Exploiting a Rare Shift in Communication Flows to Document News Media Persuasion: The 1997 United Kingdom General Election

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Abstract:
Using panel data and matching techniques, we exploit a rare change in communication flows—the endorsement switch to the Labour Party by several prominent British newspapers before the 1997 United Kingdom general election—to study the persuasive power of the news media. These unusual events provide an opportunity to test for news media persuasion while avoiding methodological pitfalls that have plagued previous studies. By comparing readers of newspapers that switched endorsements to similar individuals who did not read these newspapers, we estimate that 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 percent to as high as 25 percent of readers. These findings provide rare, compelling evidence that the news media exert a powerful influence on mass political behavior.

Key words: Media persuasion; endorsements; campaigns; elections; matching; causal inference

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1 Introduction

In long-standing 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—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 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, citizens are the source of democratic stability. More recently, however, researchers have argued that frequent failures to detect media persuasion result not from its absence, but from formidable methodological obstacles (e.g., Bartels 1993; Erikson 1976; Kinder 1998, 2003; Zaller 1996). Media influence may 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 to a large extent from stable elite and media politics, not merely from the public's resistance to persuasive messages.

Most of the evidence finding large media effects comes from work on television advertising (e.g., Huber and Arceneaux 2007; Johnston, Hagen, and Jamieson 2004) and elite influence on public opinion (e.g., Gabel and Scheve 2007; Zaller 1996). To date, few researchers have used

modern identification strategies to examine the persuasive effect of news content and endorsements on electoral preferences. Do news organizations also exert a powerful influence on citizens' political preferences? This paper addresses this question by exploiting a rare shift in the editorial stance and tone of coverage in four newspapers between the 1992 and 1997 United Kingdom (UK) general elections. By combining newspaper endorsement changes with panel survey data, we estimate these papers' effect on voting while overcoming many of the methodological obstacles that have plagued previous studies. Our results indicate that persuasion effects are large, supporting the view that the stability of democratic politics depends largely on elites.

2 The Challenges of Documenting Media Persuasion

Research on news media persuasion—and media effects more generally—faces four major obstacles that have frustrated scholars as they try to reach consensus. Two of these obstacles prevent the detection of various types of media effects. The first of these is lack of variation in message. For instance, based on the relative short-term stability of aggregate public opinion (Converse 1990; Page and Shapiro 1992), even in the face of fierce political campaigns (Finkel 1993), some researchers infer that campaigns (and news coverage of them) leave little imprint on public opinion. Others, however, note that we should only expect opinion movement when the balance of persuasive messages varies (Erikson 1976; Zaller 1996), a surprisingly rare occurrence. They point out that rival campaign messages tend to offset each other, making relative opinion stability unsurprising (Bartels 1992, 2006). In the case of news outlets, the balance of persuasive messages rarely varies because each outlet usually maintains a similar political stance over long periods. The *New York Times*, for instance, has generally supported Democratic candidates for almost 40 years (Ansolabehere, Lessem, and Snyder 2006).

The second obstacle preventing the detection of campaign and news 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 a county in which a newspaper has high circulation (e.g. Erikson 1976), general political knowledge (e.g. Price and Zaller 1993; Zaller 1992), or self-reported campaign attention or media usage (e.g. Barker 2002; but see Bartels 1993; Hetherington 1996). These error-prone variables introduce biases of potentially substantial magnitude and unpredictable direction (Achen 1983). Combined, the lack of variation in the balance of messages and difficulties measuring exposure are major obstacles to detecting media effects and may have led to the "minimal effects" paradigm that once dominated media effects scholarship (Klapper 1960; McGuire 1986).

Despite these difficulties, some studies find evidence consistent with campaign or news media persuasion. When they do find such evidence, however, researchers face two additional obstacles to demonstrating these effects convincingly. These obstacles take the form of alternative explanations that are difficult to rule out. First, individuals may choose media outlets that share their politics (self-selection), creating the appearance of persuasion. Second, media outlets may follow, not lead, their audiences' politics, which also could be mistaken for persuasion. Although many studies find individual-level associations between survey reports of exposure to certain news outlets and political opinions (Barker 1999, 2002; Barker and Lawrence 2006; Dalton, Beck, and Huckfeldt 1998; Druckman and Parkin 2005; Kahn and Kenney 2002; Lawson and McCann 2004; Newton and Brynin 2001; Project for Excellence in Journalism 2007), these associations could thus arise because of media persuasion or because of these two alternatives.

Research on media persuasion has employed several strategies to address these four obstacles. For example, Erikson (1976) finds variation in news media messages by exploiting the 1964 shift to Democratic Party endorsements by many newspapers. DellaVigna and Kaplan (2007)

find variation by examining the entry of the Fox News Channel into cable systems in the late 1990s. Several studies develop better measures of exposure by using survey data to directly tie individuals to the newspapers they read, radio programs to which they listen, or television shows they watch (Barker 1999, 2002; Barker and Lawrence 2006; Druckman and Parkin 2005; Lawson and McCann 2004; Newton and Brynin 2001). Laboratory experiments (e.g., Ansolabehere and Iyengar 1995; Berinsky and Kinder 2006; Gilliam and Iyengar 2000; Iyengar and Kinder 1987) can avoid many inferential pitfalls 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).

For the most part, however, no study directly examining news media persuasion convincingly surmounts all four obstacles (see also Bullock 1984; Lieske 1989; MacKuen and Coombs 1981; Robinson 1974). Furthermore, the few studies that come closest find large media effects (Veblen 1975; Zaller 1996), yet only examine outcomes such as policy opinions or primary election votes, not votes in national elections, which may be more difficult to shift. A few recent studies use approaches surmounting these obstacles and find evidence of large television advertising affects (e.g., Huber and Arceneaux 2007; Johnston, Hagen, and Jamieson 2004) and substantial elite influence on public opinion (e.g., Gabel and Scheve 2007; Zaller 1992), but do not examine news media persuasion. In sum, formidable methodological obstacles and the tendency of the most convincing studies to focus on other types of media effects have left the question of whether major news outlets can readily shift national-level, major party vote choice largely unanswered.

3 The 1997 UK Election and Its Advantages for Causal Inference

In this paper, we examine the persuasive effect of newspaper endorsements and slant in the 1997 UK general election. It presents a rare opportunity to study media persuasion because it provides the elements necessary to overcome the aforementioned obstacles. First, this election has

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 the UK'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 papers eventually went further, breaking with their past behavior by endorsing the Labour Party during the 1997 election campaign.

In particular, the *Sun*, which had the largest circulation in Great Britain,[1] broke with its strident support for the Conservatives and swung its support to Labour (McNair 2003; Norris 1998; Seymour-Ure 1997). The *Sun* announced its shift with a front-page endorsement of Tony Blair on the second day of the official 1997 campaign (McNair 2003). It labeled Blair a "strong, dynamic, purposeful leader" whom Britain was "crying out for" (Scammell and Harrop 1997, 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] (179).[2] According to published accounts, the *Sun*'s owner, Rupert Murdoch, dictated the *Sun*'s shift (Cassidy 2006; Scammell and Harrop 1997). He reportedly

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

[2] The Internet Appendix provides a variety of auxiliary materials, including a reproduction of the election day front page of the *Sun*, more detailed variable coding descriptions, and several additional robustness checks. It is available at http://www9.georgetown.edu/faculty/jml89/LaddLenzBritishInternetAppendix.pdf.

did so in part because Blair made policy concessions, including assuring Murdoch of his moderate views on European integration and offering Murdoch a friendly regulatory environment (Cassidy 2006; McGuire and McKinney 1997; Smith 2006).[3] 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 respective readerships in 1996.

While most British newspapers became critical of the Conservative government during its 1992-1997 term and positive about Blair, the fact that these papers endorsed Labour was a surprise. These "switching" papers had no recent histories of supporting Labour and did not leak their endorsements in advance. Of all traditional Conservative Party papers, the *Times* had had the earliest and often most serious criticisms of John Major's government (McNair 2003), yet did not endorse Labour in 1997. Scammell and Harrop (1997, 160) recount the *Sun*'s switch this way,

> Until the [*Sun*] declared for Labour, with deadly timing on the day after Major announced the election, it had been careful to distinguish between the admirable Blair and his dubious party. Now, on the instructions of Rupert Murdoch, the *Sun* threw its weight behind Labour, to the obvious discomfort of some correspondents, including its political editor, Trevor Kavanagh.

The *Sun*'s campaign coverage emphasized Blair's leadership abilities and Major's ineptitude. Unsurprisingly for a tabloid, it did not delve into the policy issues at stake in the election (Seymour-Ure 1997). By providing a rare case of over time variation in communication flows, the unexpected switch in partisan slant by these four newspapers during the 1997 campaign provides an opportunity to estimate the persuasive effect of news media outlets on voting behavior.

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[3] Although exchanging media endorsements for business concessions appears uncommon in the U.S., it is the norm in some countries (Hughes and Lawson 2004; Lawson 2002).

In exploiting changes by these papers, we capture both the effects of the editorial endorsement and changed slant in news coverage. In a media environment in which papers endorse on the front page, the line between editorials and news is blurry. While distinct in theory, these are too collinear to differentiate here.[4]

This case is also unusually well-suited for studying media persuasion because the British media environment facilitates more accurate measurement of individuals' exposure to press messages (Newton and Brynin 2001), overcoming the second major obstacle faced by media effect studies. In the U.S., for instance, connecting survey respondents with the endorsement of their newspapers is difficult because most people read local papers, and respondents in national samples thus read hundreds of different papers. In Britain, however, the major daily newspapers have national distribution, so one can more easily connect respondents in national surveys with the paper they read.

To examine the effect of these editorial and slant shifts, we use the British Election Panel Study 1992-1997 (BEPS), which interviewed the same national sample four times before the endorsement shifts (in 1992, 1994, 1995, and 1996) and once afterwards (following the 1997 election). This panel survey provides a least four elements that aid causal inference. First, it allows us to rule out self-selection because we can measure which papers respondents read before the endorsement shifts. While other tests of media persuasion with panel data remain vulnerable to self-

[4] This contrasts with the U.S., where Kahn and Kenney (2002) are able to distinguish between the effects of news content and editorial endorsements.

selection bias,[5] the suddenness of these shifts makes the prospect of self-selection remote. Among voters in the BEPS sample, 211 read one of the slant-switching papers in 1996 (the last wave before the endorsement shifts), which, using terms suitable for a quasi-experiment, we refer to as receiving the *Treatment*. We refer to the 1382 panelists who either read papers whose partisan slants were constant or who did not read a paper as the control or untreated group.[6] To rule out the possibility that readers in 1996 sensed future endorsement shifts, we also use readership in the first wave (1992) to instrument the Treatment. Second, the BEPS enables us to address concerns about measurement error by constructing an additional, more demanding, measure of the Treatment: habitual readership. We code individuals as habitual readers when they read one of the switching papers in every wave in which they were interviewed before the endorsement shifts. Third, the multiple panel waves enable us to measure many other characteristics that might differ between the treatment and control individuals, and to do so before the papers switched (pretreatment). Moreover, the large number of control subjects (1382) allows us to correct for bias from spurious covariates that vary across Treatment and control groups using parametric models and matching

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[5] When a news outlet maintains a consistent slant throughout a panel survey, those who choose to expose themselves to it may do so because they share its politics and are therefore predisposed to accept its messages, even after controlling for observable differences.

[6] Of these 211, 159 read the *Sun*, 24 read the *Independent*, 20 read the *Daily Star*, and eight read the *Financial Times*. If an individual was not interviewed in 1996, we measure readership in the last interview before the newspaper switched. The data and codebook are archived at the Inter-university Consortium for Political and Social Research (www.icpsr.umich.edu). Questionnaires are available from the Centre for Research into Elections and Social Trends (www.crest.ox.ac.uk/question.htm#beps1992-97).

techniques. Finally, the multiple pretreatment interviews also permit us to conduct two placebo tests (or falsification tests), which help to further rule out omitted variable bias and reverse causation. To summarize in more technical language, 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 both parametric techniques and matching and verifying validity with several placebo tests.

Two studies already examine newspaper influence in the 1997 election. Newton and Brynin (2001) study the role of newspaper endorsements in the 1992 and 1997 UK elections, finding evidence of news media persuasion. To help eliminate bias from self-selection, they improve upon previous studies by comparing individuals who read newspapers consistent with their partisan identification with those who did not. They find, for example, that Conservative Party identifiers are more likely to vote Conservative when they report reading a Conservative paper compared to those who read Labour papers. As with other endorsement studies discussed above, however, these results remain vulnerable to bias from self-selection. For example, Conservative Party identifiers who choose to read Labour papers may do so precisely because they are more predisposed to vote against the Conservative Party.

The second study is Norris et al. (1999), which directly examines the influence of the *Sun*'s switch in the 1997 election. In contrast with our analysis below, it finds no association between newspaper readership and vote change between waves of the 1997 British Election Campaign Study (BECS). However, unlike the panel data used in this paper (the BEPS), the first interview in the BECS occurred 2 to 4 weeks *after* the official, six-week campaign began and thus after newspapers

issued their endorsements. Consequently, this study misses any persuasion that occurred between the endorsements and this first interview.[7]

In addition, several studies examine the effects of newspaper endorsements in other contexts (Bullock 1984; Erikson 1976; Gavin and Sanders 2003; Kahn and Kenney 2002; Lessem 2003; Lieske 1989; MacKuen and Coombs 1981; Robinson 1974, 1976; Veblen 1975). However, none is able to marshal the advantageous events and data available here, while applying modern econometric techniques to maximize the validly of causal inferences. To our knowledge, no existing study of news media persuasion combines all of these qualities crucial to identifying causation.

4 Analysis

4.1 Estimating the Treatment Effect while Accounting for Nonrandom Selection on Observables

Did the change in partisan endorsements and news 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 increase in the percentage of voters choosing Labour between 1992 and 1997 among those who did and did not read these

[7] Norris et al. (1999) also measure readership after the endorsement switches, possibly introducing substantial endogeneity. In a working paper, Curtice (1999) augments the analyses in Norris et al. (1999) with another analysis using BEPS data to examine newspaper persuasion from before the campaign began to the election (what he calls the "long campaign"). Using this approach, he finds evidence that, consistent with our analysis, reading the *Sun* reduced the likelihood of voting for the Conservatives.

papers.[8] Among those 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 considerably more: 19.4 points, from 38.9 to 58.3 percent. Consequently, switching paper readers were 6.6 percent more likely to vote for Labour in 1992 and 15.2 percent more likely to do so in 1997. Thus, reading a switching paper corresponds with an (15.2 - 6.6 =) 8.6 point greater increase in the likelihood of voting for Labour. This statistically significant estimate of the bivariate treatment effect, presented in Column 1 of the top section of Table 2, suggests that the shifts in newspaper slant were indeed persuasive.

Of course, readers of the switching papers potentially differ from control individuals on a myriad of attributes, and these differences, rather than reading a paper that switched, could be inflating this bivariate relationship. By design, we reduce the possibility that such differences result from self-selection by measuring readership before these papers unexpectedly switched to Labour. Nevertheless, differences could still exist. As is evident in Figure 1, for instance, switching paper readers were more likely to vote for Labour in 1992, which may also be indicative of a greater predisposition among these readers toward switching to Labour in the future.

To address the possibility that differences on other attributes, not the slant changes, caused switching paper readers' greater shift to Labour, we condition on a large number of potentially confounding variables. We searched the literature and conducted our own analysis to determine what other variables are associated with shifting to a Labour vote. In all cases, we measure these covariates before the endorsement shifts to avoid bias that can result from measuring control

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[8] 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 among 1992 nonvoters, but there are too few respondents to draw strong inferences, though 1992 Tory voters did appear to be somewhat less likely to vote in 1997 if they read a switching paper.

variables after the treatment (post-treatment bias). 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 Labour in 1992, but who rated Labour favorably, are much more likely than are others to shift their votes to Labour in 1997. To account for any differences in evaluations of Labour, we include *Prior Labour Party Support* as well as *Prior Conservative Party Support* as controls. Unless otherwise specified, all control (or conditioning) variables are measured in the 1992 panel wave.[9] We also include indicator variables for *Prior Labour Vote*, *Prior Conservative Vote*, *Prior Liberal Vote*, *Prior Labour Party Identification*, *Prior Conservative Party Identification*, *Prior Liberal Party Identification*, and whether their *Parents Voted Labour*.

In addition to support for the parties, we find that a 6-item scale of *Prior Ideology* (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 respondents' *Prior Coping with their Mortgage* might explain vote shifts.[10] 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 *Prior Education*, *Prior Income*, *Prior Working Class Identification*, whether a respondent is a *Prior Trade Union Member*, whether he or she identifies as *White*, a 6-

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[9] For detailed descriptions and coding of these variables, see Footnote 2.

[10] 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 only in 1992, 1995, and 1997.

item scale of *Prior Authoritarianism* (Heath, Evans, and Martin 1994; Heath et al. 1999), as well as *Prior Profession,* and *Prior Region*. We also account for differences in *Age* and *Gender*, both of which Butler (1997, 247) finds to be associated with switching one's vote to Labour in 1997. Finally, to account for further differences between the treated and untreated groups on variables that might moderate persuasion, we also include *Prior Political Knowledge*, whether the respondent is a *Prior Television Viewer* or a *Prior Daily Newspaper Reader*. Finally, given that Blair positioned himself as a centrist, moderates may have shifted to Blair at higher rates, so we also include a measure of *Prior Ideological Moderation* created by folding over the 6-item ideology scale. Except for 1992 vote choice, missing values on these variables are imputed.[11] The results remain substantively identical without the imputation.

Reassuringly, readers of switching papers are in fact surprisingly similar to control respondents on these covariates. Table 3 compares the characteristics of the two groups. Relative to untreated individuals, treated respondents are a bit less politically knowledgeable, more working class, more educated, and more female. The net direction of the bias from these differences is not obvious *a priori*. Some differences likely predispose the treated to shift towards Labour, while others predispose them against shifting.

Does the evidence of persuasion hold after controlling for these differences? Columns 2-6 of the top section of Table 2 present estimates of the treatment effect after applying various methods to account for 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

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[11] We impute to avoid bias caused by list-wise deletion (King et al. 2001). Without the imputation, list-wise deletion reduces the number of respondents in the untreated group by 72 and the treated group by only six.

as controls, with categorical variables entered as a series of indicator variables, including fixed effects for region and occupation (see Table 1A in the Appendix for the full results).

Controlling for differences parametrically, however, has the disadvantage of making assumptions about the functional form of covariates that, if false, can bias estimates of causal effects (Achen 2002). This is especially true when treated and control groups differ on key covariates. For example, our probit model assumes that the effect of Working-Class Identification on switching to Labour is linear (in the probit link function). Since treated and untreated individuals do differ somewhat on this variable, the model would fail to account correctly for these differences if this variable's effect on switching to Labour is nonlinear. This problem potentially applies to other functional form assumptions such as possible variable interactions. To address these problems with solely parametric models, we also employ matching techniques. Matching both reduces omitted variable bias without making strong assumptions about functional form of covariates and increases the robustness of treatment effect estimates to model specification choices (Ho et al. 2007). Since the "true model" for can never be known with certainty (Achen 1982; King 1991), these are important advantages.

The intuition behind matching is straightforward. Instead of estimating the effect in the full sample, we first match treated with untreated respondents on observed covariates. We then drop individuals who lack matches, ensuring that the two groups are very similar on these characteristics. Consequently, differences in voting between treated and untreated individuals in the matched sample are more likely to result from the treatment itself, rather than differences on covariates.

We apply two types of matching: exact and genetic. Columns 3 and 4 present estimates of the treatment effects employing exact matching. We drop treated and untreated respondents who lack an exact match on the following variables: Labour Vote, Conservative Vote, Liberal Vote, Labour Identification, Conservative Identification, Liberal Identification, Labour Support,

Conservative Support, and Political Knowledge, ensuring the treated and untreated groups are identical on these characteristics.[12] We then apply a probit model to this subset of the original sample. Analogous to Columns 1 and 2, Column 3 presents the treatment effect without parametric controls, and Column 4 presents the treatment effect from a probit model with control variables.

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 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, genetic matching achieves good balance on all the covariates by excluding most of the original control group.[13] By matching on all covariates, we further reduce concerns about omitted variables and functional form assumptions.

Both matching and parametric controls attempt 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, and is always statistically significant at conventional levels. The estimates vary somewhat, ranging from 8.6 to 14.0 percentage points for all treated readers and 12.7 to 25.7

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[12] We implement exact and genetic matching using "Matching" (Sekhon 2007, Forthcoming). Several other recent political science studies have also used matching (e.g., Gordan and Huber 2007; Imai 2005; Simmons and Hopkins 2005). With genetic matching, we specify that exact matching should still be attempted on the same eight covariates.

[13] For additional balance statistics, see Footnote 2.

for habitual readers.[14] Thus, a substantively and statistically significant effect persists across these different approaches to nonrandom selection on observables.[15]

We conduct several additional tests of robustness. To address concerns that strong Conservative supporters may have self-selected away from switching papers between 1992 and 1996, as well as other potential self-selection problems, the seventh column of Table 2 presents the treatment effect when we instrument readership in 1996 with readership in 1992. Even when using a measure of readership 5 years prior to the actual endorsement switch, the effect is estimated at 10.9 percent.[16] As another 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

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[14] Consistent with this result, Kahn and Kenney (2002) find that newspaper coverage has more influence on regular readers. The BEPS also contains other self-reported measures that might better capture exposure to the treatment. While self-reports such as these may be highly unreliable, the BEPS asks 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. Also, we find similar results when we compare habitual switching paper readers to habitual readers of non-switching papers and when we compare them to habitual nonreaders.

[15] For the probit models, Table 2 presents the marginal effect for a 1992 Conservative voter and Conservative Party identifier, with Ideology, Conservative support, and Labour support set to the means for such a voter, and all other variables set to the sample means.

[16] This estimate has a p-value of 0.016. We find substantively similar results if we either instrument 1997 readership with 1992 readership or simply use 1992 readership as the treatment variable.

measure of previous vote, and measuring the controls in 1996 instead of 1992.[17] With 1996 as the baseline, the treatment effect estimates increase somewhat. The probit coefficient for the treatment effect is .71 (s.e. = .18, n = 1330), 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. Finally, Table 2A in the Appendix shows that the treatment effect does not appear to be specific to a subset of the switching papers. It is robust among only readers of the *Sun* and only readers of the other three switching papers, though the estimate is larger for the latter.[18] In

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[17] Measuring explanatory variables in 1996 also allows the inclusion of controls for *Prior European Integration Views* and retrospective assessments of the *1996 Economy*, which were not asked in 1992. Including these variables leaves the treatment effect estimate unchanged, see Footnote 2.

[18] Readership of the three other switching papers is too small to estimate their independent effects with precision. We also considered possible mechanisms of persuasion. Is it the mere fact of the *Sun*'s endorsement, its support for particular policies, or its coverage of the campaign? The *Sun*'s official endorsement article primarily emphasized Major's incompetence and Blair's strong leadership skills (Seymour-Ure 1997). As one would expect for a tabloid, 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 switching paper readers did not differ from others on this question, these data fail to shed light on the mechanism of persuasion.

summary, the persuasive effect observed in Figure 1 does not appear to be an artifact of differences on the observed covariates. Reading one of the papers that switched to Labour appears to have persuaded people to vote for Labour.

4.2 Accounting for Possible Nonrandom Selection on Unobservables: Two Placebo Tests and a Sensitivity Analysis

While the persuasive effect does not appear to result from differences between the treated and untreated groups on the variables discussed above, it could arise from differences on variables we have failed to incorporate in the analysis, that is, from nonrandom selection into treatment and control groups on *un*observed characteristics. To address this concern, we conduct two placebo tests and a sensitivity analysis. The first placebo test assesses whether our covariates are capturing all relevant differences between the treated and untreated. Although the UK did not hold a general election in 1996, that year's wave asked how respondents would have voted had there been an election. If the covariates are capturing the key differences between the groups, we should find little difference between the treated and untreated respondents' 1996 vote intentions in the matched samples, because the treated had not yet received the treatment. The bottom row of Table 3 presents this placebo test. Even though we do not match on 1996 vote intention, differences on it largely disappear in the matched data. In the full sample, the treated were 6.6 percent more likely to vote for Labour in 1996. This difference falls to only 2.2 percent in the exactly matched sample, and 1.7 percent in the genetically matched sample, suggesting observed covariates successfully account for most of the difference in the tendency to vote for Labour in 1996.

The long panel allows us also to conduct a second placebo test to further address concerns about unobservables. With this long panel, we can examine individuals who read the *Sun*, *Daily Star*, *Independent*, or *Financial Times* in early waves of the panel, but stop reading these papers

before the Labour endorsements. Since these individuals are similar to our treated respondents on the key observed characteristic—initially 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, failing 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 (a difference of -3.59 percentage points (s.e. = 3.8)).

Finally, we can also address concerns about unobservables through a sensitivity analysis that examines the plausibility of an omitted variable creating the appearance of an effect in the absence of a true effect (Rosenbaum 2002). In our case, to take an illustrative example, for an omitted variable to have generated the 8.6 percent (bivariate) effect, it would have to do at least as good a job predicting 1997 vote as one's prior vote and be differentiated across readership by more than .3 on a one-point scale. Since none of the observed covariates differs this much across readership (the largest is Working-class identification at .134), an omitted variable or group of omitted variables seems unlikely to be the source of our findings. Thus, two placebo tests and a sensitivity analysis assuage concerns about bias from differences on unobservables.

4.3 Did Newspapers Follow Their Readers?

Another alternative explanation for our 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 (McNair 2003). To address this concern, we conduct a third placebo test by checking 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. Our dependent variable for this third placebo test is the vote intention question in the 1996 wave (used in the first placebo test). Figure 2 presents the persuasive effect, as in Figure 1, while also showing vote intention for the treated and untreated in 1996, just before the treatment. It further differentiates between two types of treatment groups: all readers (top panel) and habitual readers (bottom panel).[19] As expected, the treatment 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.[20]

In summary, the treated group's shift to Labour did not occur before the endorsement shifts, 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.

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[19] 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 untreated groups falls somewhat due to panel attrition in 1996. Also, an anomaly occurs in 1994, in which *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.

[20] The same pattern holds when we disaggregate the control group into Conservative paper readers, Labour paper readers, other or no affiliation paper readers, and those who did not read newspapers. In each case, the treatment effect emerges between the 1996 and 1997 panel waves. For these results, see Footnote 2.

4.4 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 critical of Major's government after the 1992 election. This coverage could have provoked Conservative supporters to drop these papers and Labour supporters to read them, leaving switching paper readers potentially more vulnerable to persuasion.

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. We also showed that the treatment effect is similar when we instrument 1996 readership with 1992 readership. To address this concern further, we examine newspaper readership across the panel, but find little evidence of self-selection by 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: 36.6 and 36.5 percent, respectively. 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.[21] Thus, there is little evidence of self-selection between the 1992 wave and the 1996 wave (when we measure readership).

[21] In the final wave of the panel, some evidence of self-selection does appear. 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.

Although Conservative supporters generally 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 if they dislike speaking with an interviewer about the seemingly dismal prospects of their party. To ensure that these difficult-to-persuade individuals do not drop out of the panel at higher rates, we check the attrition rates for various groups, but find no cause for concern. In fact, a higher percentage of 1992 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 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.[22]

5 Conclusion

Using panel data and matching techniques, we exploit a rare change in news slant and find strong evidence of news media persuasion. By comparing readers of newspapers that switched to similar individuals who did not read these newspapers, we estimate that these papers persuaded a considerable share of their readers to vote for Labour. We emphasize again the unusual confluence that permits us to estimate this persuasive effect while avoiding many of the methodological problems that plague previous studies. First, we have an uncharacteristic change in the partisan slant of newspapers. Second, we can measure individuals' exposure to these news outlets before the shift occurs. 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 standard parametric techniques. Finally, the many pretreatment panel waves in the BEPS allow us to address various

[22] For more details on panel attrition, see Footnote 2.

other potential sources of bias and conduct several placebo tests. To our knowledge, no other observational media persuasion study combines these attributes.

Depending on the statistical approach, our point estimates of the persuasive effect of news endorsements and slant vary from about 10 to as high as 25 percent of readers. If, in the 1997 UK election, the *Sun*'s endorsement was in exchange for a friendly regulatory environment for Murdoch, the concession may have bought Blair between 8 and 20 percent of his 3.9 million-vote margin over the Conservatives. The magnitude of this effect is not just larger than those found in previous press 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 endorsements and 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, see Ansolabehere et al. 2006).

Our analysis provides rare, compelling evidence that the news media exert a strong influence on mass political behavior. Indeed, it appears to be one of the most powerful influences on voting political scientists have been able to document. Consequently, it seems that the source of stability in democratic politics may not primarily be the behavior of citizens, but elites.

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					
Daily Express (Scottish)	5.6	1,525	1,220	Conservative	Muted Conservative Endorsement
Daily Mail	9.9	1,675	2,151	Conservative	Conservative
Daily Telegraph	5.5	1,038	1,134	Conservative	Conservative
Consistent Labour					
Daily Mirror/Record	18.1	2,903	3,084	Labour	Labour
Guardian	2.3	429	401	Labour	Labour
Switched to Labour					
Sun	9.7	3,571	3,842	Conservative	Labour
Daily Star	1.4	806	648	Tone Favors Conservatives	Labour
Independent	1.6	390	251	No affiliation	Labour
Financial Times	0.7	290	307	"Not a Tory Majority"	Labour
Other					
Times	2.5	386	719	Conservative	None (Euro-Sceptic)
Other/Not answered	12.3				
***n* =**	1608				

Coding of newspaper slant is based on Seymoure-Ure (1997), Norris (1998) and Scammell (1997). Readership percentages from the 1996 wave of the BEPS. Circulation data from Seymoure-Ure (1997).

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

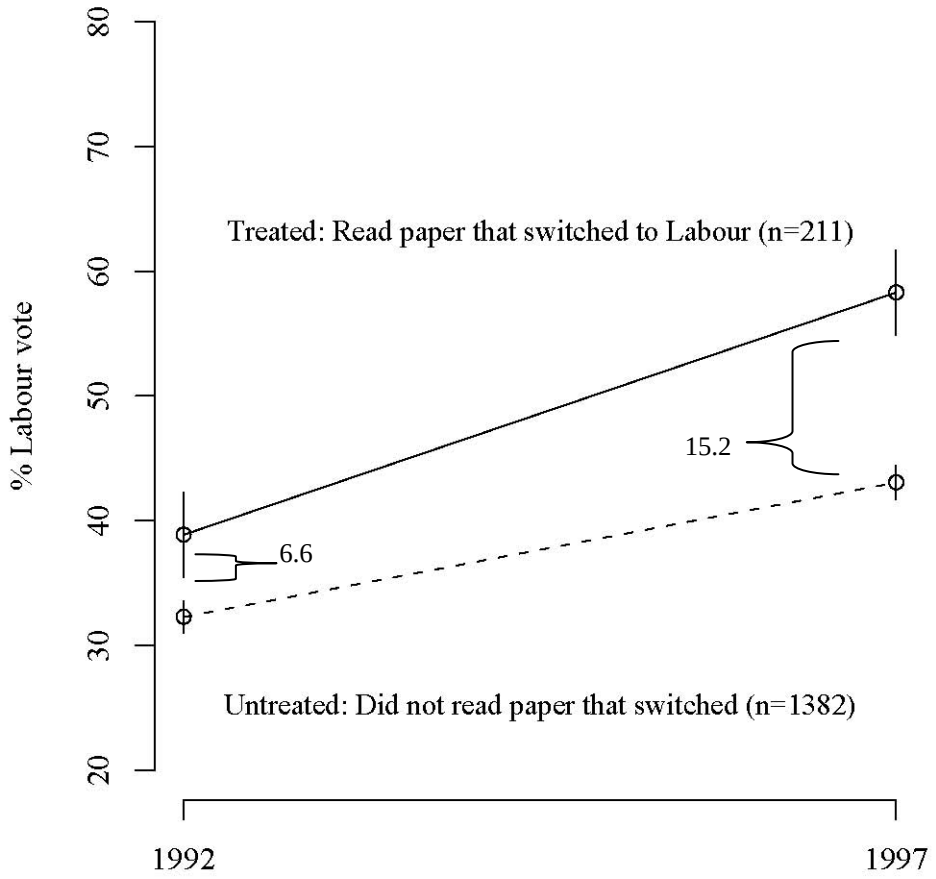
			Preprocessed with Matching				**Instrumented with 1992 Readership**
			Exact on Selected Variables		**Genetic on All Variables**		
	Bivariate	Multivariate (Probit)	Bivariate	Multivariate (Probit)	Bivariate	Multivariate (Probit)	
Among All Readers							
Treatment Effect (%)	8.6	12.2	10.9	14.0	10.4	9.6	10.9
(Standard error)	(3.0)	(3.6)	(4.1)	(6.0)	(4.3)	(4.9)	(5.2)
n Treated / *n* Control	211/1382	211/1382	192 /192	192 /192	211/211	211/211	211/1382
Among Habitual Readers							
Treatment effect (%)	12.7	23.1	17.9	23.4	15.8	25.7	13.4
(Standard error)	(4.1)	(6.4)	(5.4)	(11.3)	(6.6)	(9.0)	(5.6)
n Treated / *n* Control	102/1382	102/1382	95/95	95/95	102/102	102/102	102/1382

This table shows that reading a newspaper that switched to Labour in the 1997 election led voters to 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 treatment effect and 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 Table 1A in the Appendix. Exact matching is performed using Labour Vote, Conservative Vote, Liberal Vote, Labour Identification, Conservative Identification, Liberal Identification, 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 were not habitual readers of switching papers are excluded from the analysis. Standard errors are in parentheses.

Table 3: Comparing Covariates among the Treated and Untreated Groups

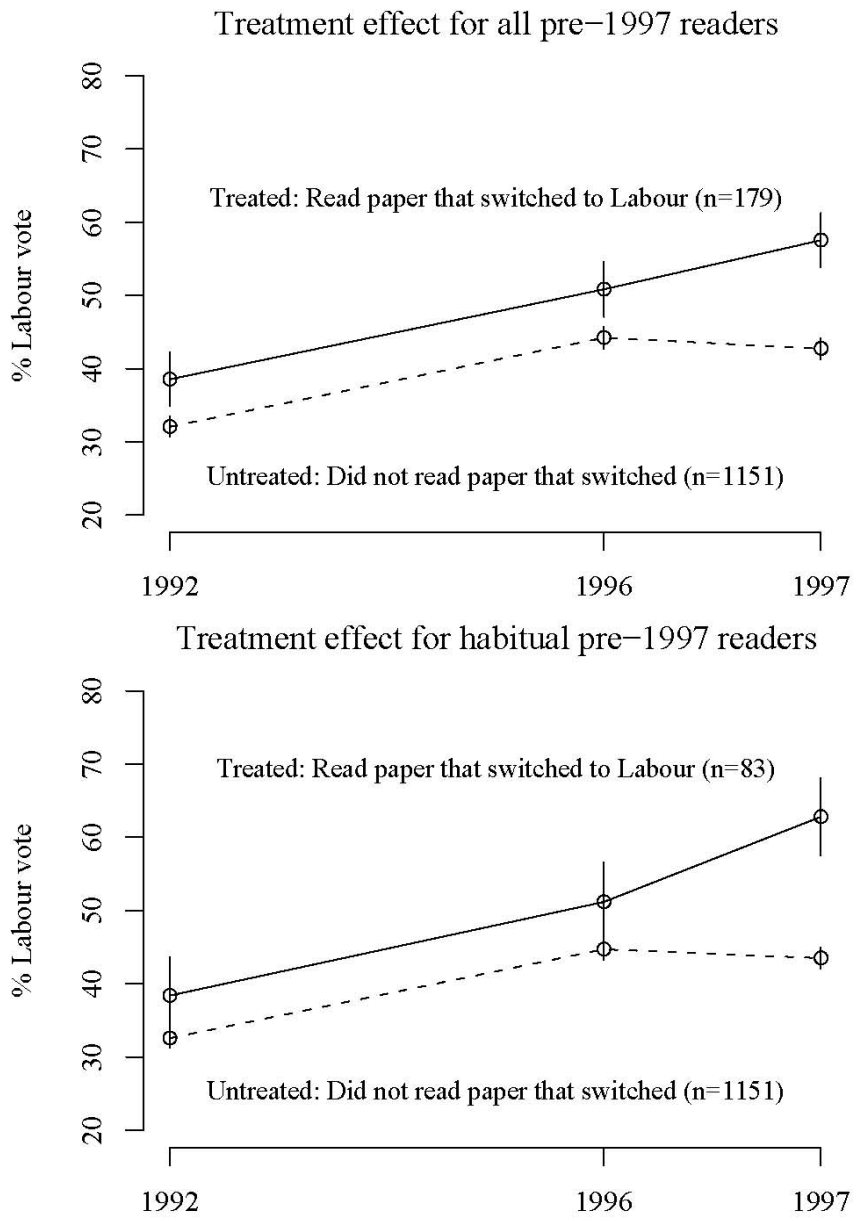
	All		***Difference*** (Treated minus Untreated)		
Covariates (measured in 1992)	**Treated**	**Untreated**	**All**	**Exact**	**Genetic**
Prior Labour Vote (Labour 1, Other 0)	0.389	0.323	0.066	0.000	0.000
Prior Conservative Vote (Conservative 1, Other 0)	0.389	0.404	-0.015	0.000	0.000
Prior Liberal Vote (Liberal 1, Other 0)	0.156	0.188	-0.032	0.000	0.000
Prior Labour Party Identification (Labour 1, Other 0)	0.337	0.314	0.022	0.000	-0.005
Prior Conservative Party Identification (Conservative 1, Other 0)	0.412	0.418	-0.007	0.000	0.005
Prior Liberal Party Identification (Liberal 1, Other 0)	0.133	0.154	-0.021	0.000	0.005
Prior Labour Party Support (Strongly Favor 1 to Strongly Oppose 0)	0.488	0.462	0.025	0.000	-0.005
Prior Conservative Party Support (Strongly Favor 1 to Strongly Oppose 0)	0.524	0.522	0.003	0.000	0.005
Prior Political Knowledge (High 1, Mid .5, Low 0)	0.545	0.671	-0.126	0.000	-0.007
Prior Television Viewer (Yes 1, No 0)	0.218	0.289	-0.071	-0.083	0.009
Prior Ideology (Liberal 1 to Conservative 0)	0.550	0.535	0.015	0.003	0.002
Prior Ideological Moderation (Moderate 1 to Extreme 0)	0.650	0.652	-0.003	0.013	-0.011
Prior Authoritarianism (Low 1 to High 0)	0.537	0.528	0.009	0.014	0.006
Prior Trade Union Member (Yes 1, No 0)	0.218	0.240	-0.022	-0.010	0.019
Prior Working-Class Identification (Working Class 1, Other 0)	0.716	0.581	0.134	0.068	-0.001
Parents Voted Labour (Yes 1, No 0)	0.436	0.354	0.082	0.062	0.000
Prior Coping with Mortgage (Difficult 1 to Not Difficult/NA 0)	0.291	0.337	0.046	0.000	0.018
Prior Education (College 1 to No Education 0)	0.598	0.514	0.105	0.089	0.008
Prior Income (High 1 to Low 0)	0.469	0.386	-0.074	-0.058	-0.009
Prior Age (Old 1 to Young 0)	0.464	0.453	-0.018	-0.020	0.003
Gender (Male 1, Female 0)	0.508	0.442	-0.101	-0.115	0.000
White (White 1, Non-white 0)	0.986	0.976	0.010	0.021	0.000
n	211	1382	211/1382	192/192	211/211
Additional Balance Check					
Labour Vote Intention in 1996 (Labour 1, Other 0)	0.508	0.442	0.066	0.022	0.017

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 (15.2 - 6.6 =) 8.6-percentage point shift to Labour between the 1992 and 1997 UK elections. Paper readership is measured in the 1996 wave, before the papers switched, or, if no 1996 interview was conducted, in an earlier wave. Confidence intervals show one standard error.

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 in Figure 1. Confidence intervals show one standard error.

Appendix

Table 1A: Probit Models of Vote in 1997 UK General Election

Explanatory variables		***Preprocessed with***		
(measured in 1992 except Treatment**)**	***All***	**Exact on Selected Variables**	**Genetic on All Variables**	**Treatment Instrumented with 1992 Measure**
Treatment (1996 or before)	0.48	0.58	0.39	0.11
	(0.13)	(0.24)	(0.19)	(0.05)
Prior Labour Vote	0.88	1.43	0.24	0.28
	(0.19)	(0.52)	(0.41)	(0.05)
Prior Conservative Vote	-0.25	0.13	-0.52	-0.08
	(0.20)	(0.54)	(0.44)	(0.05)
Prior Liberal Vote	-0.27	0.19	-0.60	-0.08
	(0.19)	(0.54)	(0.44)	(0.05)
Prior Labour Party	0.88	0.24	0.52	0.22
	(0.24)	(0.63)	(0.55)	(0.05)
Prior Conservative Party	-0.33	-0.26	-0.11	-0.06
	(0.23)	(0.62)	(0.54)	(0.05)
Prior Liberal Party	0.57	0.73	0.99	0.16
	(0.18)	(0.46)	(0.35)	(0.04)
Prior Labour Party Support	0.04	-0.63	-0.45	0.00
	(0.18)	(0.46)	(0.36)	(0.04)
Prior Conservative Party	0.37	-0.17	0.43	0.09
Support	(0.18)	(0.53)	(0.43)	(0.04)
Prior Middle Political	-0.26	-0.28	-0.19	-0.06
Knowledge	(0.12)	(0.29)	(0.23)	(0.03)
Prior High Political	-0.37	-0.27	-0.53	-0.08
Knowledge	(0.14)	(0.34)	(0.31)	(0.03)
Prior Television Viewer	0.14	0.16	-0.11	0.02
	(0.10)	(0.25)	(0.24)	(0.02)
Prior Daily Newspaper Reader	-0.18	-0.37	0.07	-0.03
	(0.10)	(0.28)	(0.26)	(0.02)
Prior Ideology	1.08	0.12	-0.05	0.14
	(0.58)	(1.26)	(1.28)	(0.10)
Prior Ideological Moderation	0.53	-1.06	-0.51	0.01
	(0.44)	(0.95)	(0.93)	(0.08)
Prior Authoritarianism	-0.16	0.76	-0.38	-0.00
	(0.36)	(0.81)	(0.80)	(0.08)
Prior Trade Union Member	0.05	0.45	0.44	0.01
	(0.10)	(0.27)	(0.25)	(0.02)
Prior Working-Class	0.16	0.33	0.36	0.04
	(0.10)	(0.24)	(0.23)	(0.02)

Parents Voted Labour	0.12	-0.02	-0.19	0.03
	(0.09)	(0.24)	(0.21)	(0.02)
Prior Coping with Mortgage	-0.20	-0.36	-0.19	-0.01
	(0.09)	(0.30)	(0.17)	(0.00)
Prior Education: O Level	0.11	-0.44	0.33	0.02
or Equivalent	(0.17)	(0.53)	(0.46)	(0.04)
Prior Education: A Level	0.09	0.78	0.12	0.03
or Equivalent	(0.19)	(0.54)	(0.47)	(0.04)
Prior Education: Some Higher	-0.04	-0.19	-0.08	-0.01
Education	(0.19)	(0.50)	(0.46)	(0.04)
Prior Education: College	0.07	0.04	-0.10	0.02
	(0.19)	(0.51)	(0.49)	(0.04)
Prior Income: £6000-£11,999	0.29	0.68	0.19	0.07
	(0.15)	(0.35)	(0.28)	(0.03)
Prior Income: £12,000-	-0.06	-0.24	-0.62	0.00
£19,999	(0.15)	(0.35)	(0.32)	(0.03)
Prior Income: £20,000+	-0.05	0.06	-0.48	0.01
	(0.16)	(0.37)	(0.36)	(0.03)
Prior Income: No Income	-0.06	0.02	-0.07	0.01
Given	(0.19)	(0.41)	(0.43)	(0.04)
Prior Age: 25-34	-0.42	-0.28	-0.70	-0.10
	(0.20)	(0.47)	(0.44)	(0.04)
Prior Age: 35-44	-0.27	-0.21	-0.32	-0.06
	(0.20)	(0.45)	(0.43)	(0.04)
Prior Age: 45-54	-0.42	-0.21	-0.51	-0.10
	(0.21)	(0.49)	(0.46)	(0.05)
Prior Age: 55-59	-0.74	-0.40	-0.05	-0.16
	(0.24)	(0.61)	(0.57)	(0.05)
Prior Age: 60-64	-0.35	-0.29	-0.28	-0.09
	(0.25)	(0.60)	(0.57)	(0.05)
Prior Age: 65+	-0.54	-0.24	-0.89	-0.12
	(0.23)	(0.53)	(0.52)	(0.05)
Prior Age: not given	-0.72	0.11	-1.13	-0.17
	(0.44)	(0.87)	(0.91)	(0.09)
Gender	-0.13	-0.15	-0.23	-0.03
	(0.10)	(0.25)	(0.24)	(0.02)
White	-0.84	0.32	-0.14	-0.20
	(0.30)	(1.16)	(0.73)	(0.06)
Prior Profession: Employer/	-0.08	1.03	-0.18	0.01
Business Owner	(0.38)	(1.29)	(0.73)	(0.08)
Prior Profession: Professional	-0.21	1.53	0.53	-0.02
	(0.42)	(1.47)	(0.85)	(0.09)
Prior Profession: Non-manual	-0.05	1.29	0.35	0.01
Laborer	(0.36)	(1.25)	(0.68)	(0.07)

Prior Profession: Personal Service	0.07 (0.41)	2.18 (1.31)	0.43 (0.75)	0.05 (0.08)
Prior Profession: Manual	-0.07 (0.37)	1.10 (1.26)	0.03 (0.68)	0.01 (0.08)
Prior Profession: Other	-0.60 (0.41)	0.84 (1.31)	-1.14 (0.80)	-0.10 (0.08)
Constant	0.13 (0.86)	-1.50 (2.39)	1.58 (1.90)	0.50 (0.17)
Log likelihood	-	-471.036	-165.499	
n	1593	384	422	1593

All covariates are measured in 1992 and coded to vary between 0 and 1. All models also include fixed effects for region, with coefficients not reported. Table 2 uses the estimates from this table in its marginal effects calculations. Standard errors are in parentheses.

Table 2A: 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)
n Treated / *n* Control	159/1382	159/1382	152/1006	152/1006	159/141	159/141
Non-Sun Treated (other treated excluded)						
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)
n Treated / *n* 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.* Standard errors are in parentheses.

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