0.209	0.209	0.208
	(28.35)**	(24.21)**	(36.23)**	(36.48)**	(36.05)**
Constant	0.433	0.300	0.284	0.336	0.386
	(8.33)**	(14.07)**	(5.81)**	(6.62)**	(8.00)**
Other Variables	Income, Statewide Elections	Income, Statewide Elections	Income, Statewide Elections	Income, Statewide Elections	Income, Statewide Elections
Fixed Effects	MSA	MSA	MSA	MSA x Year	MSA x Education
Observations	45,456	42,564	88,020	88,020	88,020

Notes: Linear probability models with standard errors clustered by MSA for cross-sectional estimates and MSA x Year for pooled estimates. T-statistics in parentheses: * significant at 5% level; ** significant at 1% level. Constants in fixed effects regressions represent the average value of the fixed effects. Fourteen income dummy variables and statewide election variables not shown. State election variables include a dummy variable for statewide races alone, interacted with high education, interacted with year dummies, and interacted with both high education and year.

Tests of Robustness

- Local newspapers appear to have increased the emphasis on topics not covered by the NYT and de-emphasized topics extensively covered by the NYT during this period.

- There is no effect in the propensity to vote in presidential elections attributed to the NYT penetration in local markets.

- The change in voting behavior cannot be attributed to mobility.

Critical Comments

- Virtues:
  - Very rich dataset.
  - Very good design (good use of fixed effects).
  - Large amount of variation: NYT undertook a expansion right before the period studied.
  - First-stage relationship is very credible.

Critical Comments

- Potential Flaws:
  - Measuring the difference between the targeted group and the non-targeted group without actually observing who are the clients of the NYT is problematic.
  - Correlation about effect of NYT and effect of other national media on voting behavior (not important for the conclusions).
  - Second Stage was not sufficiently documented.
  - Lack of a model to help interpreting the results
    - *Is this effect due to attention?*
    - *Why should we care about this effect?*

Is this Effect due to Attention?

- Alternative explanation: Opposite direction.
  - The NYT could have expanded in areas where people have more unattended demand for national information.
  - Therefore, people would be completely rational by stop reading local newspapers.
  - But then why would they change their voting behavior?

Why should we care about this effect?

- Huge debate on effect of national media on community-based activities.

- But: If individual is maximizing (alternative explanation), then maybe it's optimal to leave the local election to be decided only by people who care about local affairs.

- Again, difficult to conclude something without a model.

Suggestions for Future Research

- Look for other consequences in political behavior beside voter turnout due to the distraction effect of non-local media.

- Analyze the link between local elections and presidential elections in US over time.

- Analyze the effect of a biased national media in local elections (counterintuitive effect).

Notation:

- s_{zt} : per-capita local newspaper readership in zip code z in year t. h stands for high educated and l stands for low educated. Only s_{zt} is observable.
- NYT_{Mt}: per-capita NYT sales in the MSA M in year t.
- e_z : fraction highly educated in zip-code z.
- V_i : indicates whether the individual voted.
- ed_i : indicates whether the individual has a college degree.
- μ : unobservable: M stands for MSA; Y stands for Year; h stands for high educated and l stands for low educated.