

Exploiting a Rare Shift in Communication Flows to Document Media Effects: the 1997 British Election

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Abstract:

Using panel data and matching techniques, we exploit a rare change in communication flows—the endorsement switch by several prominent British newspapers to the Labour Party before the 1997 election—to estimate media persuasion effects. This unusual event provides an opportunity to test for such effects while avoiding methodological pitfalls that have plagued previous studies. By comparing readers of newspapers that switched to similar individuals who did not read these newspapers, we estimate these papers persuaded a considerable share of their readers to vote for Labour. Depending on the statistical approach, the point estimates vary from about 10 to as high as 25 percent of readers. These findings provide rare, compelling evidence that the news media exert powerful influence on mass political behavior.

Media persuasion; endorsements; campaigns; elections; matching; causation

Acknowledgments:

We thank Steve Ansolabehere, Larry Bartels Adam Berinsky, Andrea Campbell, Patrick Egan, Daniel Gingerich, Phil Jones, Karen Jusko, Valentino Larcinese, Chap Lawson, Jens Ludwig, Maria Petrova, Mark Peffley, Jim Snyder, Jan Vermeer, and seminar participants at Georgetown University for helpful comments, as well as Mike Myers and Amanda Spears for research assistance. All remaining errors are our own.

1 Introduction

In advanced democracies, politics is generally stable. Public support for parties changes from year to year, but usually does so slowly. New movements and parties tend to emerge gradually. What is the source of this stability? It could be the citizens of these democracies or the elites. As Zaller (1996, 38) asks, “Is politics generally stable because it is founded on the rock of stable public opinion, which largely resists fads, passions, and excitement ... Or is it stable because—and only to the extent that—elite and media politics tend to be stable?”

For many years, social scientists concluded that citizens do indeed resist change, arguing that media messages cannot easily sway public opinion or voting behavior. Instead of pushing citizens around, exposure to mass communication merely reinforces pre-existing attitudes (e.g. Ansolabehere and Iyengar 1995; Berelson, Lazarsfeld, and McPhee 1954; Finkel 1993; Klapper 1960; Lazarsfeld, Berelson, and Gaudet 1948; McGuire 1986). In this view, the source of democratic stability is the citizens. More recently, researchers have argued that frequent failure to find media persuasion results, not from the absence of media effects, but from formidable methodological obstacles (e.g., Bartels 1993; Erikson 1976; Kinder 1998, 2003; Zaller 1996). Media influence may thus be “massive” (Zaller 1996), but so difficult to detect that researchers can rarely document it. Using approaches that attempt to surmount these obstacles, a few compelling recent studies find large media effects, though the evidence remains sparse. If this alternative perspective is correct, stable democratic politics results in part from stable elite and media politics, not only from the public’s resistance to persuasive messages.

In this paper, we provide evidence of large media effects by exploiting a rare shift in the editorial stance and tone of coverage in some newspapers, but not others, between the 1992 and 1997 British general elections. By combining data on newspaper slant with panel survey data, we

estimate these papers' persuasive effect on vote choice while overcoming many of the methodological obstacles that have plagued previous studies. We find strong evidence of media persuasion, supporting the view that democratic stability comes from elites, not only from citizens.

2 The Challenges of Documenting Media Persuasion

Scholars have argued that two obstacles are primarily responsible for the frequent failure to find media effects. The first is lack of variation in media messages. Aggregate public opinion tends to be relatively stable in the short term (Converse 1990; Page and Shapiro 1992), even in the face of fierce political campaigns (Finkel 1993). Based on these findings, some researchers inferred that the media leave little imprint on public opinion. Others, however, have noted that we should only expect opinion movement when the balance of persuasive messages varies (Erikson 1976; Zaller 1996), a surprisingly rare occurrence. With media outlets, the balance of persuasive messages rarely varies because individual outlets usually maintain similar politics over long periods. The New York Times, for instance, has generally supported Democratic candidates for almost 40 years (Ansolabehere, Lessem, and Snyder 2006). With campaigns, rival messages from opposing candidates tend to offset each other, resulting in little net opinion change, even in the fiercest of contests (Bartels 1992, 2006). Since decisive tests of media influence require change in the balance of messages across individuals or across time, the scarcity of such changes has hampered research.

The second obstacle to finding media effects is that measures of exposure tend to be poor. To measure exposure, researchers often must use error-prone variables such as whether a respondent lives in the same county as a newspaper (e.g. Erikson 1976), general political knowledge (e.g. Price and Zaller 1993; Zaller 1992), or respondents' self-reports about their media usage (e.g. Barker 2002; but see Bartels 1993; Hetherington 1996). Biases introduced by these error-prone variables are unpredictable in direction and can be substantial in magnitude (Achen 1983).

Combined, the lack of variation in the balance of media messages and difficulties measuring exposure are major obstacles to detecting media effects.

Despite these difficulties, some studies find evidence consistent with media persuasion. Further scrutiny of these studies, however, often reveals alternative explanations. Two alternatives in particular are especially difficult to rule out: individuals choosing media outlets that share their politics (self-selection) and media outlets adopting the politics of their audience. Although many studies find associations between survey reports of exposure to certain media outlets and political opinions, associations that tend to be stronger when the outlets are more partisan (Barker 1999, 2002; Barker and Lawrence 2006; Dalton, Beck, and Huckfeldt 1998; Druckman and Parkin 2005; Kahn and Kenney 2002; Project for Excellence in Journalism 2007), determining whether these associations reflect media persuasion or either of these alternatives is difficult.

To avoid bias from these alternative processes, researchers employ several strategies. Some use panel data to track opinion change (Barker 1999, 2002; Lawson and McCann 2004; Bartels, 1993 #6). Others use exogenous measures of exposure (DellaVigna and Kaplan 2007) or instrumental variables (Erikson 1976; Gabel and Scheve 2007). However, concerns about self-selection across panels and violations of exogeneity assumptions haunt such analyses.

Some have tried to overcome these inferential pitfalls with experimental approaches (e.g., Ansolabehere and Iyengar 1995; Berinsky and Kinder 2006; Gilliam and Iyengar 2000; Iyengar and Kinder 1987). Laboratory experiments avoid many of these problems and should be pursued further, but face concerns over external validity. Field experiments greatly reduce concerns about external validity, but, so far, are rare (but see Gerber, Karlan, and Bergan 2006).

In sum, few studies provide convincing evidence of large media effects. The many obstacles to drawing causal inferences have simply been too formidable. In addition, those studies that have more persuasively documented large effects have used outcomes such as policy opinions or primary

election votes (e.g., Zaller 1996), not votes in national elections, which may be more difficult to shift. Thus, in our view, the question of whether the media can readily shift national level, major party vote choice remains largely unanswered.

3 The 1997 British Election and Its Advantages for Causal Inference

In this paper, we examine the persuasive effect of changes in newspaper endorsements in the 1997 British election. This election presents a rare opportunity to estimate media effects while avoiding common inferential pitfalls. First, it contains variation in media messages: a shift in the editorial stance and tone of coverage of some newspapers but not others. Soon after the 1992 election, in response to Britain's ejection from the European Exchange Rate Mechanism, a recession, and Conservative Party leadership squabbles and scandals, most British Newspapers became less enthusiastic about the Conservative government, including longtime supporters like the *Times* and *Daily Mail* (McNair 2003, 159-160; Norris 1998; Seymour-Ure 1997; Tunstall 1996, 254-5). Although most papers merely dampened their Conservative support, several went further, breaking with their past behavior by endorsing the Labour Party in the 1997 election.

In particular, the *Sun*, which had the largest circulation in Britain, *broke with its strident support for Conservatives and* swung its support to Labour (McNair 2003, 160; Norris 1998; Seymour-Ure 1997).¹ The *Sun* announced its shift with a front-page endorsement of Tony Blair on the second day of the official campaign in 1997 (McGuire 1986, 160) and finished the campaign with an election-day cover photo of Blair and a banner headline proclaiming "IT MUST BE YOU" [caps in original] (Scammell and Harrop 1997, 179). According to published accounts, the *Sun's*

¹ The disparity between the circulation and readership of the *Sun* (in Table 1) arises in part because the BEPS oversampled citizens of Scotland, where the *Sun's* circulation is unusually low.

owner, Rupert Murdoch, dictated the *Sun*'s shift (Cassidy 2006; Scammell and Harrop 1997). He allegedly did so in part because Blair made policy concessions, including assuring Murdoch that he was moderate on European integration, and reportedly offering him a friendly regulatory environment (Cassidy 2006; McGuire and McKinney 1997; Smith 2006). Although exchanging media endorsements for business concessions appears uncommon in the U.S., it is the norm in some countries (e.g., Hughes and Lawson 2004; Lawson 2002).

Besides the *Sun*, three smaller newspapers switched from no endorsement in 1992 to a Labour endorsement in the 1997 election. These were the *Daily Star*, *Independent* and *Financial Times*. Table 1 classifies newspapers by their partisan slant in 1992 and 1997 (based on Scammell and Harrop (1997), Seymour-Ure (1997), and Norris (1998)) and provides estimates of the size of their readership in 1996. By providing a rare case of over time variation in communication flows, the switch in partisan slant by these four newspapers during the 1997 campaign provides an opportunity to estimate the persuasive effect of media content on voting behavior.²

To examine the effect of the editorial shifts, we use the British Election Panel Study 1992-1997 (BEPS), which interviewed the same national sample four times before the 1997, postelection wave (in 1992, 1994, 1995, and 1996). We thus know which papers respondents read and their voting preferences before and after the slants changed. These panel data also allow us to address the concern that newspapers may have been following, not leading, changes in their readers' preferences. Furthermore, the sudden endorsement shifts between waves of a panel survey is particularly advantageous for causal inference. Tests of media effects with panel data often remain

² In Britain, the editorial endorsement of a newspaper and the slant of its news coverage are too collinear to be distinguishable. As a result, we are unable to distinguish between the effects of editorial positions and news slant, as Kahn and Kenney (2002) are in the U.S.

vulnerable to self-selection bias. When a news outlet maintains a consistent slant throughout a panel survey, those who choose to expose themselves to it may be predisposed to accept its persuasive messages, even after controlling for observable differences. In this case, however, the lack of prior support for Labour by these newspapers reduces this danger.

Although the sudden endorsement shifts help address nonrandom selection, we can further address such concerns because the BEPS's large sample allows us to compare readers of switching papers only to others who share relevant characteristics. Put differently, the BEPS allows us to estimate the treatment effect with a quasi-experimental, difference-in-differences design (Angrist and Krueger 1999; Athey and Imbens 2006; Shadish, Cook, and Campbell 2002), while correcting for nonrandom selection on observables with parametric techniques and matching.³ To address differences on unobserved variables, we exploit the panel design to conduct placebo tests and sensitivity analysis.

The British media environment also facilitates more accurate measurement of individuals' exposure to press messages than has been possible in most previous studies. In the U.S., connecting survey respondents with the endorsement of their newspapers is difficult because most people read local newspapers and thus respondents in national samples read hundreds of different papers. In Britain, however, many more people read national papers. As a result, a large percentage of a national sample reads the same few papers. Moreover, the BEPS asks respondents which newspaper they read, making it possible to connect respondents with newspaper slant over time. Among voters in the BEPS sample, 211 read one of the slant-switching papers, which, using terms suitable for a

³ Our analytic strategy shares similarities with that employed in Table 6 of Curtice (1999), while improving on it by addressing many threats to internal validity, such as accounting for nonrandom selection with parametric and matching methods.

quasi-experiment, we refer to as the *Treatment*, and 1382 either read papers whose partisan slant was constant or did not read a paper, which we refer to as the control or untreated group.⁴

In sum, this circumstance offers us an unusually good opportunity to estimate media effects while avoiding the external validity problems of laboratory experiments and many methodological problems that have plagued previous observational studies. Although existing studies examine media endorsement changes (Bullock 1984; Erikson 1976; Kahn and Kenney 2002; Lessem 2003; Lieske 1989; MacKuen and Coombs 1981; Robinson 1974, 1976; Veblen 1975), none combine all of these qualities crucial to identifying causation.

4 Data Analysis

Did the change in partisan slant by the *Sun*, *Daily Star*, *Independent*, and *Financial Times* persuade readers to vote differently than they would have otherwise? The evidence suggests that it did. Figure 1 straightforwardly compares the change in the percentage of voters who voted for Labour between 1992 and 1997 among those who did and did not read these papers.⁵ Among those

⁴ Of these 211, 159 read the *Sun*, 24 read the *Independent*, 20 read the *Daily Star*, and eight read the *Financial Times*. The data and codebook are archived at the Inter-university Consortium for Political and Social Research (ICPSR) (www.icpsr.umich.edu). Complete questionnaires are available from the Centre for Research into Elections and Social Trends (www.crest.ox.ac.uk/question.htm#beps1992-97).

⁵ Respondents who did not vote in 1992 or 1997 are excluded from the analysis. We tested for effects of reading a switching paper on turnout and on vote choice among 1992 nonvoters, but there are too few respondents to draw inferences, though 1992 Tory voters did appear to be somewhat less likely to vote in 1997 if they read a switching paper.

who did not, the percent voting for Labour rises by only 10.8 percentage points, from 32.2 to 43.0 percent. Among those who did, it rises 19.4 points, from 38.9 to 58.3 percent. Thus, reading a switching paper corresponds with an 8.6-point greater increase in the likelihood of voting for Labour. Column 1 of the top section of Table 2 presents this statistically significant treatment effect with its standard error. This bivariate result suggests that the slant shifts were indeed persuasive.

With this design, we minimize concerns about reverse causation by measuring newspaper readership before the papers switched. We code individuals as treated only if, in their last, precampaign interview, they reported reading one of the four papers that eventually switched. For many, this last, precampaign interview occurred in 1996, but because the BEPS failed to interview some respondents between the 1992 and 1997 waves, it occurred as early as 1992 for a few. Of course, with this coding, some treated individuals may have switched to reading other papers or no paper by the time of the endorsement switches in 1997, leading us possibly to underestimate the true treatment effect, an issue we address below.

A potential problem with the analysis to this point is that readers of switching papers may differ from readers of other papers. These differences, not the slant change, may cause their greater shift to Labour. In addressing such differences, we are concerned with any that might move citizens toward Labour between 1992 and 1997. We searched the literature and conducted our own analysis to determine what other variables are associated with a greater shift toward Labour. Based on our analysis, the best predictor of shifting to Labour is, not surprisingly, respondents' prior evaluations of the Labour Party (see Appendix Table 1). Respondents who did not vote for the Labour in 1992, but rated Labour favorably, are more likely than are others to shift their vote to Labour in 1997. To account for any differences in evaluations of Labour (and the Conservatives), we include *Labour*

Support and *Conservative Support* as controls.⁶ We also include dummy variables for 1992 *Labour Vote*, *Conservative Vote* and *Liberal Vote*. To avoid posttreatment bias, we measure these and all other control variables in 1992.

In addition to support for the parties, we find that a 10-item *Ideology Scale* (Heath, Evans, and Martin 1994; Heath et al. 1999) proves a good predictor of switching to a Labour vote. Given the housing market crash earlier in John Major's term (Butler and Kavanagh 1997, 247), we expect that a self-reported measure of how respondents are *Coping with their Mortgage* might explain the vote shift.⁷ We are also concerned that the tabloid format of the *Sun* and *Daily Star* might attract readers of a lower socioeconomic status—Labour's traditional base. One might expect these readers to return to the reinvigorated Labour Party, which had been out favor for two decades. To account for such differences, we include *Education*,⁸ *Household Income*,⁹ *Union Membership*, and a 6-item

⁶ We measure respondents' support for the two major parties with questions asking whether they favor or oppose each party on a 7-category scale, ranging from "strongly favor" to strongly oppose."

⁷ We code responses to the question about mortgages into four categories: *Mortgage a Bit or Very Difficult* (1), *Mortgage Not Really Difficult* (0.5), and *Mortgage Not Difficult or No Mortgage* (0). Since the housing market crash occurred after the 1992 interviews, we also tried controlling for 1995 responses to this question, and the results remained unchanged. This question was asked in 1992, 1995, and 1997.

⁸ We code the education variable into five categories: *College Degree or more*, *Some Higher Education*, *A Level or Equivalent*, *O Level or Equivalent*, and no qualifications or foreign qualifications.

Authoritarianism Scale (Heath, Evans, and Martin 1994; Heath et al. 1999). We also account for differences in *Age*¹⁰ and *Gender*, both of which Butler (1997, 247) finds to be associated with switching one's vote to Labour in 1997. Finally, we account for differences between the treated and untreated groups on variables that might moderate the persuasive effect. These include *Education*, *Political Knowledge*,¹¹ and *Watching Television News*.¹² Except for 1992 vote choice, missing values on these variables are imputed. The results remain substantively identical without the imputation.¹³

To the extent that the treated and untreated differ on these or other relevant variables, the estimate in Column 1 of the top section of Table 2 will be biased. Do the treated and untreated

⁹ We use the following income quartiles: *less than £5,999*, *£6000-£11,999*, *£12,000-£19,999*, and *£20,000+*.

¹⁰ We coded responses to the age question into three categories: *18-24*, *25-59*, *65 or Over*.

¹¹ We used the 10-item index of factual knowledge constructed by the BEPS investigators (polqiz92). We also constructed an index of knowledge based on correctly placing the parties on policy issues and found similar results. We use the pre-prepared index in the reported results.

¹² We created an index of television news viewership based on how many days per week respondents reported watching the BBC 6 o'clock news, BBC 9 o'clock news, BBC2 "Newsnight," ITN 5:40pm news, ITN "News at Ten," and the Chanel 4 7 o'clock news. For each program, responses are separated into four categories, never watching, once a week or less, two to three days a week, and four or more days a week.

¹³ We impute because listwise deletion can bias parameter estimates (King et al. 2001). Without the imputation, listwise deletion reduces the number of respondents in that untreated group by 72 and the treated group by only six.

actually differ on these observables? Table 3 compares the two groups on these characteristics, finding a few small but notable differences. Reassuringly, the groups have similar ages, ideologies, and unionization rates. Yet, relative to untreated individuals, treated respondents rate Labour a bit more favorably, are less politically knowledgeable, more educated, wealthy, female, less likely to watch television news, and a bit less likely to have trouble coping with their mortgages. The net direction of the bias from these differences is not obvious a priori. Some differences likely predispose the treated to switch to Labour and others not to.

Does the evidence of persuasion hold after accounting for these differences? Columns 2-6 of the top section of Table 2 present estimates of the treatment effect after applying various methods to correct them. We first describe the methods used in each column and then discuss the estimates. Column 2 uses a parametric approach: a probit model that includes all the variables listed in Table 3 as controls, with categorical variables entered as a series of dummy variables (see Appendix Table 1 for the full results). Controlling for differences parametrically has the disadvantage of making assumptions about the functional form of the model that, if false, can bias estimates of the causal effect (Achen 2002). For example, it assumes that the noncategorical variables, such as Labour Support, have a linear effect on the probit link function.

To reduce this model dependence, Columns 3 and 4 present estimates of the treatment effect after exactly matching treated and untreated respondents on the variables we consider most important. The intuition behind exact matching is straightforward. Instead of estimating the effect with the full sample, we first match treated with untreated respondents on relevant characteristics; in this case, Labour Vote, Conservative Vote, Liberal Vote, Labour Support, Conservative Support, and Political Knowledge. We then drop those who lack an exact match from the analysis, ensuring the treated and untreated groups are identical on matched characteristics, and apply a more conventional parametric model (in this case, probit) to this subset of the original sample (for an

exposition, see Ho et al. 2007).¹⁴ This reduces omitted variable bias without making strong assumptions about functional form and increases the robustness of treatment effect estimates to model specification choices (Ho et al. 2007). Analogous to Columns 1 and 2, Column 3 presents the treatment effect without controls and Column 4 presents the treatment effect from a probit model with control variables, both now using only exactly matched respondents.

With exact matching, we are limited to the categorical variables we consider most important because exact matching on additional variables results in a precipitous loss of respondents. However, the models in Columns 5 and 6 do attempt to balance the treated and untreated groups on *all* the Table 3 variables using a genetic approach developed by Diamond and Sekhon (2005). As the last Column of Table 3 illustrates, it achieves good balance on all the covariates by excluding most of the original control group.¹⁵ By matching on all covariates, we further reduce concerns about omitted variables and functional form assumptions.

As an additional test of balance, the bottom row of Table 3 presents a placebo test that compares responses to a vote intention question asked in 1996. Even though we are not matching on

¹⁴ We implement exact matching using “MatchIt” (Ho et al. 2006) and genetic matching using “Matching” (Sekhon 2007, Forthcoming). Several other recent political science studies have used matching (e.g., Gordon and Huber N.d.; Imai 2005; Simmons and Hopkins 2005).

¹⁵ We also checked for balance by comparing the standard deviations of the non-binary variables and found little imbalance. The largest imbalance occurred with age, with the treated group having a standard deviation 0.042 larger than the control group. Most standard deviations differed by less than 0.01. To further test for balance, we standardized the variables to have standard deviations of one and means of zero. After this transformation, the largest imbalance was small: 0.07 on Political Knowledge. Most variables had differences of less than 0.04.

this variable, differences on it largely disappear in the matched data, indicating that the matching has successfully accounted for differences in the tendency to vote for Labour in 1996, the last survey wave before the endorsement switches.

Each of these approaches attempts to eliminate bias from nonrandom selection on observables. If the estimated treatment effect arose from such bias, it should vanish when we employ these methods. Looking across the results in the top section of Table 2, however, the effect gets larger, not smaller. Although the estimates vary somewhat, ranging from 8.6 to 14.5 percent, these different approaches to nonrandom selection on observables all indicate a substantive and statistically significant effect. (For all the probit models, the table presents the marginal effect for a 1992 Conservative voter, with ideology, Conservative support, and Labour support set to the means for such a voter, and all other variables set to the sample means.) In summary, the persuasive effect observed in Figure 1 does not appear to be an artifact of differences between treatment and control groups, at least not on the covariates in Table 3. Appendix Table 2 shows that the treatment effect is robust among only readers of the *Sun* and only readers of the other three switching papers, though the effect on the latter is larger. Reading one of the papers that switched to Labour appears to have persuaded people to vote for Labour.¹⁶

¹⁶ To further test the robustness of our results, we include (in analyses not reported) additional control variables in the probit models used for Table 2. Since the tabloid style of the *Sun* and *Daily Star* probably appeals to working-class individuals (who may be predisposed to shift to Labour) we included *Self-Reported Social Class*, *Vote of Parents When He/She Was a Child*, opinion on whether *Unions have Too Much Power*, and opinion on whether *Business Has Too Much Power*, dummy variables for identifying with each party, *Vote in 1987*, *Support for European Integration*, *Retrospective evaluations of the economy* in 1996, and regional fixed effects. Finally,

5 Robustness

5.1 *Measurement Error*

To this point, our analysis has relied on a potentially weak measure of exposure to the treatment—whether individuals read switching papers in their last interview preceding the switch. This measure is potentially noisy, both because of the well-known tendency of self-reported measures to be error prone (Bartels 1993; Zaller 1992), and because some BEPS respondents may have stopped reading papers that switched between 1996 (or their earlier interview if not interviewed in 1996) and the time these papers switched in 1997. If random, error from these sources may bias our treatment effect estimates downwards. To better measure exposure, we code respondents as treated only when they report reading the *Sun*, *Daily Star*, *Independent*, or *Financial Times* in every panel in which they were interviewed between 1992 and 1996. By using this measure of “habitual readership,” we reduce measurement error somewhat by increasing the chance that respondents coded as treated in fact read a switching paper and continued to read it through the 1997 election. Of the 211 treated respondents analyzed above, 102 are habitual readers. The lower section of Table 2 presents results analogous to those in the upper section, but for habitual readers. Across its columns, the estimates are consistently larger, ranging from 11.7 to almost 25 percentage points. Thus, as expected, when we measure exposure to the treatment more accurately, our

we attempt to address further concerns about ideology. Given that Blair positioned himself as a centrist, moderates may have shifted to Blair at higher rates than did others and read switching papers at higher rates than did others. Although the genetic matching should reduce such concerns, we also added a measure of ideological moderation by folding the 12-item ideology scale. Including all these covariates in the probit models leave the treatment effect estimates unchanged.

estimates of the effect size increase considerably.¹⁷ We should note, however, that the larger estimates could also arise because the treatment effect is simply larger among habitual readers.

5.2 *Newspapers Following Readers*

A remaining alternative explanation for this finding is that switching papers may have shifted to Labour between 1992 and 1997 because they observed their readers shifting to Labour and then followed them. To address this concern, we check that readers of these papers do not begin shifting to Labour before the 1997 campaign. We do so by verifying that the persuasion effect only emerges between the 1996 and 1997 waves of the panel. Finding that it emerges *before* 1996, that is, before the endorsement switches, would raise concerns about reverse causation. Since no general election occurred in 1996, we use a question that asks respondents how they would have voted had there been an election (the same one used to test for balance in the bottom of Table 3). Figure 2 presents the persuasive effect, as in Figure 1, while also showing the means for the treated and untreated in the 1996 survey wave, just before the treatment. It does so for two types of treatment groups: all readers (top panel) and habitual readers (bottom panel).¹⁸ As expected, the treatment

¹⁷ Consistent with this result, Kahn and Kenney (2002) find that newspaper coverage has more influence on regular readers. The BEPS also contains other measures that might better capture exposure to the treatment. While self-reports such as these may be highly unreliable, the BEPS does ask whether individuals read their paper “frequently” and whether respondents read articles about politics. In our tests, the effect is not notably different among individuals who self-reported frequently reading or paying attention to political articles in switching papers.

¹⁸ Figure 2 uses unmatched data as in Figure 1. The treatment effect still emerges only between 1996 and 1997 if one uses the matched data. The number of respondents in the treated and

effect is absent before the 1997 wave, reducing concerns that the endorsement shifts were responses to already changing voting preferences among readers of these papers.

In summary, the treated group's shift to Labour did not occur before the endorsement shift, but afterwards. Of course, treated readers could have shifted after the 1996 interviews, but before the 1997 endorsement announcements. Although we cannot rule this out, treated and untreated groups are so similar on covariates that it seems unlikely the treated shifted suddenly to Labour in this short interval, long after the Conservative government had become deeply unpopular.

5.3 *Treatment Group and Panel Attrition*

Another remaining concern is that Conservative readers may have self-selected away from reading switching papers before the 1996 panel wave. Many previously pro-Conservative papers, including switching papers like the *Sun*, *Daily Star*, and *Financial Times*, became relatively more critical of Major's government after the 1992 election. Strong Conservative supporters may have reacted to this coverage by dropping these papers, leaving the remaining readers potentially more predisposed to switch to Labour.

Although plausible, we find little evidence consistent with this account. In the previous section, we showed that readers of switching papers did not become more predisposed to Labour between 1992 and 1996 (compared to others), indicating no net tendency by Conservative supporters to stop reading switching papers before they switched. To further address this concern, we examine newspaper choice across the panel, but find little evidence of self-selection by

untreated groups falls somewhat due to panel attrition in 1996. An anomaly occurs in 1994, where *Sun* readers switch to (hypothetical) Labour vote choices at lower rates than other respondents do. By 1995, however, *Sun* readers join the general shift to Labour.

Conservative readers between the 1992 and the 1996 waves. The defection rate from switching papers to other papers or no paper between 1992 and 1996 was identical for 1992 Labour and Conservative voters: 36.5 percent. Additionally, slightly more 1992 Labour voters left switching papers for Labour papers than 1992 Conservative voters left the switching papers for Conservative papers: 11 versus 9 percent. Thus, self-selection by Conservative supporters before 1997 seems quite unlikely to bias the estimates or render them unrepresentative.¹⁹

Although Conservative supporters do not drop out of the treatment group at higher rates, they may be more likely to drop out of the panel all together. Panel attrition could be higher for them because they may dislike speaking with an interviewer about the dismal prospects of their party. To ensure that these difficult-to-persuade individuals are not dropping out of the panel at higher rates, we estimate the attrition rates for various groups, but find no cause for concern. In fact, a higher percentage of Labour voters drop out of the panel between 1992 and 1997 than do Conservative voters, 47 versus 44 percent, respectively. Moreover, those who strongly support

¹⁹ After the endorsement switches, evidence of self-selection does appear. In contrast to the 1996 wave findings, fewer 1992 Labour voters left the switching papers for other Labour papers between the 1996 and 1997 waves than 1992 Conservative voters left the switching papers for other Conservative papers: 4 versus 12 percent. This self-selection may bias the treatment effect estimates downward, leading us to underestimate media persuasion. To address this possibility, we reestimated the persuasive effect while measuring the treatment in 1997 and instrumenting it with the treatment measured in 1996 (or before). With this instrumental variables approach, which has the virtue of also addressing random measurement error, the estimates suggest that switching papers persuaded about 14 percent of their readers to vote for Labour. This is a large and highly statistically significant effect, though the exogeneity of the instrument is suspect.

Labour (on the “Labour Support” variable) drop out of the panel at a considerably higher rate than those who strongly oppose Labour, 50 versus 43 percent, respectively.

5.4 *Nonrandom Selection on Unobservables*

A potential problem not yet fully addressed is nonrandom selection to treatment and control groups on *unobservable* characteristics. While our placebo test in Section 4 helps to alleviate this concern, we also conduct a sensitivity analysis, examining the plausibility of an omitted variable creating the appearance of the effect in the absence of a true effect (Rosenbaum 2002). We find that the omitted variable would have to be both moderately correlated with the treatment and strongly influence vote switching to produce the observed effect. For example, consider omitting the control variable with the largest effect on Labour vote in 1997 other than the lagged dependent variable: support for Labour in 1992, whose probit coefficient is 1.12 (see Appendix Table 1). Even if an omitted variable had a coefficient this large, to spuriously produce the treatment effect the treatment and control groups would have to differ on that variable by about .3 on a one-point scale. Since none of the observed covariates differs to this extent (the largest is ideology, at .126), an omitted variable or group of omitted variables seems unlikely to be the source of our findings.

The long panel allows us to conduct a second placebo test to further address concerns about unobservables. With the long panel, we observe individuals who read the *Sun*, *Daily Star*, *Independent*, or *Financial Times* in early waves of the panel, but stop reading these papers before they endorse Labour. Since these individuals are similar to our treated respondents on the key observed characteristic—reading one of these papers—we might also suspect them to be similar on unobserved characteristics. Of the 1382 untreated respondents, 120 fall into this category: they read the *Sun*, *Daily Star*, *Independent*, or *Financial Times* but stop before 1996, so fail to receive the treatment. Among them, we see no evidence of a treatment effect. Relative to the other control

group members, they are slightly less likely, not more likely, to shift their votes to Labour ($b = -3.59$, $s.e. = 3.8$). Thus, the sensitivity test and the two placebo tests assuage concerns about differences on unobservables.

5.5 *Year of Measurement of Control Variables*

As a final test of robustness, we estimate the treatment effect between 1996 and 1997 (instead of between 1992 and 1997), using the hypothetical vote choice question in 1996 as the measure of previous vote, and measuring the controls in 1996 instead of 1992. With 1996 as the baseline, the treatment effect estimates increase somewhat. The probit coefficient for the treatment effect is $.75$ ($s.e. = .18$, $n=1215$), implying a marginal effect of approximately 10 percent. Thus, our result holds up nicely against a wide range of controls, measured either early or late in the panel.

6 Treatment Interactions and Mechanisms

In an extension, we test whether the persuasive effect is larger or smaller for some kinds of individuals, that is, we test for additional treatment interactions. Not surprisingly, the effect is significantly larger among respondents who self-report as being working class. The effect is also somewhat larger among people who report having trouble coping with their mortgages. We also looked for evidence that the effect size varied with exposure to countervailing messages. Zaller (1992; 1996) finds that the magnitude of media effects increases when individuals are less exposed to countervailing messages.²⁰ Applying this logic to our case, we might expect more politically knowledgeable individuals to see or hear more counter-persuasive Conservative campaign appeals,

²⁰ Although in some circumstances those at middle-levels of political knowledge are most susceptible to persuasion, when holding exposure constant, as we attempt to do here, Zaller (1992) expects persuasion to be negatively related to knowledge.

reducing the persuasive power of newspapers. We have pursued several strategies to test this and related hypotheses, but found no notable differences in susceptibility to persuasion by political knowledge or TV-news viewership. Given the limited number of treated individuals, this could reflect an absence of differences or that our sample is too small to detect them.

Finally, we consider possible mechanisms of persuasion. The large sample of *Sun* readers might conceivably permit us to test how the *Sun*'s endorsement persuades readers to vote Labour. Is it the mere fact of endorsement? Its support for particular policies? Its coverage of the campaign? According to Seymour-Ure (1997), the *Sun*'s endorsement article primarily emphasized Major's incompetence and Blair's strong leadership skills. It did not dwell on particular policies. Therefore, the *Sun* may have persuaded its readers by convincing them Blair was a stronger leader. Although the BEPS asks about Blair's and Major's leadership abilities, potentially allowing us to test this mechanism, unfortunately the questions' wording produces almost universally positive ratings for Blair and almost universally negative ratings for Major. Consequently, as *Sun* readers did not differ from others on this question, these data seem unlikely to shed light on the mechanism of persuasion.

7 Discussion

The magnitude of the effect documented here is not just larger than those found in previous endorsement studies, which usually find persuasion effects between one and five percent (Erikson 1976; Krebs 1998; Lessem 2003), but also suggests the influence of media slant on vote choice is large relative to other well-documented effects on voting. For example, it is larger than the incumbency advantage in U.S. House elections, one of the most studied effects in political science, which has averaged about five percentage points in recent decades (for a review of this large literature, see Ansolabehere et al. 2006).

Moreover, our analysis probably underestimates the true persuasive effect. The use of an error prone self-reported measure of media exposure, its assessment in 1996, many months before the newspapers switched, and the higher panel attrition rate among Labour supporters, who constitute the most easily persuaded group, all probably bias these estimates downward. Additionally, the *Sun*'s switch presumably received considerable media coverage. Control group members may have learned about its switch and thus received part of the treatment, violating the stable unit treatment value assumption (Rubin 1974) and consequently biasing our treatment effect estimate downward.

Is the large effect observed here representative of media power in other times and settings? There are reasons to believe U.S. newspaper endorsements may be less persuasive than those in Britain. Unlike U.S. newspapers, British papers often endorse on the front page—the *Sun* did so twice during the 1997 campaign (Tunstall 1996, 161-2)—and tend to slant their coverage more than U.S. papers do.

Furthermore, endorsements that involve a switch of parties, like some of those in 1997, may also have larger effects than other newspaper endorsements because biased sources can be especially informative when they argue against their bias (Calvert 1985). More than their American counterparts, British papers offer strong partisan perspectives and have close ties to the parties (Tunstall 1996). For example, the *Sun*'s past Conservative Party bias was well known, especially in the previous election in 1992, when it waged a vitriolic campaign against Labour and its leader, Neil Kinnock (Cassidy 2006).

Finally, media messages may have even larger effects on other kinds of attitudes and behaviors than on voting preferences in national elections. In general, attitudes are easier to influence than behavior (Eagly and Chaiken 1993). Other recent studies finding large media effects do so by looking at either vote choice for lower-level offices or opinions about public policy

(though see, Huber and Arceneaux 2007; Johnston, Hagen, and Jamieson 2004). For example, Zaller (1992; 1996) finds “massive” media effects on policy opinions and primary and congressional voting. However, these types of preferences may be easier to influence than votes for high-level office, such as for a party in a parliamentary system or for a president, where powerful long-term party attachments play a larger role (Achen and Bartels 2006; Cohen 2003; Converse 1964; Green, Palmquist, and Schickler 2002; Kinder 1998). Thus, whatever media effects one finds on national vote choice—a relatively difficult to influence behavior—likely imply even greater media influence on other political preferences.

8 Conclusion

Using panel data and matching techniques, we exploit a rare change in media slant and find strong evidence of persuasion. By comparing readers of newspapers that switched to similar individuals who did not read these newspapers, we estimate these papers persuaded a considerable share of their readers to vote for Labour. Depending on the statistical approach, the point estimates vary from about 10 to as high as 25 percent of readers. If the *Sun*'s endorsement was in exchange for a friendly regulatory environment for Murdoch, the concession may have bought Blair between 8 percent and 20 percent of his 3.9 million-vote margin over the Conservatives. This finding provides rare, compelling evidence that the media exert a strong influence on mass political behavior, and, consequently, implies that the source of stability in democratic politics may not only be the behavior of citizens, but also elites.

We emphasize again the unusual confluence that permits us to estimate the treatment effect while avoiding many of the methodological problems that plague previous studies. First, we have an uncharacteristic change in the partisan slant of normally loyal newspapers. Second, we can measure individuals' exposure to this shift. Third, the large sample size of the BEPS allows us to address

omitted variable bias by matching similar exposed and unexposed respondents in addition to the usual parametric techniques. Finally, we address various potential sources of bias by using panel data that span the shift. To our knowledge, no other observational media effects study combines these attributes.

Table 1: British Newspapers' Party Orientations

	What paper did you read most often? (%)	Circulation (thousands)		Editorial Stance	
		1996	1992 1997	1992	1997
Doesn't read newspaper	30.4				
Consistent Conservative					
<i>Daily Express</i> (Scottish)	5.6	1,525	1,220	Conservative	Muted Conservative endorsement
<i>Daily Mail</i>	9.9	1,675	2,151	Conservative	Conservative
<i>Daily Telegraph</i>	5.5	1,038	1,134	Conservative	Conservative
Consistent Labour					
<i>Daily Mirror/Record</i>	18.1	2,903	3,084	Labour	Labour
<i>Guardian</i>	2.3	429	401	Labour	Labour
Switched to Labour					
<i>Sun</i>	9.7	3,571	3,842	Conservative	Labour
<i>Daily Star</i>	1.4	806	648	Tone favors Conservatives	Labour
<i>Independent</i>	1.6	390	251	No affiliation	Labour
<i>Financial Times</i>	0.7	290	307	"Not a Tory majority"	Labour
Other/Uncertain					
<i>Times</i>	2.5	386	719	Conservative	None (Euro-Sceptic)
Other/Not answered	12.3				
<i>n</i> =	1608				

Coding of newspaper slant is based on Seymoure-Ure (1997), Norris (1998) and Scammell (1997).
 Percentage of respondents reading who reported them in the 1996 wave of the BEPS survey.
 Circulation data from Seymoure-Ure (1997).

Table 2: Persuasive Effect of Endorsement Changes on Labour Vote Choice between 1992 and 1997

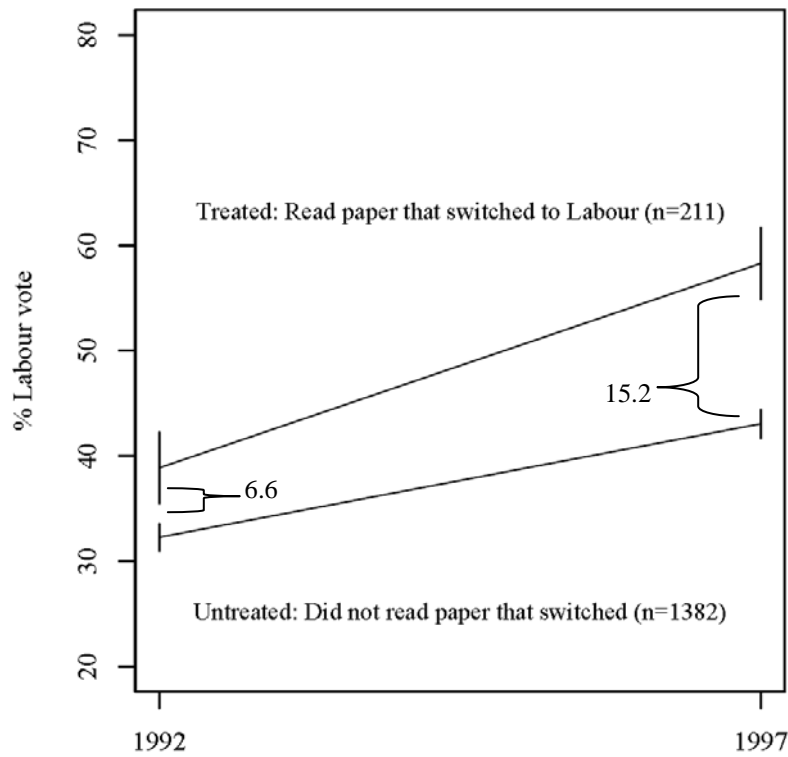
	Matching					
			Exact on Selected Variables		Genetic on All Variables	
	Bivariate	Multivariate (Probit)	Bivariate	Multivariate (Probit)	Bivariate	Multivariate (Probit)
Among All Readers						
Treatment Effect	8.6	12.2	8.7	14.5	10.4	13.0
(%)						
(Standard error)	(3.0)	(3.6)	(3.0)	(4.0)	(4.5)	(4.4)
<i>n</i> Treated / <i>n</i> Control	211/1382	211/1382	201/1100	201/1100	211/181	211/181
Among Habitual Readers						
Treatment effect	11.7	21.1	15.2	24.8	15.7	22.7
(%)						
(Standard error)	(4.4)	(5.9)	(4.1)	(4.0)	(5.9)	(8.4)
<i>n</i> Treated / <i>n</i> Control	102/1382	102/1408	98/917	98/917	102/97	102/97

This table shows that reading a newspaper that switched to Labour in the 1997 election led voters to also switch to Labour, an effect that persists when controlling for the variables listed in Table 3. Bivariate analyses are simply a difference-in-differences means test. In multivariate probit models, vote choice in 1997 is the dependent variable and explanatory variables include the treatment and the variables listed in Table 3. For these models, the Table reports the marginal effect of the treatment and its standard error for a 1992 Conservative voter, with ideology, Conservative support, and Labour support set to the means for such a voter, and all other variables set to the sample means. Parameter estimates from multivariate probit models are reported in Appendix Table 1. Exact Matching is performed using Vote Choice, Labour Support, Conservative Support, and Political Knowledge. Genetic matching is performed using all variables in Table 3. In the matching analyses, unmatched observations are discarded before parametric estimation and the remaining observations are weighted to equalize treated and control subclasses. In the bottom half of the Table, those who read switching papers but not habitually are excluded from the analysis.

Table 3: Comparing Covariates among the Treated and Untreated Groups

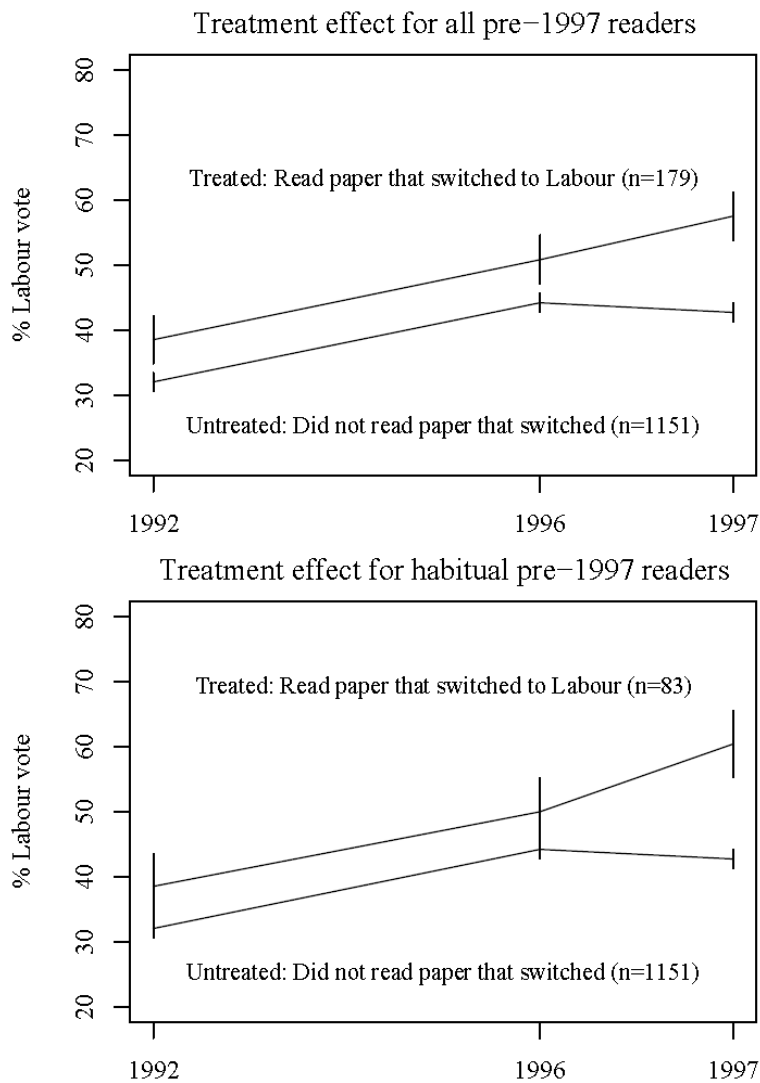
Covariates (measured in 1992)	<i>All</i>		<i>Difference</i>		
	Treated	Untreated	All	Exact	Genetic
Labour Vote (Labour 1, Other 0)	0.389	0.323	0.066	0.000	0.000
Conservative Vote (Conservative 1, Other 0)	0.389	0.404	-0.015	0.000	0.000
Liberal Vote (Labour 1, Other 0)	0.156	0.188	-0.032	0.000	-0.005
Labour Support (Strongly Favor 1 to Strongly Oppose 0)	0.488	0.462	0.026	0.000	-0.004
Conservative Support (Strongly Favor 1 to Strongly Oppose 0)	0.524	0.522	0.002	0.000	0.014
Political Knowledge (High 1, Mid .5, Low 0)	0.545	0.671	-0.126	0.000	-0.005
Watching Television News (Yes 1, No 0)	0.218	0.289	-0.071	-0.048	0.005
Ideology Scale (Liberal 1 to Conservative 0)	0.645	0.634	0.011	0.001	-0.002
Authoritarianism Scale (Low 1 to High 0)	0.537	0.528	0.008	0.013	0.005
Union Membership (Yes 1, No 0)	0.218	0.240	-0.022	-0.031	0.000
Coping with Mortgage (Not well 1 to Well/NA 0)	0.291	0.337	-0.046	-0.034	0.019
Education (College 1 to No Education 0)	0.598	0.514	0.084	0.049	-0.009
Household Income (High 1 to Low 0)	0.469	0.386	0.084	0.054	0.027
Age (Old 1 to Young 0)	0.464	0.453	0.011	0.013	0.007
Gender (Male 1, Female 0)	0.508	0.442	0.066	0.133	0.000
<i>n</i>	211	1382	211/1382	201/1100	211/181
<i>Additional Balance Check</i>					
Labour vote intention in 1996 (Labour 1, Other 0)	0.508	0.442	0.066	0.022	-0.014

Figure 1: Persuasive Effect of Endorsement Changes on Labour Vote Choice between 1992 and 1997



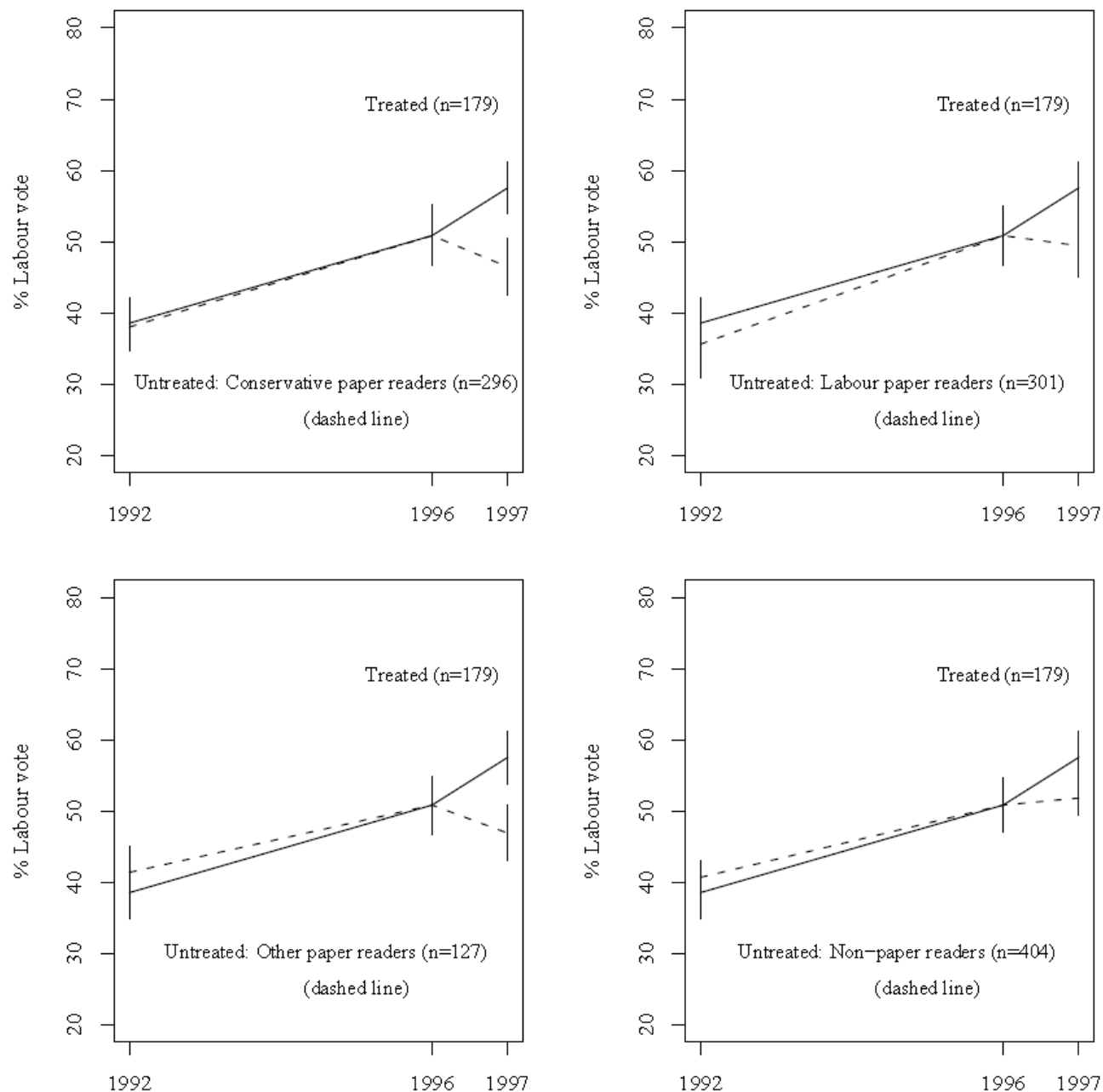
This figure shows that reading a paper that switched to Labour is associated with an 8.6-percentage point ($15.2 - 6.6$) shift to Labour between the 1992 and 1997 British elections. Paper readership is measured in the 1996 wave, before the papers switched, or, if no 1996 interview, in an earlier wave. Confidence intervals show one standard deviation.

Figure 2: The Treatment Effect Only Emerges in 1997



Using the hypothetical vote choice question asked in the 1996 wave, this figure shows that the treatment effect only emerges after 1996. Habitual readers are those who read a paper that switched in every wave in which they were interviewed before the 1997 wave. Respondents who failed to report a vote choice or vote intent in any of the three waves are excluded from the analysis, which results in a smaller n than Figure 1 because of attrition or failure to report a vote intent in the 1996 wave. Confidence intervals show one standard deviation.

Figure 3: Comparing the Treated with Four Control Groups



This figure shows that the treatment effect persists when comparing treated respondents to various control groups, including (persistent) Conservative paper readers, Labour papers readers, other or no affiliation paper readers, and those who did not read newspapers. To facilitate comparisons, we mean-differenced the control groups in 1996, ensuring the treatment group and each control group have the same mean in 1996. Readership is measured in 1996. Respondents who failed to report a vote choice or vote intent in any of the three waves are excluded from the analysis, resulting in smaller *ns*. Confidence intervals show one standard deviation.

Appendix Table 1: Probit Models of Vote in 1997 British General Election

	All	Preprocessed Matching	
		Exact	Genetic
Treatment	0.49 (0.12)	0.56 (0.13)	0.54 (0.18)
Labour Vote	1.15 (0.16)	1.67 (0.27)	0.66 (0.35)
Conservative Vote	-0.19 (0.16)	0.09 (0.28)	-0.63 (0.37)
Liberal Vote	-0.06 (0.14)	0.25 (0.26)	-0.31 (0.33)
Labour Party Support	1.12 (0.21)	0.77 (0.29)	0.68 (0.47)
Conservative Party Support	-0.39 (0.21)	-0.49 (0.29)	-0.80 (0.48)
Mid Knowledge	-0.24 (0.12)	-0.29 (0.14)	-0.32 (0.23)
High Knowledge	-0.38 (0.13)	-0.41 (0.16)	-0.41 (0.27)
Watching TV News	0.10 (0.09)	0.17 (0.11)	0.42 (0.21)
Read Daily Newspaper	-0.16 (0.09)	-0.20 (0.11)	-0.42 (0.23)
Ideology Scale	1.00 (0.42)	0.94 (0.48)	0.91 (0.85)
Authoritarianism Scale	-0.14 (0.36)	0.00 (0.42)	-0.37 (0.77)
Trade Union Member	0.09 (0.10)	0.07 (0.11)	0.30 (0.21)
Mortgage Not Really Difficult	0.05 (0.11)	0.11 (0.13)	0.01 (0.23)
Mortgage a Bit or Very Difficult	0.18 (0.12)	0.18 (0.14)	0.50 (0.26)
O Level or Equivalent	0.18 (0.16)	0.29 (0.19)	0.21 (0.41)
A Level or Equivalent	0.15 (0.18)	0.29 (0.21)	-0.16 (0.41)
Some Higher Education	0.05 (0.17)	0.18 (0.19)	-0.25 (0.39)
College Degree	0.20 (0.16)	0.29 (0.19)	-0.26 (0.40)
	-0.05	-0.02	-0.08

£6000-£11,999

(head of household income)	(0.11)	(0.13)	(0.25)
£12,000-£19,999	0.21	0.26	0.60
(head of household income)	(0.13)	(0.15)	(0.28)
£20,000+	-0.11	-0.10	-0.11
(head of household income)	(0.16)	(0.18)	(0.32)
Age 25-59	0.07	0.05	-0.14
	(0.13)	(0.15)	(0.26)
Age 65 or over	0.47	0.41	0.17
	(0.21)	(0.24)	(0.43)
Female	0.08	0.16	-0.11
	(0.09)	(0.10)	(0.19)
Constant	-1.34	-1.68	0.26
	(0.48)	(0.60)	(1.02)
Log likelihood	-640.951	-471.036	-165.499
<i>n</i>	1593	1301	392

All covariates measured in 1992 and coded to vary between 0 and 1. Table 2 uses the estimates from this table in its marginal effects calculations.

Appendix Table 2: Persuasive Effect of Endorsement Changes on Labour Vote Choice, Sun and non-Sun readers, 1992-1997

			Matching			
			Exact		Genetic	
Sun Readers Treated (other treated excluded)						
	Bivariate	Probit	Bivariate	Probit	Bivariate	Probit
Treatment effect (%)	6.2	10.7	8.2	11.9	8.2	12.6
(Standard error)	(3.4)	(4.1)	(3.4)	(4.4)	(3.7)	(5.2)
<i>n</i> Treated / <i>n</i> Control	159/1382	159/1382	152/1006	152/1006	159/141	159/141
Non-Sun Treated (other treated excluded)						
	Bivariate	Probit	Bivariate	Probit	Bivariate	Probit
Treatment effect (%)	16.1	16.0	16.6	22.4	23.6	16.1
(Standard error)	(5.7)	(7.1)	(5.4)	(8.9)	(4.2)	(5.4)
<i>n</i> Treated / <i>n</i> Control	52/1382	52/1382	49/647	49/647	52/53	52/53

For details, see the note to Table 2. Of the non-Sun Treated, 24 read the *Independent*, 20 read the *Daily Star*, and eight read the *Financial Times*. The treatment effect holds separately for each of these papers: approximately 14% for the *Independent*, 6% for the *Daily Star*, and 39% for the *Financial Times*.

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