

Does the *New York Times* Spread Ignorance and Apathy?

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Abstract

Technological advances over the past century have dramatically lowered the cost of transmitting information over large distances, allowing high quality national media such as the *New York Times* to enter local markets. However, consumers attracted to outside media may consume less local information and, as a result, disengage from local affairs. In the late 1990's, the *New York Times* implemented a national distribution strategy, establishing or expanding home delivery in more than 100 cities across the country. Using longitudinal data on local newspaper circulation, *Times* penetration, and voting, we find that as the *New York Times* becomes more widely circulated in a market, sales of local newspapers to individuals targeted by the *Times* declines. Moreover, as *Times* penetration increases, college educated individuals targeted by the *Times* become less likely to vote in local elections. Although some consumers benefit from availability of outside media in local markets, "distraction" from local affairs is a possible negative consequence of modern information technology.

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Introduction

Technological advances over the past century have dramatically lowered the cost of transmitting information over large distances. These innovations allow consumers to obtain news and information from distant, as opposed to local, sources and allow media firms to seek a national audience for their products. Aggregating consumers into a national market allows sellers to create better, more specialized products even if they appeal to few individuals in any particular geographic market. High quality specialized national media products made available by market enlargement make some consumers better off.

At the same time, availability of targeted national media may attract some consumers away from local media products. For example, individuals in Fargo with a preference for high quality national or international news may switch from the *Fargo Forum* to the *New York Times* if home delivery of the *Times* becomes available in that market. But in switching from a local to an outside product, these individuals may consume less local information and may disengage from local affairs. For example, the *Times* reader in Fargo, ignorant of local issues, might grow apathetic about local political contests and fail to vote. Although some consumers benefit from availability of outside media in local markets, “distraction” from local affairs is a possible negative consequence of modern information distribution technology.

The concern that outside media might distract individuals from local affairs is not new. In his well-known essay and book *Bowling Alone*, Robert Putnam argues that media-based entertainment diverts attention from community-based social activities such as bowling leagues.¹

¹ Putnam's work contributes to a large literature in media studies exploring the impact of media and media firms on social and cultural behavior. See, for example, Sampedro (1998) and Footer and Graber (2000).

Many governments subsidize domestic media with the goal of promoting local customs and local culture.²

Despite this concern, empirical evidence that media affect local behavior is scarce. This paper estimates whether outside media attract individuals away from local media products and depress local political engagement. The *New York Times* substantially expanded its national distribution in the mid-late 1990's, introducing or expanding home delivery in more than 100 cities between 1995 and 1998. With the *New York Times* as an example of an outside product targeted at highly educated readers, we use this expansion to ask: (1) does circulation of the *Times* outside of New York reduce local newspaper circulation among college-educated citizens?; and (2) does the *Times* depress local political participation among its readers? Using longitudinal data on zip code circulation of local newspapers, individual data on political participation in four elections held in even-numbered years, and MSA-level longitudinal data on *New York Times* circulation, we find that as the *Times* becomes more widely circulated in a market, circulation of local newspapers among targeted readers declines relative to circulation among consumers not targeted by the *Times*. Moreover, we find that targeted individuals are less likely to vote in local elections in areas with greater *Times* penetration. There is evidence that distraction is weaker in presidential contests, which are covered by the *Times*. Finally, mobility, which could explain both disinterest in local elections and interest in outside media, does not appear to drive the results.

Our results are relevant to a number of ongoing intellectual and policy debates. First, our research supports Putnam's argument that media play a role in social networks and can

² See, for example, annual reports of Telefilm Canada which emphasize the role of domestic media in cultural preservation. Reports are available at <http://www.telefilm.gc.ca/01/172.asp>, viewed May 31, 2002. Information regarding national policies toward cultural protection in media markets are available in the summary report of the 2000 UNESCO ministerial round table on cultural diversity, available at http://64.26.177.19/events/index_e.shtml, viewed May 31, 2002.

distract individuals from community-based activities. Second, our work demonstrates that media markets affect the political process. While a large literature considers the effect of media messages on political outcomes, there is little empirical evidence that media markets affect political behavior.³ Third, our work is relevant to the desirability of localism in media. A host of regulations, promulgated by the FCC and other bodies, seek to promote local content and local programming.⁴ These regulations are often criticized as essentially protectionist measures favoring small, local firms against larger national competitors. Yet our result that national media weaken interest in local affairs may provide a rationale for promotion of locally-tailored products.

The paper proceeds as follows. Section 1 describes how outside media affect engagement in local affairs. Section 2 describes the data used in the study and explains our focus on the *New York Times* in the empirical work. Section 3 discusses empirical strategy and examines the effect of *Times* penetration on local newspaper readership and local political participation. Section 4 concludes the paper.

I. How Do Outside Media Affect Engagement in Local Affairs?

Large markets tend to have more, more varied, and by many measures better products. This is particularly true in media markets.⁵ Any national media product has the potential to divert consumer attention from local social or community affairs. An individual watching a fishing channel on cable television cannot, for example, simultaneously attend a political rally,

³ A notable exception is Strömberg (1999), which documents that political strategy depends on voter access to mass media. See also Oberholzer-Gee and Waldfogel (2001), which documents that availability of black-targeted media affects black voter turnout.

⁴ We are thinking here of the FCC's "must-carry" rules requiring cable systems to carry local television channels (see <http://www.fcc.gov>) as well as newspaper regulations such as the Newspaper Preservation Act.

⁵ In newspaper markets, papers in large markets employ more reporters covering more topics than papers in smaller markets. The number of Pulitzer Prizes per daily newspaper reporter, 1980-1999, is also systematically higher in larger markets. See George and Waldfogel (2000). Variety is also greater in large radio markets. In the top 165 U.S. markets, an average of 12.5 different radio station formats are available in the largest market quintile while only 8.2 formats are available in the smallest. See Berry and Waldfogel (2001).

read a local newspaper, or actually go fishing. However, local newspapers are a particularly important source of local information and hence their circulation provides a good measure of citizens' intake of local information.⁶ As close substitutes for local newspapers, national newspapers are therefore likely to be an important source of distraction from local affairs.

Disengagement from local affairs may similarly manifest itself in multiple ways. An individual with limited knowledge of local issues may be less likely to attend public hearings or PTA meetings than individuals familiar with community events. However, information is most often linked to political participation. A considerable body of empirical work demonstrates that campaign activities that lower the cost of acquiring political information increase voter turnout (Wielhouwer and Lockerbie, 1994; Shachar and Nalebuff, 1999; Gerber and Green, 2000 and Gerber, Green, and Green, 2000). If lowering the cost of obtaining political information increases turnout, raising the difficulty of getting local political information to a group may therefore reduce the tendency to vote.⁷

1. Consequences of Distraction

This study is primarily concerned with the positive questions of whether outside media affect local media consumption and local political participation. These effects, if they operate,

⁶ There is empirical evidence that local media affect political knowledge. Mondak (1995a, 1995b) surveyed political interest in Cleveland and Pittsburgh during the 1992 strike which silenced the *Pittsburgh Press* and *Pittsburgh Post-Gazette* for eight months surrounding the 1992 election. Mondak shows that individuals in Pittsburgh spent relatively more time discussing national and statewide candidates than individuals in Cleveland, who experienced no disruption in newspaper access. Mondak also found that Pittsburgh residents considered themselves more informed about statewide relative to local issues than individuals in Cleveland. Although Mondak's results may partially reflect the short-term nature of the strike, they suggest that local newspapers affect local knowledge even in markets with abundant alternatives to local newspapers.

⁷ Outside media might also affect local product markets, since firms seeking a local audience would find it more costly to reach consumers that eschew local media. Evidence from the advertising literature indicates that local media affect what consumers learn about local product markets. For example, Glazer (1981) studies the effect of advertising on grocery prices using a local newspaper strike as an instrument for access to advertising. His results indicate that prices on advertised goods were higher in markets without a daily newspaper. Although Glazer's objective is to determine whether advertising affects prices, the mechanism through which the effect operates is local media.

do not necessarily reflect inefficiency. We would argue that these effects are of positive interest regardless of whether they are inefficient, but there are two possible inefficiencies one might link to them. First, a typical local newspaper has a substantial amount of local content while a national paper contains little such information. If consumers vary in the extent to which they like local and non-local coverage, when only a local paper is available, a range of types will purchase the product. When an outside paper enters the market, some consumers with preferences for national content will switch to the outside paper, and they are better off. However, these consumers may have some appetite for local content. The resulting allocation may be inefficient in the sense that the all-or-nothing nature of newspaper products allows more distraction of consumers than would arise with continuously adjustable content.⁸

Second, distraction may affect the distributional consequences, as well as possibly the efficiency, of voting outcomes. If the individuals distracted from voting are mostly “left of center,” for example, then voting outcomes may move to the right. The efficiency implications are more ambiguous. For example, one can construe voting as citizens’ internalization of their concern for policy effects on others. The reduction in internalization that comes with distraction may make voting a more or less efficient means of resource allocation.

II. Data

1. Sources

The analyses in section III consider the effect of *New York Times* penetration on local newspaper readership and local political participation among individuals targeted by the *Times*. The empirical work relies on three panel data sets constructed from longitudinal information on zipcode-level local newspaper circulation, MSA-level *New York Times* circulation, product-level

⁸ This recalls Peltzman’s (1973) mechanism whereby public school provision reduces consumption of education.

local newspaper content and individual voting data. The data sets and their sources are described below.⁹

1.a. Local Newspaper Circulation (Zipcode-Level Data)

We analyze the effect of *New York Times* penetration on local newspaper circulation using a zipcode-level panel data set on local newspaper circulation, in conjunction with an MSA-level panel on *New York Times* circulation and zipcode-level demographic data drawn from the 1990 Census. We have usable local newspaper circulation data for 8,990 zip codes in 1995 and 1998. We have MSA *New York Times* circulation in March 1995, 1997, and 1999.

All newspaper circulation data are from the Audit Bureau of Circulations (ABC). ABC is an association that audits circulation statements for use by advertisers. The ABC circulation figure measures sales for a particular day at each paper. Our zipcode-level circulation data are drawn from ABC's *Circulation Data Bank* which reports newspaper sales by zip code for about 1,000 daily newspapers throughout the country. Many newspapers did not report zip code circulation to ABC until 1996 or 1997, limiting the longitudinal sample to 637 newspapers that account for three quarters of newspaper circulation. While national newspapers such as the *New York Times*, *Wall Street Journal*, *USA Today* and *Christian Science Monitor* do not report zip code-level sales to ABC, the *New York Times* does report circulation by MSA. Because *Times* circulation is only available at the market level, we restrict attention to zip codes situated within MSA's. Eliminating markets for which *Times* circulation is unavailable and those within the New York consolidated metropolitan statistical area (CMSA) reduces the sample to a final size of 8,990 zip codes in 259 MSA's.

⁹ Because we seek to employ longitudinal estimation approaches, we have collected multiple time period observations for each of the variables of interest. Because the data are drawn from disparate sources, the timing is not precisely parallel among the data sets.

1.b. Local Political Participation (Individual Data)

We analyze the effect of *New York Times* penetration on voting using individual-level voting and demographic data drawn from the 1994, 1996, and 1998 Current Population Surveys (CPS), along with the MSA level *New York Times* circulation in March 1995, 1997, and 1999 described above.

The CPS data include voting information, the MSA of residence, and socioeconomic characteristics for about 80,000 eligible voters each year. Differences in MSA definitions between *New York Times* circulation data from ABC and individual data in the CPS reduces the usable sample to about 45,000 individuals each year. While we cannot link individuals over time, we treat CPS data as an MSA-level panel.

1.c. Local Newspaper Positioning (Product-Level Data)

We analyze the effect of *New York Times* penetration on local product positioning using newspaper-level panel data on the assignment of reporters and editors to topical reporting beats, again in conjunction with the *Times* MSA-level panel data. *Burrelle's Media Directory* maps the beats covered by more than 30,000 reporters and editors at U.S. daily newspapers to approximately 150 reporting topics. Beat data for 1993 and 1999 aggregated to 12 general areas are used to characterize newspaper targeting decisions at major papers in markets with different levels of *Times* penetration.

2. Sample Characteristics

Table 1 reports summary statistics for circulation, voting, and demographic data. There is considerable variation in *Times* circulation across markets, with penetration ranging from

0.005% at the 5th percentile to 0.95% at the 95th percentile.¹⁰ (The data measure *New York Times* circulation outside of the home market, hence the New York CMSA is not included in the table or analyses that follow). There is also considerable variation in local newspaper circulation across markets, with per capita sales ranging from 2% at the 5th percentile to 37% at the 95th percentile.¹¹ The fraction of individuals in each zip code with a college degree also varies considerably, ranging from about 5% to 50% across zip codes.

The table also summarizes voting and individual data from the CPS. The tendency to vote differs across education group and between presidential and non-presidential elections. In 1996, 58% of persons without a college degree voted, while 82% of college-educated persons voted. In 1994 and 1998, voter turnout was about 45% percent among those without a college degree and more than 65% among the college educated.

3. *Why the New York Times?*

We consider the effect of *New York Times* penetration on consumption of local newspapers and participation in local elections. In principle, any national newspaper such as *USA Today* or the *Wall Street Journal*, or large regional newspaper such as the *Washington Post*, might attract consumers away from local papers. We choose to study the *Times* for several reasons. First, we seek a close substitute for local newspapers. A general interest newspaper such as the *Times* is likely a better substitute for a local newspaper than a business newspaper such as the *Wall Street Journal* or *Financial Times*. Second, we seek a national product with high circulation and considerable variation in penetration across markets. Today the national

¹⁰ We define penetration as the ratio of circulation to population.

¹¹ Note that zip code circulation includes circulation of all local and regional papers sold in the zip code, hence per capita sales exceed one in some locations.

edition accounts for more than 40% of total circulation.¹² National circulation of regional newspapers such as the *Los Angeles Times* or the *Washington Post* is far smaller than national *New York Times* circulation.¹³

Third, it is useful to study a product that appeals to a distinct audience. Targeting allows the effects of outside media on local media consumption and local political participation to be identified from differences between the target and non-target group. The *New York Times* targets readers with tastes for cultural and international coverage, a set of preferences for which education seems a reasonable proxy. Company annual reports, investor presentations, and press releases provide substantial evidence of this targeting.¹⁴ County-level circulation data for the *New York Times*, *New York Post* and *New York Daily News* in counties close to New York City provides additional empirical evidence. The share of *Times* circulation to total circulation for the three New York dailies increases substantially with the fraction of individuals in a county with a college degree.¹⁵ In contrast, *USA Today* appeals to a less distinct audience.¹⁶

¹² See annual reports posted at the *New York Times* web site, www.nytc.com.

¹³ For example, data available from the Audit Bureau of Circulations show circulation of the *Washington Post* outside of the Washington, DC MSA to be about 10% of total circulation in 1998. Circulation of the *LA Times* outside of California represents less than a percent of the paper's total circulation.

¹⁴ Information concerning *New York Times* targeting is available in company annual reports, investor presentations, and press releases posted at www.nytc.com. For example, the 1998 annual report cites a strategic objective of producing a paper that “Provides advertisers with a well-educated, affluent audience of loyal and involved readers.” Similarly, the President of the *New York Times* describes in the company mid-year review the success of the paper’s “Expect the World” campaign in “attracting increasing numbers of affluent, well-educated readers nationwide.”

¹⁵ The *New York Times* reports county-level circulation for 30 counties in Connecticut, New York and New Jersey. Linking *Times* circulation data to county-level census data allows us to estimate the relationship between the fraction of county residents with a college degree and the share of newspaper sales held by the *Times*.

¹⁶ The ability of a national newspaper to distract targeted readers from local media depends on the positioning of the national product *relative to local media*. While the *New York Times* appears to target the most highly educated individuals in *every* market, the *USA Today* likely appeals to highly educated readers in some markets and less highly educated readers in other markets, depending on the style and content of competing local newspapers. Circulation data available from ABC confirm that circulation of *USA Today* is positively correlated with the fraction of individuals in a market with a college degree, but the correlation is much weaker than for the *New York Times*.

Finally, and perhaps most important, the *Times* undertook a strategy of national circulation expansion during the period we study. Although the *Times* had substantial circulation outside of the New York area in 1994, they accelerated their national distribution efforts shortly thereafter. In August 1996, the *Times* vice president for circulation describes a “strategy to broaden the distribution of *The Times* outside the New York metropolitan area,” adding “we’ve increased the availability of *The Times* to more households through home delivery and to more individuals through retail outlets -- and we continue to do so.”¹⁷ Investor documents note that “during 1996, *The Times* entered into agreements with approximately 30 newspapers and other delivery agents located in the United States and Canada to deliver *The Times* in their respective markets and, in some cases, to expand current markets.”¹⁸ In 1997, the *Times* “inaugurated its first-ever national image advertising campaign with the brand promise “Expect the World.”¹⁹ That year the company stated that “the “paper’s national initiative gained momentum in 1997 as *The Times* entered into a series of third-party home delivery partnerships, covering an additional 57 markets nationwide. At the end of last year, home delivery was available in a total of 115 markets across the country.”²⁰

In principle one might use the features of the expansion strategy, either printing plant openings or the announcement of new home delivery areas, to construct instruments for *Times* penetration. Data limitations prevent us from making explicit use of either supply shift. First, the New York Times Company does not report a comprehensive list of home delivery markets,

¹⁷ *PR Newswire*, “The *New York Times* Expands National Circulation Through Chicago Tribune, Denver Post,” New York, August 12, 1996. A more comprehensive description of the expansion strategy can be found in Iver Peterson, “*Times* Expanding Nationwide Distribution,” *New York Times*, January 2, 1997, Section D, p. 1.

¹⁸ New York Times Company, Form 10-K, December 29, 1996, p. 6.

¹⁹ *New York Times Company Annual Report, 1997*, available at www.nytc.com, accessed May 30, 2002. Additional information on the national advertising campaign can be found in *PR Newswire*, “The *New York Times* Launches National Image Campaign as Part of Expansion Strategy,” New York, January 28, 1997.

²⁰ *New York Times Company Annual Report, 1997*, available at www.nytc.com, accessed May 30, 2002.

and so we cannot identify markets with home delivery at the start of our sample period. Second, while company annual reports identify the locations of new printing sites, sites that open during our sample period are located in markets previously served by other printing locations. New markets served by the new printing sites are not systematically reported. Finally, new home delivery markets are reported only in a fragmentary way. Press releases in some cases identify only larger expansion cities, and expansion often occurs in MSA's with some existing home delivery service.

While we cannot use the information on expanded distribution literally to construct instruments, we take the *Times* national expansion strategy as a broad source of variation in *Times* penetration that is exogenous to interest in local products and local affairs. Table 2 shows circulation growth by Census region along with a fragmentary measure of the supply expansion, the number of markets adding home delivery in 1997.²¹ The table clearly shows that regions adding more home delivery sites experienced greater circulation growth. Particularly in the context of the *Times*'s stated strategy, this growth in circulation provides evidence of a supply-driven expansion.

The data described above provide the basis for answering the two primary questions of the paper: (1) Does the *New York Times* differentially attract targeted readers away from local newspapers? and (2) Does the *New York Times* differentially reduce voting among targeted readers in local elections relative to non-targeted readers? The remainder of the paper seeks answers to these questions.

²¹ Expansion markets are identified from company press releases and a small map in the *New York Times Company Annual Report, 1997*. We assign points on the published map to states or Census regions.

III. Empirical Strategy and Results

1. Empirical Strategy

We employ both cross sectional and longitudinal empirical strategies for measuring the effects of *New York Times* circulation in local markets on local newspaper sales and local political participation. The simplest cross sectional approach to documenting the effect of *Times* penetration on local behavior would be to ask whether educated persons are less likely to buy their local papers in markets with higher *New York Times* penetration. Yet, the tendency for target audiences to read local papers may be determined by factors related to local *New York Times* penetration, such as the quality of other local media, casting doubt on a causal interpretation of the correlation between *New York Times* penetration and the tendency of the target audience to vote or read the local paper. However, if unobserved determinants of relevant local behavior are constant across the target and non-target audience, then the inclusion of market fixed effects allows us to identify the effect of the *New York Times* from the relationship between *Times* penetration in a market and the tendency for the target audiences to buy a local newspaper or vote relative to the non-target group. Hence, we estimate many of the regressions below using market fixed effects.

It is possible that the fixed, unobservable determinants of local behavior are not common across the target and non-target audience. Using data on *New York Times* sales, local newspaper circulation and voting for various years, we can also estimate longitudinal empirical models that allow for group-specific MSA fixed effects. We identify the effects of *Times* penetration on local behavior from the relationship between *changes* in *New York Times* penetration and *changes* in local newspaper sales and political participation for the target relative to the non-target group. Unlike the cross sectional approach, this method produces valid inference even if

the unobserved determinants of local newspaper readership and voting are not common across the target and non-target groups.

2. *New York Times* Penetration and Local Newspaper Circulation

Implementing the empirical strategies above to estimate whether the *Times* differentially attracts targeted readers would be straightforward with data on per capita local newspaper sales by education level in each zip code. With readership data by education level, it would be possible to determine the effect of the *Times* on each group, estimating:

$$(1) \quad s_{zt}^{high-ed} = \mathbf{a}_0^h + \mathbf{a}_1^h NYT_{Mt} + \mathbf{m}_M^h + \mathbf{e}_{zt}^h$$

and

$$(2) \quad s_{zt}^{low-ed} = \mathbf{a}_0^l + \mathbf{a}_1^l NYT_{Mt} + \mathbf{m}_M^l + \mathbf{e}_{zt}^l$$

where $s_{zt}^{high-ed}$ and s_{zt}^{low-ed} are per-capita local newspaper readership in the zip code among target and non-target readers at time t , NYT_{Mt} measures per capita *New York Times* sales in the MSA in year t , \mathbf{m}_M^h and \mathbf{m}_M^l are group-specific time-constant MSA-level unobservables, and \mathbf{e}_{zt}^h and \mathbf{e}_{zt}^l are time and group-specific errors. The constants \mathbf{a}_0^h and \mathbf{a}_0^l show baseline local newspaper readership for each education group and the coefficients \mathbf{a}_1^h and \mathbf{a}_1^l reflect the effect of *Times* penetration on local newspaper readership by group. Direct comparison of \mathbf{a}_1^h and \mathbf{a}_1^l would reveal any difference in the effect of *Times* penetration on highly educated individuals relative to those with less education.

Unfortunately, readership data are not available by education or other category that might reasonably proxy for *New York Times* targeting. However, we do observe the fraction of

individuals with a college degree by zip code. We can use variation in the fraction highly educated across zip codes to infer the effect of *Times* penetration in a market on targeted relative to non-targeted readers. To do this, we define e_z as the fraction highly educated in the zip code and note that:

$$(3) \quad s_{zt} = e_z * (s_{zt}^{high-ed}) + (1 - e_z) * (s_{zt}^{low-ed}).$$

Then substitution of equations 1 and 2 into 3 produces:

$$(4) \quad s_{zt} = \mathbf{b}_0 + \mathbf{b}_1 NYT_{Mt} + \mathbf{b}_2 e_z + \mathbf{b}_3 e_z * NYT_{Mt} + \mathbf{n}_{zt},$$

where the error $\mathbf{n}_{zt} = \mathbf{m}_M^h + \mathbf{e}_{zt}^l + e_z * (\mathbf{m}_M^h - \mathbf{m}_M^l) + e_z * (\mathbf{e}_{zt}^h - \mathbf{e}_{zt}^l)$. If the market effects are common across groups, then the time-constant unobservable effects, \mathbf{m}_M^h and \mathbf{m}_M^l , simplify to a single MSA effect. In this case, the model can be estimated with an MSA fixed effect using data for a single time period, and the impact of *Times* circulation on local behavior is identified from variation in *Times* penetration the across MSA's. With a market fixed effect, the impact of *Times* circulation on local newspaper readership by group, \mathbf{a}_1^h and \mathbf{a}_1^l , cannot be estimated separately. However, the effect of interest, the difference between the target and non-target effect, $\mathbf{b}_3 = (\mathbf{a}_1^h - \mathbf{a}_1^l)$, is identified in the model. A negative coefficient estimate for \mathbf{b}_3 would indicate that *Times* penetration reduces readership among highly educated readers relative to less highly educated readers. This the first of our two basic measurement approaches.

The first two columns of table 3 show results from the cross sectional model estimated in 1995 and 1998 with MSA fixed effects. The gap between the tendency for college-educated persons and others to read a local paper is about 19 percentage points. The coefficient of greatest interest, the interaction between the fraction of college educated individuals in the zip code and

New York Times penetration in the market (\mathbf{b}_3), is negative in both columns. This indicates that the gap in the tendency to read a local newspaper among those with and without a college degree is smaller in markets with greater *Times* penetration. The magnitude of the estimate suggests that a one copy increase in *Times* circulation in a market reduces local newspaper sales among college-educated readers by about 5.

How large is this estimated effect? A one-copy per capita increase in *Times* penetration reduces local newspaper purchases by college educated persons by 5 copies per college educated person. Individuals with a college degree make up one fifth of the population across zipcodes in our sample. If all *Times* readers are college-educated, then a single per capita copy represents 5 copies *per college educated person*. Then the coefficient estimate of -5 indicates roughly one-for-one displacement of local paper sales by *Times* sales. However, if the pattern of *Times* penetration is correlated with penetration of other national media targeted at educated persons, then the *Times* coefficient in these regressions and others that follow will reflect the influence of both the *Times* and these other media. We return to this point below.

Using longitudinal data allows calculation of the effect of *Times* penetration on local newspaper circulation on both the target (\mathbf{a}_1^h) and non-target (\mathbf{a}_1^l), groups directly, rather than just the difference, \mathbf{b}_3 . Longitudinal data also allows the model to be estimated allowing for group-specific time-constant unobservables ($\mathbf{m}_M^h \neq \mathbf{m}_M^l$). This is implemented by including both an MSA fixed effect and an MSA-specific coefficient on e_z . With this strategy, the effect of *Times* penetration on local behavior is identified from the relationship between the change in *Times* penetration and the changes in local newspaper readership among the target relative to the non-target group.

The third column of table 3 estimates the model using data from 1995 and 1998, with an education-specific MSA fixed effect. The estimate for \mathbf{b}_1 shows the effect of *Times* penetration on the non-target group to be close to zero. The estimate of -10.9 for \mathbf{b}_3 is negative and significant, indicating that *Times* penetration reduces readership among the target relative to the non-target audience. The longitudinal estimate is roughly double the cross-sectional estimate.

Results in table 3 suggest that the *New York Times* draws targeted readers away from local papers. One way of confirming this result is to consider whether local newspapers reposition products in response to competition from the *Times*. Since the *Times* targets readers with preferences for non-local content, we might expect local newspaper publishers to replace content heavily represented in the *Times*, such as national news or arts coverage, with content less readily available in the *Times*, such as local news or local sports coverage.

Data available from *Burrelle's Media Directory* illustrate how *Times* penetration affects local newspaper positioning. Burrelle's identifies the assignment of reporters and editors at daily newspapers to about 150 specific beats in 1993 and 1999. We restrict attention to the largest paper in each MSA, which are likely the *Times'* closest competitors. We aggregate the beats to 12 categories and examine how the fraction of reporters and editors in each category (beat shares) varies with *New York Times* sales.²²

The first column of table 4 shows the average fraction of reporters and editors in each of the 12 beat categories. The second and third columns report cross sectional regressions of beat shares in each category on *New York Times* penetration in 1993 and 1999, respectively. The final column uses the panel to estimate fixed effects regressions, identifying the relationships from changes in *Times* penetration and changes in the fraction of reporters assigned to each area.

²² Details on the nature of beat assignments at daily newspapers can be found in George (2000).

While only two of the cross sectional coefficients in table 4 are significant, the fixed effects specifications in column (4) show a number of interesting results. Changes in local news coverage are positively related to changes in *New York Times* penetration while changes in national and international coverage are negatively related to changes in *Times* penetration, suggesting that newspapers facing increased competition from the *Times* increase their emphasis on topics not covered by the *Times* and de-emphasize topics extensively covered by the *Times*. Growth in opinion and special issues coverage, both heavily covered by the *Times*, are also negatively related to changes in *Times* penetration. One might suspect reverse causality, that the *Times* enters local markets with little national coverage. Yet, the absence of cross sectional results indicates this is not the case. Repositioning of local newspapers appears to respond to growth in *Times* circulation in a market.

3. New York Times Penetration and Local Political Participation

3.a Local Elections

The results above show that the *Times* attracts college-educated readers away from local newspapers. We now consider whether the *Times* depresses political participation among these individuals. Our strategies for documenting the effect of *Times* penetration on voting are similar to our approaches for documenting effects on local newspaper readership. The task is made easier, however, by the availability of individual-level data. Because the *Times* includes substantial national but little local political coverage outside of New York, we expect the *Times* to depress political participation in local elections. Because the CPS reports voting data only for even-numbered years, we focus initially on 1994 and 1998, years without a presidential election. We then examine data on the 1996 presidential election.

As above, we first examine the effect of *New York Times* penetration on the probability that an individual votes using cross sectional data with MSA fixed effects, estimating:

$$(5) \quad V_i = \mathbf{g}_0 + \mathbf{g}_1 NYT_M + \mathbf{g}_2 ed_i + \mathbf{g}_3 NYT_{Mi} * ed_i + \mathbf{m}_M + \mathbf{e}_i,$$

where V_i is whether the individual voted, ed_i indicates whether the individual has a college degree, \mathbf{m}_M is an MSA-specific time-constant effect, and \mathbf{e}_i is an individual error. The effect of *Times* penetration, \mathbf{g}_3 is identified by the differential effect of *Times* penetration on the voting tendencies of the target relative to the non-target group. Pooling the two years of data allows us to estimate \mathbf{g}_1 . The approach in equation 5 is justified under the assumption that both the target and non-target groups have the same unobservable effect \mathbf{m}_M . We estimate linear probability models and cluster standard errors by MSA.

The first two columns of table 5 report coefficient estimates separately for 1994 and 1998. The third and fourth columns pool the two years of data with MSA and MSA x Year fixed effects. Column (4), which allows each year its own set of MSA fixed effects, is the analogue of the separate year regressions in columns (1) and (2). The column (3) regression with time-constant MSA fixed effects gives very similar results. In each specification, \mathbf{g}_2 is positive and significant, indicating that college-educated persons are about 15% percentage points more likely than others to vote. Estimates for \mathbf{g}_3 are negative and significant, indicating that higher *Times* penetration reduces the gap in the tendency to vote between individuals with a college degree and those without. Increasing *Times* circulation by one standard deviation (about 0.006 papers per capita) reduces the tendency to vote among college educated individuals by 0.006×4 , 2.4 percentage points, with a 95% confidence interval ranging from about 1.1 to 3.7 percentage points. Moving from the 5th percentile to the 95th percentile (from 0.005% to 1.0%) in *Times* penetration depresses voting by 0.995×4 or about 4 percentage points. Viewed another way,

increasing *Times* circulation by one newspaper per capita reduces political participation among college educated individuals by about 4 votes per capita. Since college-educated persons represent about one fifth of the population, an increase in overall per capita circulation of one newspaper is equivalent to an increase of 5 papers among the target group. Hence according to these estimates, *Times* readers displace voters roughly one-for-one.

A second approach, possible here with individual data, is to estimate a group-specific longitudinal model over two non-presidential election years, such as:

$$(6) \quad V_{it} = \mathbf{g}_0 + \mathbf{g}_1 NYT_{Mt} + \mathbf{f}_t + \mathbf{m}_M + \mathbf{e}_{it} .$$

One can interpret the coefficient \mathbf{g}_1 as the effect of *Times* penetration on each group. If one thinks that voting would have changed in ways correlated with the change in *New York Times* circulation, then one can measure the effect of *Times* penetration as the difference between target and non-target \mathbf{g}_1 . We accomplish this by pooling the education groups to estimate:

$$(7) \quad V_{it} = \mathbf{g}_0 + \mathbf{g}_1 NYT_{Mt} + \mathbf{g}_2 ed_{it} + \mathbf{g}_3 NYT_{Mt} * ed_{it} + \mathbf{f}_t + \mathbf{f}_t' ed_{it} + \mathbf{m}_M + \mathbf{m}'_M ed_{it} + \mathbf{e}_{it} .$$

Here, \mathbf{g}_3 measures the effect of *Times* penetration on political participation among the target relative to the non-target audience in non-presidential election years. Results are shown in column (5) of table 5. The effect of *Times* penetration on voting among the target group (beyond its effect on the non-target group) is -22 and significant. According to the longitudinal estimates, an increase in per capita *Times* circulation of one standard deviation (0.006) reduces the tendency of college educated citizens to vote by 22×0.006 , 13.2 percentage points, with a 95% confidence interval ranging between 3.6 and 22.8 percentage points. Moving from the 5th to 95th percentile depresses voting by about 22 percentage points. Each additional copy of the *Times* sold in an MSA reduces political participation among individuals with a college degree by

4-5 votes. The estimate of distraction is about five times larger with group-specific fixed effects than specifications that assume the unobserved \mathbf{m}_M is the same across groups.

There are three points to make about the magnitude of this coefficient estimate. First, the number of readers per newspaper sold is roughly 3. While local newspapers sales average only about 18% per capita, surveys indicate that about 55% of persons read daily newspapers.²³ Hence, one-for-one displacement is by no means the maximum possible. Second, our *Times* coefficients may reflect both the effect the *Times* and the effects of other media correlated with the pattern of *Times* penetration. Finally, our estimate is not terribly precise.

3.b Presidential Elections

Because the *New York Times* covers presidential but not local elections, an important implication of our analysis is that the *Times* depress political participation less in presidential election years than in other years. To see whether this is the case, we first estimate equation 5 for 1996, a presidential election year. The first column of table 6 shows the effect of the *Times* on voting among the target relative to the non-target group, \mathbf{g} , to be zero and insignificant. The result suggests that the distracting effect of the *Times* is lower in 1996 than in 1994 or 1998. To test this, we pool data for all three years and allow the *NYT*, *ed* and *NYT*ed* variables to have different coefficients in the non-presidential and presidential election years. Columns (2) and (3) of table 6 present the pooled specification with MSA and MSA x Year fixed effects, respectively. The estimated *Times* distraction effect is substantially smaller in 1996, although the significance levels are somewhat low. Column (4) allows the MSA unobservables to vary across the target and non-target groups. Here again, distraction is lower in the presidential year, although the result is not significant.

²³ See Newspaper Association of America News Release “Newspaper Readership Demonstrates Steady Gains,” May 1, 2002 available at www.naa.org, accessed on May 30, 2002.

3.c. Robustness

We take our results to indicate that the spread of the *New York Times* causes disengagement from the local community. It is possible, however, that causality is reversed: the *Times* expands in markets where growing populations of college educated individuals are becoming less engaged in local affairs relative to the remaining population.²⁴ Since there is evidence that length of residence in an area affects voter turnout (Squire, Wolfinger, and Glass, 1987), we examine the possibility that tenure explains both *Times* circulation and disinterest in voting.

The CPS asks individuals how long they have lived at their current address, and we can reestimate our model with education-specific controls for length of residence. If mobility explains both disinterest in voting and the appeal of the *Times*, then the residency variables should absorb the effects we attribute above to *Times* penetration. We estimate both the cross-sectional and pooled specifications with controls for length of residency. We also allow residency effects to vary by group. Table 7 presents results. In all of the specifications, the residency variables are positive and virtually all significant. Coefficients measuring the effect of *Times* penetration on participation are very similar to estimates in table 5 and table 6, although the standard errors for the pooled estimates are higher. Overall, the results indicate that, controlling for length of residence, the *Times* depresses voting among highly educated individuals relative to less educated individuals. The effect is less in presidential than in local elections.²⁵ While length of tenure is a very important determinant of voting and a plausible determinant of interest in the *Times*, our results are apparently not attributable to mobility.²⁶

²⁴ A large literature documents that length of residence in an area has a large effect on voter turnout. See Squire, Wolfinger, and Glass, 1987.

²⁵ We also confirm that mobility does not drive the effect of *Times* penetration on local newspaper sales. The census asks individuals whether they lived in the same house and same county in 1985 as in 1990. Including

IV. Conclusion

The results above show that the presence of the *New York Times* in local markets reduces per capita circulation of local newspapers among highly educated readers targeted by the *Times*. Moreover, as *Times* penetration increases, these readers are less likely to vote in local elections. The distracting effect of the *Times* is less in presidential races, which are covered heavily by the *Times*.

In demonstrating that national media consumption can affect local political participation, the results provide empirical support for popular arguments that media affect engagement in local communities. However a few caveats is in order. This study examines how outside media affect consumption of a single medium, daily newspapers. It is possible that individuals choosing a national newspaper over a local daily increase their intake of local content through other media, such as local television news or local weekly newspapers. Similarly, political organizations using local newspapers to reach a particular audience may find alternative ways of reaching the distracted audience. Yet, the parallels we document between distracting effects of *Times* penetration on voting and on local newspaper consumption suggest that *Times* distraction is not undone through other media.

We also examine only the distracting effect of the *New York Times*. Distraction by other non-local media may compound the effect measured here. Indeed, it would be interesting to examine the impact of television on engagement in local affairs, say between 1950 and 1980.

the fraction of individuals in a zip code living in the same house and same county from 1985 to 1990 in the readership regressions shows that while length of residence is correlated with local newspaper readership, including these variables in the regressions has little effect on coefficients of interest.

²⁶ We also consider whether differential trends in home ownership among the target relative to the non-target audience influence our results. The CPS recorded whether individuals own or rent their homes in 1994-1998. While renters are always less likely to vote, we cannot reject the hypothesis that the effect of *Times* penetration is the same for owners and renters.

The effect of the *New York Times* on its target audience may be small compared with the overall distraction of non-local media generally.

Is the distraction identified in this study a problem? Certainly individuals who opt for outside over local media are better off in their capacity as media consumers. Consumers who switch to national media taking into account ensuing distraction are also better off overall. Even if newspaper markets present consumers with an inefficient an all-or-nothing choice between local and non-local papers, competition from outside media may improve on an initial situation with too *little* distraction. In the strictest sense, then, the only inefficiency associated with distraction concerns the failure of individuals to take into account how their voting decisions affects others.

But more broadly, political and scholarly debate over the virtue of localism reveals deep concern that media from large markets suppress diversity and foster disengagement from local affairs.²⁷ This study offers empirical evidence that local media play a role in local political participation and that this role is undermined for some citizens by outside media. Yet, while our results show that targeted consumers are distracted from local affairs, we also document that local products grow more local when exposed to competition from the *Times*. Hence, limiting the flow of outside media into local markets to promote local engagement may be counter-productive

²⁷ See Stephen Labaton, "The Media Business; Broadcasters Ask Senate To Let F.C.C. Loosen Rules" *New York Times*, July 18, 2001, Section C, p 4. According to the *New York Times* account of Senate Commerce Committee hearings on relaxing media cross ownership rules, Senator Ernest Hollings "said the rules had already been relaxed too much and what remained of them was central to preserving local programming and diverse array of voices on the air." For international examples, see UNESCO report of the November 2000 roundtable on Cultural Diversity (available at http://64.26.177.19/events/index_e.shtml, viewed May 31, 2002.) Arguing that "respect for cultural diversity hinges on respect for the individual", the French culture minister advocates strong trade barriers against US media.

Results presented here suggest a number of directions for further research. The distracting effect of non-local media may affect not just voter turnout but other political behavior that depends on local media and local information. Information has been linked, for example, to party loyalty (Jones and Hudson, 1998). As the cost of informing particular groups of citizens about individual candidates increases, members of these groups may be more likely to vote along party lines. Media markets may also affect political outcomes. If incumbents have lower-cost alternatives to newspapers for communicating policy positions, the loss of readers to outside media may affect the relative balance between incumbents and challengers.²⁸ Finally, if politicians draft policy platforms to appeal to individuals easily reached through mass media, media markets may affect substantive policy outcomes as well as participation.²⁹

The distraction of outside media may have consequences for product markets as well as politics. Local advertisers cannot easily reach persons who do not consume local media products. Detachment of consumers from local information sources may therefore contribute to the spread of national firms. Consumers disengagement from local markets may also increase or decline with new information technologies. The Internet reduces the cost of distributing information, and its widespread adoption may affect incentives to participate in local affairs.³⁰ Our results suggest the promise of these and other avenues of research.

²⁸ An extensive literature documents that campaign spending by challengers has a larger effect on outcomes than spending by incumbents. See in particular Jacobson (1978, 1985) and Ansolabehere and Gerber (1994). A notable exception is Levitt (1995). One interpretation of this finding is that incumbents have more channels through which to inform citizens about both policy platforms and expected performance than do challengers. See Fiorina (1977).

²⁹ Strömberg (1999) uses data on radio penetration in the US during the Depression to study whether regions with higher radio ownership attracted a larger share of relief funds. The effect of radio ownership turns out to be quite large, with a 1% increase in radio ownership increasing public relief spending by 0.62%.

³⁰ The ability of the Internet to distract individuals from local affairs depends to some degree on whether local or non-local content predominates on the web. Sinai and Waldfogel (2000) examine the availability of local content on the Internet.

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Table 1: Sample Statistics

| | N | Mean | SD | 5% | 25% | 50% | 75% | 95% |
|--|-------|--------|--------|---------|--------|--------|--------|--------|
| <i>Newspaper Readership (ABC)</i> | | | | | | | | |
| MSA Per Capita NYT Sales (1995) | 259 | 0.0027 | 0.0065 | 0.00005 | 0.0003 | 0.0009 | 0.0026 | 0.0095 |
| MSA Per Capita NYT Sales (1999) | 259 | 0.0028 | 0.0057 | 0.00007 | 0.0004 | 0.0010 | 0.0028 | 0.0095 |
| Zip Per Capita Local Newspaper Sales (1995) | 8,990 | 0.1880 | 0.1106 | 0.02305 | 0.1098 | 0.1809 | 0.2498 | 0.3765 |
| Zip Per Capita Local Newspaper Sales (1998) | 8,990 | 0.1838 | 0.1102 | 0.02132 | 0.1072 | 0.1751 | 0.2420 | 0.3737 |
| <i>Zip Code Demographics (Census)</i> | | | | | | | | |
| Zip Fraction College Educated | 8,990 | 0.1961 | 0.1355 | 0.0496 | 0.0985 | 0.1552 | 0.2577 | 0.4820 |
| Zip Fraction White | 8,990 | 0.8645 | 0.1935 | 0.4099 | 0.8296 | 0.9464 | 0.9840 | 0.9984 |
| Zip Fraction Black Pop | 8,990 | 0.0828 | 0.1668 | 0.0000 | 0.0026 | 0.0149 | 0.0727 | 0.4542 |
| Zip Fraction Asian Pop | 8,990 | 0.0201 | 0.0431 | 0.0000 | 0.0014 | 0.0065 | 0.0190 | 0.0847 |
| Zip Fraction Indian Pop | 8,990 | 0.0066 | 0.0302 | 0.0000 | 0.0007 | 0.0025 | 0.0059 | 0.0183 |
| Zip Fraction Other Pop | 8,990 | 0.0261 | 0.0684 | 0.0000 | 0.0004 | 0.0035 | 0.0153 | 0.1446 |
| Zip Fraction Young (<30) | 8,990 | 0.4388 | 0.0812 | 0.3228 | 0.3973 | 0.4370 | 0.4756 | 0.5605 |
| Zip Fraction Old (=65) | 8,990 | 0.1661 | 0.0715 | 0.0701 | 0.1229 | 0.1584 | 0.2001 | 0.2811 |
| Zip Median Income (\$1,000) | 8,990 | 33.25 | 12.13 | 17.27 | 25.35 | 31.54 | 38.96 | 54.61 |

| | 1994 (N=45,456) | | 1996 (N=43,769) | | 1998 (N=42,564) | |
|--|--------------------|-----------|--------------------|-----------|--------------------|-----------|
| <i>Individual Data(CPS)</i> | <i>Mean</i> | <i>SD</i> | <i>Mean</i> | <i>SD</i> | <i>Mean</i> | <i>SD</i> |
| Voting Probability (All) | 0.525 | 0.499 | 0.642 | 0.479 | 0.506 | 0.500 |
| Voting Probability (No College Degree) | 0.469 | 0.499 | 0.581 | 0.493 | 0.448 | 0.497 |
| Voting Probability (College Degree) | 0.704 | 0.457 | 0.821 | 0.384 | 0.666 | 0.472 |
| Fraction College Degree | 0.239 | 0.427 | 0.254 | 0.436 | 0.265 | 0.441 |
| Fraction Black | 0.119 | 0.323 | 0.108 | 0.310 | 0.108 | 0.310 |
| Fraction Asian | 0.025 | 0.155 | 0.029 | 0.168 | 0.032 | 0.175 |
| Fraction Indian | 0.007 | 0.083 | 0.008 | 0.087 | 0.009 | 0.092 |
| Fraction Hispanic | 0.051 | 0.220 | 0.054 | 0.227 | 0.065 | 0.247 |
| Fraction Female | 0.462 | 0.499 | 0.465 | 0.499 | 0.467 | 0.499 |
| Fraction Under 30 | 0.216 | 0.412 | 0.212 | 0.409 | 0.204 | 0.403 |
| Fraction Over 64 | 0.168 | 0.374 | 0.160 | 0.366 | 0.163 | 0.369 |

Notes: New York CMSA excluded from all estimates.

Table 2: Home Delivery Expansion and *New York Times* Circulation Growth, 1994-1998

| Region | 1994 <i>New York Times</i> Sales per 1,000 1990 Census Population | <i>New York Times</i> Sales, %Growth 1994-1998 | Home Delivery Markets Added 1997 |
|--------------------|---|---|-------------------------------------|
| Mountain | 1.97 | 29.58% | 10 |
| Pacific | 1.61 | 15.89% | 10 |
| East North Central | 1.61 | 4.96% | 11 |
| New England | 11.85 | 4.60% | 3 |
| South Atlantic | 2.49 | 4.06% | 12 |
| East South Central | 0.71 | 2.31% | 1 |
| West South Central | 0.83 | 2.11% | 1 |
| West North Central | 0.77 | 1.76% | 5 |
| New York MSA | 55.17 | -6.24% | 0 |
| Middle Atlantic | 9.82 | -6.80% | 4 |

Total

Notes: 1994 circulation per 1,000 reflects the average across MSA's in the region. Regions are the 9 Census regions, except that circulation in the New York MSA is separated from circulation in the rest of the Middle Atlantic region. New home delivery markets added in 1997 reflect our interpretation of a map published in *New York Times Company Annual Report, 1997*.

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

| | <i>Cross Sectional Results</i> | | <i>Longitudinal Results</i> |
|---|--|--|--|
| | 1995 (1) | 1998 (2) | 1995-1998 (3) |
| Per Capita NYT (β_1) | | | 1.5572 (1.95) |
| <i>Zip Fraction High Ed (b_2)</i> | <i>0.1926</i> <i>(6.57)**</i> | <i>0.1875</i> <i>(7.04)**</i> | <i>0.1628</i> <i>(16.80)**</i> |
| 1998 Year Dummy | | | -0.0117 (9.53)** |
| Zip High Ed * 1998 | | | 0.0105 (3.01)** |
| <i>Zip Fraction High Ed * NYT (b_3)</i> | <i>-4.6355</i> <i>(1.64)</i> | <i>-5.2955</i> <i>(2.03)*</i> | <i>-10.9005</i> <i>(4.31)**</i> |
| Zip Fraction Black | -0.0664 (7.01)** | -0.0614 (7.55)** | -0.0574 (13.03)** |
| Zip Fraction Asian | -0.1129 (3.07)** | -0.1418 (4.07)** | -0.1032 (4.30)** |
| Zip Fraction Native American | -0.0978 (1.61) | -0.0890 (1.79) | -0.0867 (4.41)** |
| Zip Fraction Other Race | 0.0001 (0.00) | 0.0079 (0.12) | -0.0994 (7.62)** |
| Zip Fraction Age \geq 65 | 0.2373 (4.91)** | 0.1299 (2.68)** | 0.1895 (5.23)** |
| Zip Fraction Age $<$ 30 | -0.1752 (2.99)** | -0.2330 (4.39)** | -0.2007 (7.93)** |
| Zip Median Income (\$1,000) | 0.0002 (0.61) | 0.0005 (1.59) | 0.0003 (2.45)* |
| Constant | 0.1912 (4.62)** | 0.2180 (5.74)** | 0.2061 (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.

Table 4: Do Local Newspapers Respond to *New York Times* Penetration?

| Beat Category | Mean | 1993 | 1999 | 1993-1999 |
|-------------------------|-------|--------|----------|-----------|
| Art & Literature | 0.023 | 0.193 | 0.130 | 1.942* |
| Business | 0.140 | -0.018 | -1.419 | -3.199 |
| Entertainment | 0.122 | 0.845 | 1.046 | 0.402 |
| Home | 0.027 | -0.626 | -0.309 | -0.833 |
| Local News | 0.181 | -0.410 | 1.572 | 14.169*** |
| National & Foreign News | 0.088 | -0.001 | 0.466 | -6.107** |
| Opinion | 0.063 | 0.185 | 0.783 | -3.798* |
| Science & Technology | 0.025 | 0.765* | 0.119 | 0.566 |
| Special Issues | 0.119 | -1.694 | -1.209 | -5.249* |
| Sports | 0.076 | -0.438 | 0.746 | 2.029 |
| Style | 0.040 | 0.869 | -0.461 | 0.230 |
| Travel & Leisure | 0.059 | 0.125 | -1.441** | 1.383 |

Notes: Dependent variables are shares of reporters and editors in each category. Special Issues considers topics such as environment, labor, consumer affairs, real estate and other topics that individually represent a small fraction of resources at individual papers. T-statistics in parentheses: * significant at 10% level; ** significant at 5%; ***significant at 1% level. N=250.

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) |
| <i>Per Capita NYT</i> (g_1) | - | - | -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)** | - |
| <i>NYT*High Ed</i> (g_3) | -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.

Table 6: Does Distraction Drop in Presidential Years?

| | Voting Probability Non-Presidential and Presidential Elections | | | |
|---------------------------------------|---|-----------------------------------|-----------------------------------|-----------------------------------|
| | 1996 (1) | Pooled (94, 96, 98) (2) | Pooled (94, 96, 98) (3) | Pooled (94, 96, 98) (4) |
| <i>Per Capita NYT (g₁)</i> | - | -1.331 | 0.637 | 1.961 |
| | - | (0.30) | (0.36) | (0.47) |
| NYT*1996 | - | -2.797 | 1.240 | -2.510 |
| | - | (1.47) | (0.35) | (1.43) |
| High Ed (γ_2) | 0.167 | 0.171 | 0.163 | 0.000 |
| | (13.17)** | (6.76)** | (6.72)** | (.) |
| High Ed*1996 | - | 0.001 | 0.008 | -0.004 |
| | - | (0.04) | (0.30) | (0.17) |
| High Ed*1998 | - | -0.045 | -0.031 | -0.085 |
| | - | (1.20) | (0.84) | (3.18)** |
| 1996 Year Dummy | - | 0.172 | 0.102 | 0.172 |
| | - | (4.12)** | (1.64) | (3.89)** |
| 1998 Year Dummy | - | -0.043 | -0.069 | -0.032 |
| | - | (0.91) | (1.11) | (0.64) |
| <i>NYT*High Ed (g₃)</i> | -0.680 | -4.117 | -4.119 | -17.790 |
| | (0.34) | (3.76)** | (3.58)** | (2.47)* |
| <i>NYT*High Ed*1996</i> | - | 3.441 | 4.139 | 2.305 |
| | - | (1.69) | (1.95)* | (1.47) |
| Black | 0.077 | 0.072 | 0.069 | 0.071 |
| | (5.24)** | (7.35)** | (7.14)** | (7.30)** |
| Asian | -0.132 | -0.162 | -0.157 | -0.160 |
| | (4.94)** | (9.06)** | (8.67)** | (9.08)** |
| Indian | -0.038 | -0.067 | -0.064 | -0.066 |
| | (1.37) | (4.09)** | (3.82)** | (4.03)** |
| Hispanic | -0.056 | -0.055 | -0.060 | -0.059 |
| | (5.15)** | (6.86)** | (6.88)** | (7.44)** |
| Sex | -0.041 | -0.025 | -0.025 | -0.025 |
| | (10.02)** | (11.12)** | (10.98)** | (11.04)** |
| Age <30 | -0.176 | -0.211 | -0.211 | -0.211 |
| | (25.06)** | (47.45)** | (47.49)** | (47.63)** |
| Age 65+ | 0.162 | 0.194 | 0.194 | 0.193 |
| | (22.79)** | (43.32)** | (43.58)** | (43.18)** |
| Constant | 0.488 | 0.244 | 0.292 | 0.292 |
| | (33.87)** | (5.59)** | (4.83)** | (6.50)** |
| Other Variables | Income, Statewide Elections | Income, Statewide Elections | Income, Statewide Elections | Income, Statewide Elections |
| Fixed Effects | MSA | MSA | MSA x Year | MSA x Education |
| Observations | 43,769 | 131,789 | 131,789 | 131,789 |

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.

Table 7: Does Mobility Explain Distraction?

| | Voting Probability Non-Presidential and Presidential Elections | | | | | |
|---------------------------------------|---|---------------|---------------|---|---|---|
| | 1994 | 1996 | 1998 | Pooled (94, 96, 98) | Pooled (94, 96, 98) | Pooled (94, 96, 98) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| <i>Per Capita NYT (g₁)</i> | - | - | - | -0.323 | 0.543 | 2.968 |
| | - | - | - | (0.07) | (0.31) | (0.71) |
| NYT*1996 | - | - | - | -2.551 | 1.710 | -2.279 |
| | - | - | - | (1.38) | (0.53) | (1.35) |
| High Ed (γ_2) | 0.140 | 0.215 | 0.119 | 0.180 | 0.173 | - |
| | (5.69)** | (12.97)** | (3.74)** | (6.88)** | (6.93)** | - |
| High Ed*1996 | - | - | - | -0.007 | 0.000 | -0.007 |
| | - | - | - | (0.25) | (0.01) | (0.31) |
| High Ed*1998 | - | - | - | -0.046 | -0.030 | -0.084 |
| | - | - | - | (1.33) | (0.89) | (3.30)** |
| 1996 Year Dummy | - | - | - | 0.178 | 0.104 | 0.176 |
| | - | - | - | (4.31)** | (1.71) | (4.04)** |
| 1998 Year Dummy | - | - | - | -0.036 | -0.071 | -0.026 |
| | - | - | - | (0.78) | (1.16) | (0.53) |
| <i>NYT*High Ed (g₃)</i> | -3.955 | -0.725 | -3.032 | -3.592 | -3.739 | -16.996 |
| | (2.89)** | (0.37) | (1.59) | (3.33)** | (3.31)** | (2.44)* |
| <i>NYT*High Ed*1996</i> | - | - | - | 2.795 | 3.475 | 1.679 |
| | - | - | - | (1.36) | (1.62) | (1.06) |
| Residency >1 Year | 0.070 | 0.064 | 0.054 | 0.063 | 0.063 | 0.063 |
| | (6.33)** | (5.85)** | (5.53)** | (10.11)** | (10.01)** | (10.07)** |
| Residency >3 Years | 0.159 | 0.117 | 0.151 | 0.142 | 0.142 | 0.142 |
| | (18.22)** | (11.77)** | (17.86)** | (27.32)** | (26.83)** | (27.19)** |
| Ed*Residency>1Yr | 0.057 | -0.001 | 0.056 | 0.040 | 0.038 | 0.041 |
| | (3.41)** | (0.04) | (2.92)** | (3.78)** | (3.64)** | (3.85)** |
| Ed*Residency>3Yr | -0.027 | -0.057 | -0.012 | -0.033 | -0.032 | -0.033 |
| | (1.83) | (4.28)** | (0.86) | (3.93)** | (3.69)** | (3.91)** |
| Constant | 0.282 | 0.372 | 0.179 | 0.112 | 0.165 | 0.162 |
| | (5.90)** | (23.95)** | (7.65)** | (2.61)** | (2.81)** | (3.69)** |
| Other Variables | | | | Race, Age, Sex, Income, Statewide Elections | Race, Age, Sex, Income, Statewide Elections | Race, Age, Sex, Income, Statewide Elections |
| Fixed Effects | | | | MSA | MSA x Year | MSA x Education |
| Observations | 45,456 | 43,769 | 42,564 | 131,789 | 131,789 | 131,789 |

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. Race, sex, age, 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.

