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TE KAUNIHERA Ā-ROHE O TE MATAU-A-MĀUI

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Moderators of Candidate Name-Order Effects in Elections: An Experiment

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Past studies of elections have shown that candidates whose names were listed at the beginning of a list on a ballot often received more votes by virtue of their position. This article tests speculations about the cognitive mechanisms that might be responsible for producing the effect. In an experiment embedded in a large national Internet survey, participants read about the issue positions of two hypothetical candidates and voted for one of them in a simulated election in which candidate name order was varied. The expected effect of position appeared and was strongest (1) when participants had less information about the candidates on which to base their choices, (2) when participants felt more ambivalent about their choices, (3) among participants with more limited cognitive skills, and (4) among participants who devoted less effort to the candidate evaluation process. The name-order effect was greater among left-handed people when the candidate names were arrayed horizontally, but there was no difference between left- and right-handed people when the names were arrayed vertically. These results reinforce some broad theoretical accounts of the cognitive process that yield name-order effects in elections.

KEY WORDS: ballot name order, primacy effect, information, ambivalence, cognitive skill, cognitive effort

Psychologists have long been fascinated with order effects in social influence and choice. For example, since the early days of systematic research on persuasion, the order in which opposing messages are presented has been recognized to influence their impact on attitudes (e.g., Haugtvedt & Wegener, 1994; Hovland, Campbell, & Brock, 1957). The order of information can also be important in forming impressions of people (e.g., Anderson & Hubert, 1963; Asch, 1946; Forgas, 2011; Jones & Goethals, 1972; Webster, Richter, & Kruglanski, 1996). When answering multiple-choice questions in questionnaires, people are influenced by the order in which the options are presented (e.g., Krosnick & Alwin, 1987). When taste-testing foods or beverages, people are influenced by the order in which products are experienced (e.g., Dean, 1980; Mantonakis, Rodero, Lesschaeve, & Hastie, 2009). Articles published more toward the front of an issue of a journal are cited more than ones that appear later (Berger, 2011). And in elections, the order of candidate names

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on ballots influence election outcomes (e.g., Miller & Krosnick, 1998). This article focuses on order effects in this latter context.

Ballots offer a unique case of simultaneous option presentation (Mogilner, Shiv, & Iyengar, 2013) that has been the focus of recent research. Elections are, of course, processes of communication in which collectivities of individuals express their preferences. The effectiveness of such communication hinges in part upon the fairness and transparency of the process. In principle, structural features of the electoral process should not award advantages or disadvantages to any of the candidates. That is, democracy requires a procedurally "level playing field" on which every candidate has equal access to victory. Yet much research done during the last 50 years has pointed to one aspect of ballots that do sometimes appear to cause bias in election outcomes: the order of candidates' names (for a review, see Krosnick, Miller, & Tichy, 2004).

This article offers a theory of the psychology underlying name-order effects, which yields a list of potential moderators of the effect. Tests of the impact of these moderators afford the opportunity to test the logic of the generative theory. Unfortunately, real elections are limited in the degree to which they can be used to reveal the psychology of name-order effects, because it is impossible to measure many attributes of individual voters, and it is impossible to manipulate attributes of the candidates and of campaigns. So we carried out tests in a context that did allow for such measurements and manipulations: a simulated two-candidate election conducted within an Internet survey of a national sample of American adults. Some moderators that cannot be easily measured in real electoral settings were measured, and some posited moderators were manipulated across participants to allow for strong causal inference. We begin below by reviewing the existing literature on name-order effects and their moderators, outline the theory and predictions about moderators, and then describe the methods, findings, and implications of the experiment.

Name-Order Effects in Elections

The body of research on name-order effects indicates that candidates often received more votes when their names were listed first than when their names were listed after the names of one or more candidates with whom they competed. Although one recent study failed to find so-called "primacy effects" (Alvarez, Sinclair, & Hasen, 2006), and another found them to occur only for candidates not affiliated with major political parties (Ho & Imai, 2008), many other studies have found evidence suggesting that primacy effects often occur (e.g., Koppell & Steen, 2004; Krosnick et al., 2004; Miller & Krosnick, 1998).

For instance, Miller and Krosnick (1998) examined the 1992 general elections in the three largest counties in Ohio, where candidate name order was rotated from precinct to precinct. Statistically significant name-order effects appeared in 48% of the races. The advantage of being in first position was an average of 2.5%. Even in races where the name-order effect was not statistically significant, 75% of the observed vote-share differences were in the direction of primacy effects, a result very unlikely to have occurred by chance alone and suggesting that primacy effects were pervasive.[1]

More recently, Koppell and Steen (2004) found that among 180 candidates running in the 1998 Democratic primary elections in New York City, 161 received more votes when listed first. The average first-position advantage was about 3.4 percentage points, and the largest boost was 14.5 points. Krosnick and colleagues (2004) also found that in the race for President of the United States

[1] Past studies of name-order effects have often entailed design limitations that compromise the clarity of their empirical demonstrations (for a review, see Miller & Krosnick, 1998). One common design drawback was a lack of random assignment to different name orders, which made it impossible to disentangle order effects from other conflated factors. Another factor limiting some past studies was improper statistical significance testing, which prevented researchers from distinguishing reality from illusion.

in 2000, 19 of 21 tests (for seven candidate pairs running in California, North Dakota, and Ohio) manifested a trend in the direction of primacy, nine of which were statistically significant. Ho and Imai (2006) found name-order effects for some candidates running in the 2003 California gubernatorial recall election. And Ho and Imai (2008) found name-order effects in primaries and in statewide general elections in California held between 1978 and 2002.

Moderators of Name-Order Effects

If primacy effects are indeed real, why do they occur? Why would any citizen go to the trouble of going to the polls and then cast a vote that is influenced by something so trivial as the order of the names on the ballot? And when are name-order effects most likely to occur?

Some past studies have explored the conditions under which primacy effects are most likely to appear and the characteristics of voters who are most likely to manifest these effects (e.g., Koppell & Steen, 2004; Miller & Krosnick, 1998). But these studies have been limited in their ability to do so, because they studied voting behavior of individuals aggregated into precincts or districts and did not collect information about the characteristics of individual voters. Furthermore, some proposed moderators, such as the amount of information that the news media disseminated to voters about the candidates, were studied observationally, limiting the strength of causal inferences that could be made.

Why Name-Order Effect Might Occur

One possible explanation for primacy effects involves the tremendous burden levied on voters in the context of American democracy. Being a "good citizen" requires considerable time and effort, much like the responsibilities of a part-time job. On most ballots, numerous candidates vie for a voter's support, and the issues at stake in any campaign are often complex. This means that developing solid knowledge with which to choose between candidates is challenging. Some races receive so little media attention that even very effortful voters have trouble detailing the job responsibilities involved and the credentials, track records, and philosophies of the competing candidates.

As a result, people may sometimes vote for candidates and on referenda with relatively little relevant information. If a voter walks into a voting booth with a clear preference for a candidate in a highly visible race, possesses little to no information about races further down on the ballot, and feels obligated to vote in those latter races in order for his or her ballots to be counted, voters may choose a candidate in each such race arbitrarily. Such arbitrary choices may be biased toward selecting the first name listed. Thus, name-order effects may be most likely when voters are uninformed about the candidates running in a particular race. We refer to this as the "information deficit" hypothesis.

However, name-order effects might also sometimes occur under very different conditions: when voters are very well informed. Consider a voter who has devoted great effort to learning about candidates competing for President of the United States and has discovered an array of reasons to vote for and against each one. When he or she finally walks into the voting booth, making a choice between the candidates might be very difficult, because the pros and cons of the candidates nearly balance out. As a result, when under pressure to make a choice and move on with life, the voter again might choose arbitrarily, and in such situations, a bias toward the first-listed name might occur.[2] Thus, name-order effects might occur due to ambivalence. We refer to this as the "ambivalence" hypothesis.

[2] Ambivalence towards candidates is not uncommon. For example, one study suggested that about 30% of the electorate hold ambivalent attitudes toward the major American political parties (Basinger & Lavine, 2005). As would be expected, more ambivalent citizens take longer to crystalize their preferences (Lavine, 2001).

Factors that Precipitate Name-Order Effects

The information deficit and ambivalence hypotheses suggest a series of possible moderators of the name-order effect.

Information volume. In light of the information deficit hypothesis, name-order effects might be especially likely to occur when voters know little about the candidates competing in a race. Consistent with this logic, Miller and Krosnick (1998) found that primacy effects were smaller in races that had been covered more frequently in the news. Volume of media attention to a race seems most likely to be positively correlated with the amount of knowledge that voters gain about the candidates. But the observational nature of such analysis precludes drawing the strongest causal conclusions about the effects of knowledge volume, so this hypothesis merits further study.

Ambivalence. No past research has yet explored whether name-order effects are especially likely to occur among voters who feel deeply ambivalent about the candidates, seeing pros and cons to both and unable to comfortably choose between them. However, previously reported evidence that primacy effects have occurred in the race for President of the United States (a superpublicized contest) seems unattributable to lack of information. This leaves open the possibility that ambivalence might incline some voters to grab the first name they see.

Cognitive skills. In light of the information deficit hypothesis, another potential moderator of name-order effects might be a voter's level of cognitive skills. The term "cognitive skills" refers to the ensemble of abilities that enable interpreting incoming information, storing it in memory, retrieving the information later, and integrating the retrieved information in order to select between candidates. Voters who have strong cognitive skills are likely to accumulate relatively large stores of information about candidates, whereas people with more limited cognitive skills may be exposed to information about candidates but may be less able to encode it, to retain it, and to use it to form thoughtful judgments. Therefore, these individuals may be less equipped to vote on substantive bases, so they may be more susceptible to influence by name order.

Cognitive effort. Even if a person is able to perform the cognitive tasks involved in encoding, storing, retrieving, and integrating information about candidates to choose between them, the individual may not be motivated to do this cognitive work. If that is the case, even after exposure to an array of information, a person may end up relatively uninformed about the candidates. A state of low information might exacerbate the likelihood of manifesting name-order effects. Some voters may exert effort when encountering information about candidates because they have a general tendency to process all information carefully (e.g., Cacioppo & Petty, 1982). Other voters may exert effort because they care about politics in particular and enjoy thinking carefully about that topic (e.g., Glenn & Grimes, 1968). Whatever the cause, expending more cognitive effort to learn and think about candidates may attenuate name-order effects.

Name-array orientation. Another possible moderator is the layout of candidates' names on the ballot. Some ballots present candidate names in a horizontal array, with names next to one another. Other ballots present candidate names in a vertical array, with some names above others. And still other ballots present names in grids involving both vertical and horizontal arrays of names. No prior study has examined whether horizontal versus vertical presentation of candidate names affects voters' choices. Some scholars in educational testing (e.g., Haladyna, Downing, & Rodriguez, 2002) and questionnaire design (e.g., Dillman, Smyth, & Christian, 2009) have recommended presenting response choices vertically rather than horizontally. In line with this recommendation, we explored whether the name-order effect would be weaker when names were presented vertically than when presented horizontally.

Handedness. Primacy effects have generally been assumed to result from the temporal order in which voters encounter the names on a ballot as they read from top to bottom or left to right. Could ballot-order effects arise from the spatial positions of names on a ballot, as well as (or instead of) their temporal order?

The spatial positions of items on a page or a computer screen can influence a wide variety of judgments and behaviors. Numerous studies suggest that people implicitly associate "up" with "good" and "down" with "bad" (e.g., Casasanto, 2009; Meier & Robinson, 2004; Stepper & Strack, 1993; Tourangeau, Couper, & Conrad, 2013). These associations are present in linguistic idioms and metaphors (e.g., "high on life"; "down in the dumps") and are embodied in physical movements as well (e.g., standing tall when feeling proud and slouching when feeling sad). When voters demonstrate what appears to be a preference for the *first* candidate in a list of names, could they really be showing a preference for the *top* candidate? Given a vertical array of names, it may be impossible to distinguish the influences of temporal (or numerical) order from those of spatial position: spatial height and temporal primacy are perfectly confounded as voters read from the top to the bottom of a list. Both spatial height and temporary primacy might yield a tendency to vote for the candidate whose name is listed at the top of the list.

By examining name-order effects when names are arrayed horizontally, however, it may be possible to tease apart influences of temporal primacy and spatial position. In addition to associations between vertical space and positivity, linguistic and cultural conventions also suggest associations between "goodness" and horizontal space (e.g., "My right hand man"; "two left feet"). A series of experiments shows that people associate goodness with left-right space implicitly, but not always in the way cultural conventions suggest they should. Right-handers tend to associate "good" with "right" and "bad" with "left," consistent with idioms in language, but left-handers show the opposite implicit associations. For lefties, the left side is the "good" side of space, and the right side is the "bad" side. As a consequence of these implicit associations, people tend to evaluate things they encounter on their dominant side of a page or a computer screen more favorably. When asked to decide which of two products to buy, which job applicant to hire, or which alien creature looks more honest, intelligent, or attractive, right- and left-handers respond differently: right-handers tend to prefer the product, person, or creature presented on their right side, but left-handers tend to prefer the one on their left (Casasanto, 2009).

The association between "good" and one's dominant side of space was discovered through tests of the body-specificity hypothesis (Casasanto, 2009): the proposal that people with different kinds of bodies may form correspondingly different thoughts, feelings, and judgments (Casasanto, 2011, for review). The link between "good" and "dominant side" appears to be mediated by motor fluency. People associate "good" with the side of space on which they can habitually act more fluently, using their dominant hand, and "bad" with the side on which they act more clumsily using their nondominant hand (Casasanto & Chrysikou, 2011). This association can be detected even when people make judgments orally without using their hands to respond, and it has been shown to influence people's reaction times for judging the meanings of positive and negative words (de la Vega, de Filippis, Lachmair, Dudschig, & Kaup, 2012) and their memories for the locations of events with positive and negative emotional valence (Brunyé, Gardony, Mahoney, & Taylor, 2012).

Beyond the laboratory, body-specific associations between space and valence have been observed in the speech and gestures of right- and left-handed U.S. presidential candidates during televised debates from the 2004 and 2008 elections (Casasanto & Jasmin, 2010). For right-handers (Bush, Kerry), right-hand gestures were more strongly associated with positive-valence speech, and left-hand gestures with negative-valence speech. But the opposite associations were observed in left-handers' speech and gestures (McCain, Obama). Here, we investigated whether handedness-based implicit associations interact with the left-right placement of names on a ballot to bias people's voting behaviour.

If name-order effects in voting are, in fact, due to temporal or numerical sequence in which voters encounter the names on a ballot, then these effects should not differ between right- and left-handers. Regardless of whether names are arrayed vertically or horizontally, people in English-speaking cultures read from top to bottom and left to right, regardless of their handedness, so name-order effects should be the same. But if primacy effects are exclusively due to spatial position and *not* to temporal order, then left- and right-handers should show similar effects of name order with a vertical array but opposite effects of order with a horizontal array. Left-handers should manifest a tendency to vote for candidates who appear on the left, and right-handers should manifest a tendency to candidates who appear on the right. If both spatial position and temporal order bias voters' selections, then a hybrid effect should appear. For example, with horizontal presentation, all voters may be biased toward voting for the candidate whose name appears on the left (due to temporal ordering), and this bias may be stronger among left-handers than among right-handers (due to spatial position).

Overview of the Study

To permit testing these hypotheses, participants participated in a simulated election in an Internet survey. They read information about two hypothetical candidates and voted for one of them. Experimental manipulations varied the amount of information that voters received about the candidates, the order of the candidate names in the voting question, and whether the candidate names were presented vertically or horizontally. We explored the impact of a variety of purported moderators, including the cognitive skills, cognitive effort, ambivalence, and handedness of the voters and the orientations and positions of the candidates' names on the simulated ballot.

Method

Sample

Participants were a national sample of American adults recruited by Luth Research.[3] From among 1.4 million members of Luth Research's survey panel (who had signed up to complete online surveys), a stratified random sample of 83,986 people was selected, with demographic strata specified to resemble the distributions of gender, age, household income, ethnicity, region, and education in the U.S. adult population according to the 2000 U.S. Census. E-mail invitations were sent to these individuals, and data were collected between October 7 and October 21, 2009, from 2,069 (completion rate = 2.5%); 572 of these individuals answered questions used in the present study. We report results using data only from these individuals.[4]

The unweighted sample resembled the nation (as gauged by the 2009 Current Population Survey conducted by the U.S. Census Bureau, 2009) with regard to age, race, ethnicity, and gender but diverged from it with regard to education and income (compare columns 1 and 2 in Table 1). The participating participants underrepresented people with relatively little education and people with high incomes.

[3] Panel members were recruited via a variety of methods. Initially, random digit dial (RDD) telephone calls were made to invite a random sample of American adults to sign up to receive e-mail invitations to participate in surveys, yielding about 2,500 panel members. Additional telephone calls were made to individuals working in the information technology sector who were on professional lists; these calls yielded about 2,500 more panel members. These initial 5,000 panel members were offered a chance to win cash or gift certificates if they referred friends or family who might sign up to complete online surveys. Panel members were also recruited through online ads and through e-mails sent by companies and organizations with which the potential panelist was affiliated.

[4] This excludes 35 participants who did not answer the following question accurately in the middle of the survey: "To help us make sure our website is working properly, please select the number seven below." The response options were integers ranging from 1 to 7.

Table 1. Demographics of the Weighted and Unweighted Samples Compared to the Population

Variable	Unweighted Survey Sample	2009 Current Population Survey[a]	Weighted Survey Sample
Age			
18–24	8.5%	12.6%	12.6%
25–34	18.5	17.9	17.9
35–44	15.6	18.2	18.2
45–54	19.2	19.5	19.5
55–64	17.6	15.1	15.1
65 and up	20.5	16.6	16.6
Total	100.0%	100.0%	100.0%
Race			
White	81.2%	81.2%	81.4%
African American	12.0	11.8	11.8
Other/Multiple	6.8	7.0	6.9
Total	100.0%	100.0%	100.0%
Hispanic Origin			
Non-Hispanic	91.1%	86.3%	86.1%
Hispanic	8.9	13.7	13.9
Total	100.0%	100.0%	100.0%
Gender			
Female	44.0%	51.5%	51.3%
Male	56.0	48.5	48.7
Total	100.0%	100.0%	100.0%
Income			
<$20,000	17.6%	13.4%	12.8%
$20,000–$34,999	17.2	14.7	14.5
$35,000–$49,999	15.8	13.6	13.3
$50,000–$74,999	25.2	20.0	19.9
$75,000–$99,999	11.6	13.8	13.9
≥$100,000	12.5	25.4	25.6
Total	100.0%	100.0%	100.0%
Education			
No High School Degree	2.5%	14.1%	12.6%
High School Degree	18.7	30.9	31.4
Some College/AA Degree	38.8	28.0	28.4
BA Degree or More	40.0	27.1	27.5
Total	100.0%	100.0%	100.0%
N	572	239,205	572

[a]*Source*: U.S. Census Bureau Current Population Survey, Annual Social and Economic Supplement, March 2009. (Adult population only)

Weights

To correct for these demographic discrepancies, we followed procedures recommended by the American National Election Studies (ANES) for building weights optimally (DeBell & Krosnick, 2009) using software called the ANES Weighting Algorithm (the AWA; Pasek, DeBell, & Krosnick, 2010). The AWA uses an iterative, multiplicative approach to generate weights, which adjusts the vector of weights in each iteration by comparing the sample marginals with the population marginals (Pasek, 2010). Age (in six categories), education level (four categories), household income (six categories), race (White, African American, other/multiple), Hispanic ethnicity (Hispanic, non-Hispanic), and gender (female, male) were used as the raking variables. The weights were capped at

5.00. The procedure corrected well for the discrepancies between the study participants and the population; the poststratified participants closely resembled the nation (compare the second and third columns of Table 1).

Questionnaire Overview

Participants first answered general questions assessing political interest, party identification, and preferences on policy issues. Participants were then told that they would be asked to vote after reading a series of statements made by two hypothetical candidates running hypothetically for a seat in the U.S. Congress: Alan Mitchell and Robert Swanson.[5] Each screen displayed one statement from one candidate, and participants reported how much they agreed or disagreed with each statement. On every issue for every respondent, Alan Mitchell's position was presented before Robert Swanson's.[6] After reading all of the statements, participants voted for one of the two candidates. Additional questions assessed ambivalence, cognitive effort, political knowledge, and demographics.

Experimental Design

Each participant was randomly assigned to receive either a large amount of information about the candidates or a small amount, either to see Mitchell's name listed first or to see Swanson's listed first in the vote question, and to see the candidates' names either in a horizontal array or in a vertical array. Information volume was manipulated by assigning some participants to read about the candidates' positions on four issues (health care, immigration, stem cell research, and education), and the remaining participants read about those four issues plus four others: gun control, global warming and the environment, the war in Iraq, and tax reform. These manipulations were represented in our analysis by three binary variables: Name order (coded 0 for people who saw Swanson's name first and 1 for people who saw Mitchell's name first), Information (coded 0 for people who read about eight issues and 1 for people who read about four issues), and Array (coded 0 for people who saw the candidates' names vertically and 1 for people who saw the names horizontally).

The information presented to each participant was designed so that his or her own positions on the issue were expressed by Mitchell, and Swanson always took the opposite positions. To accomplish this, each participant's answers to policy attitude questions asked early in the survey were used to determine which candidate statements would be attributed to which candidate in the information presentation that the participant saw later. For example, if a participant opposed school vouchers (in response to this question: "Do you favor or oppose the government giving vouchers to parents to pay for some of the fees for a child to attend a private school instead of attending his or her local public school?"), that participant later read the following statements by the candidates on the issue of education:

> *Alan Mitchell*: "Public schools are an important component of American society. We should try to improve public schools rather than destroying them with vouchers."
>
> *Robert Swanson*: "I believe that the government should not be in the business of running schools. I think parents should receive state-funded vouchers, which would pay for any type of schooling the parents choose."

[5] "Alan" is the first name of 0.2% of Americans; "Robert" is the first name of 3.1%; "Swanson" is the last name of 0.02% of Americans; and "Mitchell" is the last name of 0.1% (U.S. Census Bureau, 2010). Thus, these names are roughly comparable in terms of familiarity and popularity.

[6] Bruine de Bruin (2005) found that objects presented later in a sequence were evaluated more positively. Thus, perhaps presenting Swanson's issue positions after Mitchell's advantaged Swanson. This was a constant across all participants and was therefore not confounded with any other variables of interest here.

Measures

Vote choice. Participants were asked: "Based on what you read, if an election were held today and you had to choose between the two candidates, who would you vote for?" Responses were coded 1 for Mitchell and 0 for Swanson.

Ambivalence. Immediately after answering the vote-choice question, participants reported how conflicted they felt when choosing between the two candidates and how hard it was to make the choice, on 5-point rating scales (extremely conflicted/hard, very conflicted/hard, moderately conflicted/hard, slightly conflicted/hard, not conflicted/hard at all). This is a meta-psychological measure of ambivalence (Holbrook & Krosnick, 2005), which taps the subjective experience of evaluative conflict regarding an object (see Cacioppo, Gardner, & Berntson, 1997; Thompson, Zanna, & Griffin, 1995). Responses were coded to range from 0 (indicating low ambivalence) to 1 (indicating high ambivalence) and were averaged to yield an index of ambivalence, which was dichotomized at the median.[7] Participants who were high in ambivalence were coded 1, and participants who were low in ambivalence were coded 0.

Cognitive skills. Educational attainment was used as a proxy measure of cognitive skills, because years of education correlate extremely strongly with scores on direct tests of cognitive skills (Ceci, 1996). Educational attainment was measured by asking participants to report the highest grade in school they had completed or the highest degree they had received. Participants who had not graduated from high school were coded 1 ("Low Cognitive Skills"), and participants who had graduated from high school were coded 0 ("High Cognitive Skills").

Cognitive effort. Two questions gauged the amount of effort that participants put into the candidate-evaluation task: "How carefully did you think when deciding which candidate to vote for?" (extremely carefully, very carefully, moderately carefully, slightly carefully, and not carefully at all), and "How hard did you work to make sure you voted for the best candidate?" (extremely hard, very hard, moderately hard, slightly hard, and not hard at all). Responses to both questions were coded 1, .75, .5, .25, and 0, respectively, and answers to the two questions were averaged to yield an index of cognitive effort, which was dichotomized at the midpoint and coded 0 ("High Cognitive Effort") and 1 ("Low Cognitive Effort").

Handedness. Participants were asked, "Are you left or right handed?" Left-handed participants were coded 1, and all others were coded 0.

Demographic variables. Each participant reported his or her year of birth, race, Hispanic ethnicity, gender, and household income at the end of the survey (question wordings and codings appear in the appendix).

Results

Alan Mitchell received 90.6% (N = 247) of the votes when he was listed first and only 75.3% of the votes when he was listed second (N = 312; see row 1 of Table 2). This 15.3 percentage point difference is a statistically significant primacy effect ($\chi^2(1) = 21.48$, $p = .00$).[8]

As expected, the first position advantage was stronger among participants who were given less information about the candidates ($b = .19$, $p = .01$; see column 1 of Table 3).[9] Participants who were

[7] Ambivalence, cognitive skills, and cognitive effort were dichotomized to simplify presentation and analysis. When the moderators were treated as continuous variables, the main results remained essentially unchanged.

[8] All observed differences for directional hypotheses were in the expected direction, so we report one-tailed p-values for these tests. All other reported *p*-values are two-tailed.

[9] Although logit and probit models are popular estimation strategies for binary dependent variables such as this, the linear probability model (LPM), estimated by ordinary least squares, is increasingly popular in econometric analyses (e.g., Angrist & Pischke, 2009). Used with the correct standard errors, linear probability models produce unbiased estimates when coefficients from logit or probit are flawed (Angrist & Pischke, 2009; Freedman, 2008).

Table 2. Alan Mitchell's Vote Share Under Different Name Orders

Participant Group	Alan Mitchell Listed First	Alan Mitchell Listed Second	Difference
All Participants	90.6%	75.3%	15.3**
	(247)	(312)	
Low-Information Condition	94.8%	66.5%	28.3**
	(131)	(157)	
High-Information Condition	85.9%	84.2%	1.7
	(116)	(155)	
High-Ambivalence Participants	86.0%	62.1%	23.9**
	(123)	(133)	
Low-Ambivalence Participants	95.1%	85.1%	10.0*
	(124)	(179)	
Low Cognitive Skills Participants	94.7%	73.0%	21.7**
	(97)	(138)	
High Cognitive Skills Participants	88.3%	78.3%	10.0*
	(144)	(162)	
Low Cognitive Effort Participants	92.4%	63.8%	28.6**
	(58)	(61)	
High Cognitive Effort Participants	90.1%	78.1%	12.0**
	(189)	(251)	
Horizontal Name Array Condition	93.4%	70.7%	22.7**
	(128)	(141)	
Vertical Name Array Condition	87.7%	79.1%	8.6
	(119)	(171)	
Horizontal Name Array-Left Handed	100.0%	63.8%	36.2
	(13)	(18)	
Horizontal Name Array-Right Handed	92.3%	71.3%	21.0**
	(110)	(117)	
Vertical Name Array-Left Handed	100.0%	92.3%	7.7
	(12)	(20)	
Vertical Name Array-Right Handed	86.9%	78.9%	8.0
	(103)	(146)	

Note. *N*s for each cell appear under percentages. *p*-values were obtained from the *F* statistic that corrects the Pearson χ^2 statistic for the survey design.
$*p < .05$, $**p < .01$

shown the candidates' opinions on only four issues manifested a strong and significant primacy effect of 28.3 percentage points ($\chi^2(1) = 34.82$, $p = .00$; see row 2 of Table 2). In contrast, participants who were shown the candidates' opinions on eight issues manifested a small and nonsignificant difference of 1.7 percentage points ($\chi^2(1) = 0.15$, $p = .75$; see row 3 of Table 2).

Also as expected, the primacy effect was much larger (23.9 percentage points) among participants who were highly ambivalent about their candidate choice ($\chi^2(1) = 19.53$, $p = .00$; see row 4 of Table 2) than among low-ambivalence participants (10.0 percentage points, $\chi^2(1) = 7.54$, $p = .04$; see row 5 of Table 2). The difference between these groups was on the cusp of marginal significance ($b = .12$, $p = .10$; see column 1 of Table 3).

Consistent with expectations, the primacy effect was more pronounced among participants with less education (21.7 percentage points, $\chi^2(1) = 8.78$, $p = .00$; see row 6 of Table 2) than among participants with more education (10.0 percentage points, $\chi^2(1) = 7.62$, $p = .05$; see row 7 of Table 2). The difference between these two groups was significant ($b = .14$, $p = .04$; see column 1 of Table 3).

As expected, among participants who devoted little cognitive effort to the task of evaluating the candidates, the primacy effect was a highly significant 28.6 percentage points ($\chi^2(1) = 15.50$, $p = .00$;

Table 3. Predicting the Probability of Voting for Alan Mitchell

Predictor	Model 1	Model 2	Model 3
Demographic Controls			
Age	.00	.00	.00
	(.00)	(.00)	(.00)
Black	−.01	−.02	−.02
	(.07)	(.07)	(.07)
Hispanic	−.01	−.00	.01
	(.11)	(.11)	(.11)
Female	.00	−.01	−.01
	(.07)	(.07)	(.07)
Lefty	–	.08	.22*
		(.10)	(.09)
Main Effects of Moderators			
Low-Information Condition	−.13†	−.13†	−.12†
	(.07)	(.07)	(.07)
High Ambivalence	−.20**	−.21**	−.21**
	(.08)	(.08)	(.08)
Low Cognitive Skill	−.09	−.09	−.10
	(.07)	(.07)	(.07)
Low Cognitive Effort	−.12	−.12	−.11
	(.09)	(.09)	(.08)
Horizontal Array	−.07	−.07	−.03
	(.07)	(.07)	(.07)
Lefty × Horizontal Array	–	–	−.34*
			(.16)
Name Order Main Effect			
Name Order	−.09	−.10	−.08
	(.16)	(.16)	(.16)
Demographic Interactions			
Name Order × Age	−.02	−.02	−.02
	(.02)	(.02)	(.02)
Name Order × Hispanic	.08	.08	.07
	(.12)	(.12)	(.12)
Name Order × Black	.07	.08	.09
	(.09)	(.09)	(.09)
Name Order × Female	.01	.02	.02
	(.08)	(.08)	(.08)
Moderator Interactions			
Name Order × Low Information	.19**	.19*	.19*
	(.08)	(.08)	(.08)
Name Order × High Ambivalence	.12†	.13†	.13†
	(.09)	(.09)	(.09)
Name Order × Low Cognitive Skill	.14*	.15*	.16*
	(.08)	(.08)	(.08)
Name Order × Low Cognitive Effort	.14†	.15†	.14†
	(.10)	(.10)	(.10)
Name Order × Horizontal Array	.13†	.13†	.10
	(.08)	(.08)	(.09)
Name Order × Lefty	–	.03	−.08
		(.10)	(.10)
Name Order × Lefty × Horizontal Array	–	–	.28†
			(.17)
Constant	.90**	.89**	.87**
	(.15)	(.15)	(.15)
N	547	546	546

Note. Cell entries are unstandardized regression coefficients with robust standard errors underneath in parentheses. $*p < .05$, $**p < .01$, $†p < .10$

see row 8 of Table 2). Among participants who devoted considerable cognitive effort, a weaker primacy effect of 12.0 percentage points appeared ($\chi^2(1) = 10.84$, $p = .01$; see row 9 of Table 2). The moderating effect of cognitive effort was marginally significant ($b = .14$, $p = .08$; see column 1 of Table 3).

The primacy effect was sizable and statistically significant among participants who saw the candidate names arrayed horizontally (22.7 percentage points, $\chi^2(1) = 23.47$, $p = .00$; see row 10 of Table 2). But among participants who saw the candidates' names arrayed vertically, the first-position advantage of 8.6 percentage points was not significant ($\chi^2(1) = 3.54$, $p = .14$); see row 11 of Table 2). These two effect sizes were marginally significantly different from one another ($b = .13$, $p = .06$; see column 1 of Table 3).

When handedness and its interaction with name array were added to the regression (see columns 2 and 3 of Table 3), the baseline effect of name order, which describes the order effect among participants (1) who had much information about the candidates, (2) were not ambivalent about their choice, (3) were more educated, (4) expended much cognitive effort, (5) saw a vertical array of names, and (6) were right-handed was not distinguishable from zero ($b = -.10$, $p = .55$; see column 2 of Table 3). And as expected, the primacy effect was significantly larger as information volume decreased ($b = .19$, $p = .01$; see column 2 of Table 3), was marginally significantly larger among people higher in ambivalence ($b = .13$, $p = .08$; see column 2 of Table 3), was significantly larger among more educated people ($b = .15$, $p = .03$; see column 2 of Table 3), and was marginally significantly larger among people who expended less cognitive effort ($b = .15$, $p = .08$; see column 2 of Table 3). The size of the name-order effect was marginally significantly greater among people who saw a horizontal array of candidate names than among people who saw a vertical array ($b = .13$, $p = .06$; see column 2 of Table 3). Right-handed people and left-handed people did not differ significantly in terms of the order effect under conditions of vertical presentation ($b = .03$, $p = .75$; see column 2 of Table 3).

Consistent with expectations, a marginally significant three-way interaction of name order by left-handedness by array appeared ($b = .28$, $p = .05$, see column 3 of Table 3). When the candidates' names were arrayed horizontally, the name-order effect was marginally significantly greater among left-handers than among right-handers ($b = .21$, $p = .08$; see column 2 of Table 4). But when candidates' names were arrayed vertically, the name-order effect was not significant among left-handers (7.7 percentage points; $\chi^2 = 1.12$, $p = .30$, N = 32; see row 14 of Table 2) or among right-handers (8.0 percentage points; $\chi^2 = 2.64$, $p = .21$, N = 249; see row 15 of Table 2) and did not differ between these two groups ($b = -.05$, $p = .62$, see column 3 of Table 4).

Discussion

Summary of Findings

In this study, candidates gained 15 percentage points by virtue of being listed first. This finding resonates with past studies that also found primacy effects in real elections, though the effect size here is much larger than has typically been observed previously (e.g., Koppell & Steen, 2004; Krosnick et al., 2004; Miller & Krosnick, 1998).

The effects of the moderators examined here seem quite substantial and theoretically informative. Under the conditions where the name-order effect was expected to be strongest—among people with little information, high ambivalence, lower cognitive skills, and less cognitive effort—the primacy effect was huge ($b = .63$, $p = .00$). And under the conditions where the effect was expected to be smallest—among people with a lot of information, no ambivalence, high cognitive skills, and higher cognitive effort—the name-order effect disappeared completely ($b = -.09$, $p = .57$; see column 1 of Table 3). Thus, even this limited set of moderators seems to have identified conditions under which the effect thrived versus vanished.

Table 4. Predictors of Voting for Alan Mitchell Separating by Horizontal and Vertical Name Array Conditions

Predictor	Full Sample	Horizontal Only	Vertical Only
Demographic Controls			
Age	.00	.00	.00
	(.00)	(.00)	(.00)
Black	−.02	−.16	.10
	(.07)	(.10)	(.09)
Hispanic	−.00	−.19	.25*
	(.11)	(.16)	(.09)
Female	−.10	−.10	.08
	(.07)	(.09)	(.09)
Lefty	.08	−.13	.20*
	(.10)	(.15)	(.09)
Main Effects of Moderators			
Low-Information Condition	−.13†	−.12	−.11
	(.07)	(.09)	(.09)
High Ambivalence	−.21**	−.24*	−.14
	(.08)	(.10)	(.10)
Low Cognitive Skills	−.09	−.20†	−.06
	(.07)	(.10)	(.09)
Low Cognitive Effort	−.12	.02	−.24*
	(.09)	(.12)	(.11)
Horizontal Array	−.07	–	–
	(.07)		
Name-Order Main Effect			
Name Order	−.10	−.15	.06
	(.16)	(.17)	(.22)
Demographic Interactions			
Name Order × Age	−.02	−.01	−.03
	(.02)	(.03)	(.03)
Name Order × Hispanic	.08	.26	−.17
	(.12)	(.16)	(.11)
Name Order × Black	.08	.16	.09
	(.09)	(.12)	(.11)
Name Order × Female	.02	.10	−.07
	(.08)	(.10)	(.12)
Moderator Interactions			
Name Order × Low Information	.19*	.20*	.16†
	(.08)	(.10)	(.11)
Name Order × High Ambivalence	.13†	.17†	.03
	(.09)	(.12)	(.13)
Name Order × Low Cognitive Skills	.15*	.25*	.11
	(.08)	(.12)	(.11)
Name Order × Low Cognitive Effort	.15†	−.03	.31**
	(.10)	(.15)	(.13)
Name Order × Horizontal Array	.13	–	–
	(.08)		
Name Order × Lefty	.03	.21†	−.05
	(.10)	(.15)	(.10)
Constant	.89**	.95**	.75**
	(.15)	(.14)	(.22)
N	546	266	280

Note. Cell entries are unstandardized regression coefficients with robust standard errors underneath in parentheses.
$*p < .05$, $**p < .01$, $†p < .10$

The present evidence of moderation by information volume resonates with past studies that found weaker name-order effects in races that received more news media attention (Miller & Krosnick, 1998) and strengthens the case for this moderator by documenting causal influence of information volume. This study is the first to provide direct evidence of the role of ambivalence in moderating name-order effects and thereby offer support for the claim that name-order effects sometimes occur because a voter feels torn between the candidates.

This study was also the first to document the individual-level influences of cognitive skills and cognitive effort in moderating the primacy effect in voting. By showing that greater cognitive skills and greater cognitive effort were associated with reduced name-order effects, the present study suggests that such effects may depend not only on how much voters know about the candidates but also on their ability and willingness to make use of the available information.

The name-order effect was large when names were displayed horizontally but not when names were arrayed vertically. Under horizontal-presentation conditions, left-handers showed a greater tendency toward a primacy effect than did right-handers. Presumably, left-handers favored the candidate who appeared on the left in part because his name appeared on their dominant side (i.e., their "good" side of space), consistent with previous tests of the body-specificity hypothesis (Casasanto, 2009).

The effect of handedness is informative about the cognitive underpinnings of the name-order effect. The present data cannot be fully explained by the sequential order in which participants read the candidates' names. If the primacy effect was driven by sequential order alone, the effect should not have varied with handedness, since both right- and left-handed English speakers read from left to right (see Maass, Pagani, & Berta, 2007, on directional bias by language culture). Therefore, it appears that implicit associations between emotional valence and left-right spatial position (which differ between left-handed and right-handed people) are partly responsible for the name-order effect observed here. That is, two biases appear to have been at work here: an effect of spatial position, which is rooted in handedness (see Casasanto, 2011, for review), and a sequential effect, rooted in the culture-specific convention of reading from left to right. The roles of the spatial arrangement of candidates' names and of voters' handedness as moderators of the name-order effect merit further investigation.

Limitations and Future Research

A few aspects of this study suggest hesitation before broadly generalizing the findings. First, the participants were not a representative sample of people who vote in elections. In real elections, people who have little information about the candidates and are minimally interested may choose not to vote at all. This natural filtering process is not reflected in the present study, since all participants were led into the virtual voting booth. This may be a reason why the magnitudes of the name-order effects and the strength of the moderators' effects may be considerably lessened in real elections.

Second, the simulated nature of the election raises questions about its resemblance to real elections. In this study, participants read verbal statements by the candidates. Cues such as their party identifications, endorsements, incumbency status, faces, and voices—all factors that may influence election outcomes—were not provided to the participants. Furthermore, unlike in some elections, the candidates' names were displayed in the vote question without party affiliations next to them. This suggests caution in generalizing our findings to all elections, especially the major elections that display the party affiliation of candidates.

However, past studies have found primacy effects in real elections where candidates' party affiliations were listed on the ballot (Ho & Imai, 2008; Miller & Krosnick, 1998), so it seems unlikely that adding party affiliations would have completely eliminated the name-order effects we saw in the present study. The size of the effect might have become smaller than what we see here (see

Miller & Krosnick, 1998), and the effect might manifest conditionally (see Ho & Imai, 2008), but the order effect seems likely to persist. Presenting other information about the candidates, such as photographs of them (see, for example, Banducci, Karp, Thrasher, & Rallings, 2008), might reduce the magnitude of the order effect and the magnitudes of moderation observed here. These factors remain to be studied experimentally in the future.

Lastly, because this study examined a two-candidate election, we cannot reach conclusions about races with more candidates. However, in a study of two-, three-, four-, and five-choice options of wine samples, a primacy effect was found to be similarly large across all of the choice sets (Mantonakis et al., 2009). So the same may be true for the number of candidates in a race.

Implications of the Findings

The policy implications of these results seem clear. In order to prevent giving an advantage to any particular competitor, candidates' names on ballots should be rotated across electoral units so that each name appears in the first position for approximately equal numbers of voters. This may be especially important for elections held in countries with compulsory voting, such as Australia. Perhaps more voters there cast ballots without needed information than would occur in countries where such citizens can choose not to vote. In Australia, all voters see candidate names in a single random order, which gives a consistent advantage to the candidate lucky enough to end up at the top of the list (King & Leigh, 2009). In the United States, some states' (e.g., Ohio, North Dakota, Montana, Idaho, and Kansas) electoral laws require name rotation across precincts; Krosnick et al., 2004). Other states, such as California, rotate name orders for only some races. But the majority of the states do not implement procedures to safeguard against biasing impact of name-order effects. The present findings, coupled with those of other studies, suggest that these states should reconsider their voting methodologies.

By casting light on the psychological processes likely to be responsible for the name-order effect in elections, the present findings bring other remedial strategies to mind. Lack of information, ambivalence, lack of cognitive skills, and lack of cognitive effort all appear to be at work. The present findings regarding information volume and cognitive effort suggest that engaging the public in learning about candidates and equipping people with lots of substantive information may ameliorate name-order effect. It is likely to be difficult if not impossible to eliminate ambivalence, and cognitive skills are likely to be relatively fixed attributes of individuals, so they may not be of practical value when seeking to minimize the impact of name order.

This is the first study to show a stronger primacy effect on voting under horizontal presentation than under vertical presentation, and this finding should be viewed with caution. If this pattern replicates in subsequent studies, the practical implications seem clear. Candidates' names should be arrayed vertically on ballots, not horizontally, to avoid exacerbating the name-order effect and to avoid introducing *different* biases for right- and left-handed voters.

Lastly, the present findings add to the psychological literature on order effects in judgment. Many studies have explored how the order of choices influences people's decisions (e.g., Fazio, Powell, & Williams, 1989), and the cognitive mechanisms underlying order effects appear to vary, depending on the context and nature of the judgment involved. For example, order effects in selections of navigational routes (Christenfeld, 1995) or food products (Coney, 1977; Dean, 1980; Fazio et al., 1989) or knowledge tests (Atwell & Wells, 1937; Clark, 1956) seem to occur for quite different reasons from those underlying name-order effects in elections. By illuminating how name-order effects are moderated by information volume, ambivalence, cognitive skills, cognitive effort, the spatial orientation of the names on the ballot, and the handedness of the voter, the present study adds new findings to the order-effects literature and encourages future study of the mechanisms and moderators at work in order effects generally.

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Appendix

Wordings and Codings of Demographic Questions

Variable	Question Wording	Coding
Age	In what month and year were you born?	2010-Year of birth
Race	Below are five race categories. Please choose one or more races that you consider yourself to be. White; Black or African American; American Indian or Alaskan Native; Asian; Native Hawaiian or other Pacific Islander; Other	African American = 1; Other race = 0
Hispanic	Are you Spanish, Hispanic, or Latino?	Hispanic = 1; Non-Hispanic = 0
Gender	Are you male or female?	Female = 1; Male = 0
Income	Which category below represents the total combined income of all members of your FAMILY during the past 12 months? This includes money from jobs, net income from business, farm or rent, pensions, dividends, interest, social security payments, and any other money income received by members of your FAMILY who are FIFTEEN (15) years of age or older. Less than $5,000; $5,000 to 7,499; $7,500 to 9,999; $10,000 to 12,499; $12,500 to 14,999; $15,000 to 19,999; $20,000 to 24,999; $25,000 to 29,999; $30,000 to 34,999; $35,000 to 39,999; $40,000 to 49,999; $50,000 to 59,999; $60,000 to 74,999; $75,000 to 99,999; $100,000 to 149,999; $150,000 or more	Less than $20,000 = 1; $20,000–$34,999 = 2; $35,000–$49,999 = 3; $50,000–$74,999 = 4; $75,000–$99,999 = 5; More than $100,000 = 6
Education	What is the highest level of school you have completed or the highest degree you have received? 1st, 2nd, 3rd, or 4th grade; 5th or 6th grade; 7th or 8th grade; 9th grade; 10th grade; 11th grade; 12th grade no diploma; High school graduate—Diploma or equivalent; Some college but no degree; Associate degree—Occupational/ vocational; Associate degree—Academic program; Bachelor's degree; Master's degree; Professional school degree; Doctorate degree	Low education = All categories up to "High school graduate—Diploma or equivalent", inclusive; High education = All categories above "High school graduate—Diploma or equivalent"

Memo

17 November 2021

To: Warwick McNaughton, Governance Services

cc: Alison Reid and Dr Jesse Allpress, Research and Evaluation Unit (RIMU)

From: Ting Huang, RIMU

Subject: Analysis of order of candidate names on election outcomes

The Research and Evaluation Unit (RIMU) has been asked to undertake an analysis of the impacts of candidate order on election outcomes (ballot order effects) in the last four Auckland Council elections (2010, 2013, 2016 and 2019). The findings will be used to inform Governance Services on options and recommendations for ordering candidate names on voting documents for the 2022 Auckland Council election.

1.0 Current situation

Auckland Council issues a postal ballot paper with an accompanying candidate information booklet. Voters fill out the ballot paper – selecting their preferred candidate(s) for mayor, ward and local board[1] – and return the form in a pre-addressed envelope.

Currently, local authorities in New Zealand are able to choose from the following methods of ordering names on the ballot paper and candidate booklet:

1. Alphabetical
2. Pseudo-random (the names are randomised and the same random list printed on each voting document)
3. True-random (each voting document has a different random order of names).

To date, Auckland Council has ordered both ballot paper and booklet alphabetically. Governance Services is currently preparing a report on options and recommendations for voting document ordering in 2022.

In early 2016, RIMU undertook an assessment of the academic research on whether candidate order impacts election outcomes. Whilst a large number of international research studies suggest that candidates listed first were more likely to be elected, our analysis of Auckland Council election data from the 2010, 2013 and 2016 elections showed no compelling evidence for this effect.

2.0 Analysis of Auckland Council election data

Replicating the same methodology used in RIMU's previous analyses, the following analyses were conducted for the last four Auckland Council elections (2010, 2013, 2016 and 2019):

- The impact of ballot position on the *number of votes received* by candidates (i.e. the impact on the vote share)
- The impact of ballot position on whether an individual was *elected or not* (i.e. the impact on election outcomes).

An important consideration when interpreting these analyses is that the 'sample size' (number of wards and local boards) for these three Auckland Council elections is relatively small. This means

[1] Along with their selections for District Health Board and Licencing Trust, where applicable.

that there is likely to be a greater amount of 'noise' in the data, than in analyses with larger sample sizes. This increased noise means that it is harder to determine whether patterns in the data are real impacts of candidate order or are due to 'other factors' (including large individual candidate effects and random variation).

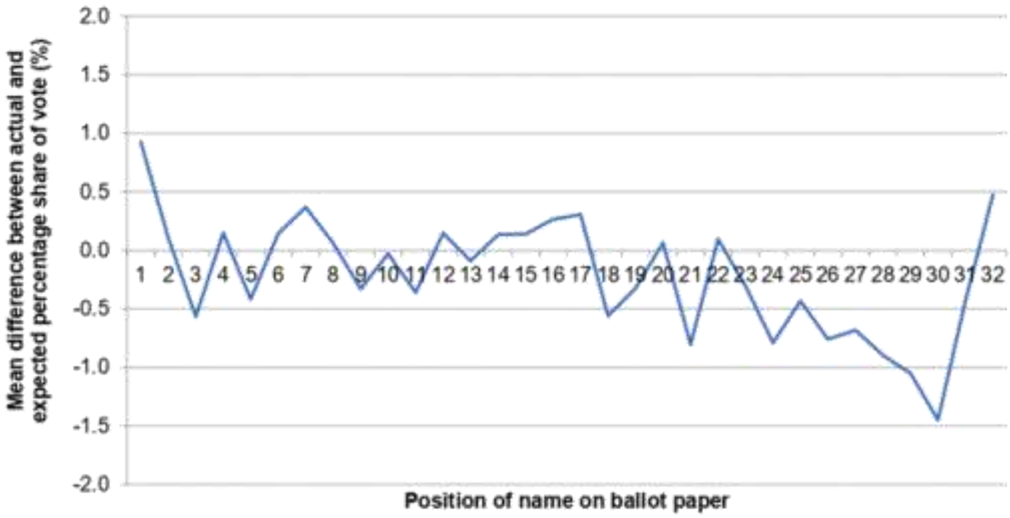
The analyses below include a total of 1811 candidates, of whom 663 were elected. A total of 47 wards and 124 local boards (counting subdivisions separately) were included in the analysis. The mayoral election races from all four elections were not included.

3.0 Impact on vote share

The first analysis looked at whether ballot paper order had an effect on candidates' vote share. The analysis compared the percentage of votes received by candidates in each ballot paper position with the expected percentage share of votes, absent any order effects.[2]

The difference between actual and expected vote shares can be seen in Figures 1-3. The overall analysis in Figure 1 suggests that being listed first appears to increase a candidate's vote share by an average of 1.0 percentage point. Middle ballot positions deviate from the expected vote share[3] in a random manner by approximately ±0.5%. Candidates listed in later ballot positions appear to receive slightly lower vote share than expected. However, given the variability across the whole graph, findings presented here should be interpreted with caution.

Figure 1: Mean difference between actual and expected vote share percentages for candidates in each ballot paper position (wards and local boards combined)



[2] The expected share of votes for each candidate was calculated for each ward and local board separately, by dividing 100% by the number of candidates in the given ward/local board (in effect, assuming an equal distribution of votes within a given ward or local board). This method enabled a comparison of the percentage of votes actually received by candidates in each ballot position with the expected share of votes. The actual vote shares and expected vote shares were then averaged for all candidates in a given ballot position. A difference from expected vote share was then calculated for each position by subtracting the mean actual vote share from the mean expected vote share.

[3] Note, percentage vote share here refers to the absolute increase or decrease in the percentage of votes received. For example, a 1 percentage point effect means that a candidate who would have otherwise received 15% of the vote receives 16%, while a candidate who would otherwise receive 60% of the vote receives 61%.

Wards and local boards were also analysed separately to see if there were any differences in the effects. Figures 2 and 3 show local boards and wards, respectively.

Analysis for the local boards in Figure 2 shows a similar pattern to the overall analysis (refer to Figure 1 above), while the ward analysis does not show any discernible pattern. This was possibly due to the small sample size and/or large candidate-specific effects that outweigh any ordering effects.

Figure 2: Mean difference between actual and expected vote share percentages for candidates in each ballot paper position – local boards

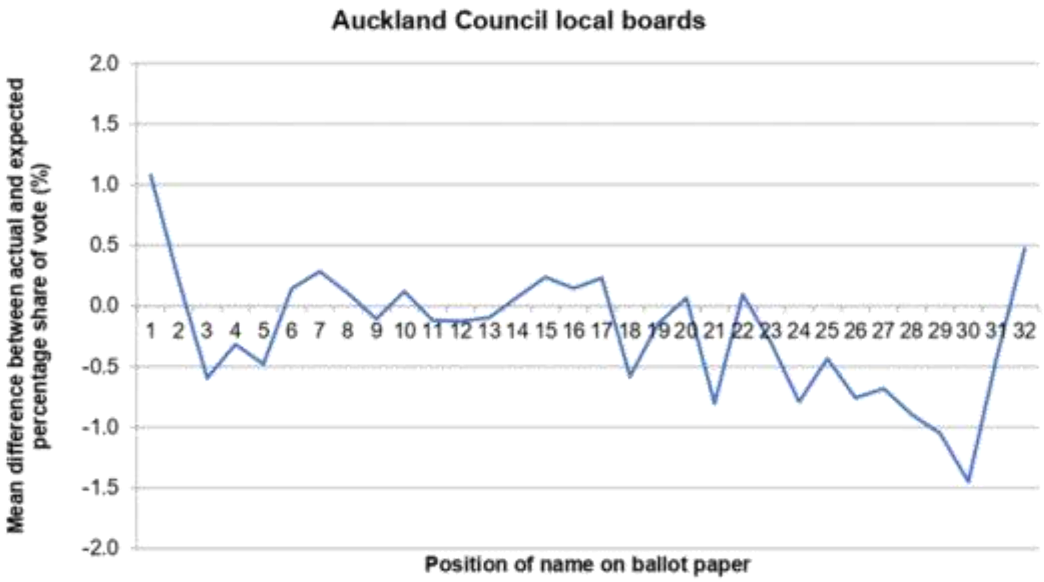


Figure 3: Mean difference between actual and expected vote share percentages for candidates in each ballot paper position – wards

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4.0 Impact on election outcomes

The second analysis assesses whether being listed first on ballot papers provides any advantage to candidates for being elected.

In this analysis the number of first-listed candidates who were actually elected was compared with the number of first-listed candidates that we would expect to have been elected, absent any order effects.[4] Overall, a total of 69 first-listed candidates were actually elected to their respective ward or local board, which was similar to the total of 65 first-listed candidates that we would have expected to have been elected by chance alone.

Statistically, a chi-square goodness of fit test showed that the number of first-listed candidates elected did *not* differ significantly from the expected number, χ^2 (1, N = 663) = 0.21, p =0.60. The pattern remained consistent when wards and local boards were separated.

The third analysis looked at whether candidates with names earlier in the alphabet were more likely to be elected. The candidate pool was split into four approximately equal-sized groups, and the number of candidates elected from within each group was compared to the expected number of elected candidates from that group (Table 1).[5]

Table 1. Actual vs. expected number of candidates elected by letter of last name

	Total number of Candidates	*Actual* number of candidates elected	*Expected* number of candidates elected
A through D	490	190	179
E through L	478	180	175
M through R	420	132	154
S through Z	423	161	155

A chi-square goodness of fit test showed that the actual numbers of candidates elected within each group did *not* differ significantly from the expected number, χ^2 (3, N = 663) = 3.83, p =0.28. A similar non-significant pattern was found when wards and local boards were separated.

In summary, neither analysis above shows any observable effect of candidate order on actual election outcomes – whether candidates were elected or not.

5.0 Summary of findings and recommendations for 2022

The three different analyses of Auckland Council elections data show that while there might be a small impact of being listed first on the percentage share of votes received in local board elections, there is no compelling evidence that candidates being listed first were more likely to be elected in the last four elections. Given the relatively small sample size and variability in the data, these analyses may be less able to detect the real effects. Therefore, conclusions should be drawn with caution. That said, it is reasonable to conclude that results from the last four elections were not significantly affected by the use of alphabetical ordering on voting documents.

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[4] The number of first-listed candidates that we would expect to have been elected was calculated within each ward or local board, by dividing the number of seats by the number of candidates vying for seats. For example, for a local board with 4 seats and 8 candidates, the probability of the first-listed candidate being elected if voting was random is 0.5. When summed across all wards and local boards, these probabilities tell us the total number of first-listed candidates that we would expect to have been elected if seats were assigned randomly and therefore there were no impacts of candidate order.

[5] The number of candidates that we would expect to have been elected from each group was calculated by dividing the number of candidates in that group by the total number of candidates (1811), and multiplying the product by the total number of elected candidates (663). For example, for the 'A through D' group, which had 423 candidates, the expected count was calculated: (423/1811) * 663 = 154.86.

Are Ballot Order Effects Heterogeneous?*

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Objective. Past research on ballot order effects has typically focused on the average benefit a candidate receives if placed at the top of the ballot. This study addresses a gap in the literature by examining the possibility that a simple average may mask systematic differences in how the ballot order effect varies across candidates and voters. *Methods.* Using data from all Australian federal elections between 1984 and 2004, a sample that covers 1,187 separate electoral contests and 7,113 candidate × election observations, this study estimates the effect of ballot order on a candidate's share of the primary vote. To determine whether ballot order effects differ across voters as well as candidates, the study also makes use of electorate-level demographic data from the 1996 and 2001 Australian censuses. *Results.* The results of these estimations indicate that being placed first on the ballot increases a candidate's vote share by about 1 percentage point. As a proportion of their total vote, this effect is much larger for independents and minor parties than for major parties. The ballot order effect appears to be similar for male and female candidates, and does not show strong trends upward or downward over the 20-year period covered by our study. Across electorates, the ballot order effect is higher in places where voters are younger and fluency in English is lower. *Conclusions.* A statistically significant ballot order effect was a consistent feature of Australian federal elections between 1984 and 2004. Moreover, this study challenges the assumption that ballot order effects are homogenous, and finds that the effect of being placed atop the ballot varies across both candidates and voters.

It has long been suspected that the order in which candidates' names are placed on a ballot somehow influences the decision-making process of voters. Theories of ballot position have suggested, variously, that candidates benefit from being placed first on the ballot, due to a "primacy effect," or last on the ballot, due to a "recency effect" (Koppell and Steen, 2004).

These theories are based on the notion that voters are less likely to make rational decisions when presented with a choice of candidates about whom

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they have little information. When voters are ignorant about a series of candidates they seek other cues, such as name familiarity or a candidate's political party, to assist the decision-making process (Miller and Krosnick, 1998). In the absence of any such cues, or where voters are ignorant about or ambivalent toward the candidates presented, it has been shown that the ordering of candidates on the ballot influences a voter's decision (Upton and Brook, 1975; Bakker and Lijphart, 1980; Darcy and McAllister, 1990; Brockington, 2003; Koppell and Steen, 2004; Alvarez, Sinclair, and Hasen, 2006; Shue and Luttmer, 2006).

These studies have not uniformly agreed on the size of the ballot order effect, nor on whether the benefit accrues only to those at the top of the ballot. The type of election may also matter. Ho and Imai (2008) suggest that ballot order effects tend to affect only local, relatively unpublicized elections or those elections where the candidates are nonpartisan or are largely unknown to the voting public.

In the majority of studies, however, the "primacy effect" of first place on the ballot has been shown to deliver the greatest benefit to candidates. Analyzing Ohio state elections, Miller and Krosnick (1998) suggest that first place
increases a candidate's percentage of votes earned by 2.3 percentage points compared with last place on the ballot. Using data from statewide elections in California, Ho and Imai (2008) show that candidates in nonpartisan elections increase their vote share by 3.3 percentage points when listed first on the ballot, while candidates in Democratic or Republican primary races boost their vote by between 2 and 4 percentage points when listed first.

Similarly, the "donkey vote" has been widely documented in Australian elections. Since Australia uses a preferential voting system (also known as the single transferable vote or automatic runoff system), voters number the candidates in order of preference. Election officials begin the count by sorting ballot papers into piles based on first preference votes. The candidate with the smallest number of votes is then eliminated, and each of the ballot papers that gave a first preference vote to that candidate is assigned to the voter's next preferred candidate. This process continues until one candidate has more than 50 percent of the remaining valid votes. Donkey voting occurs where an indifferent elector votes for candidates in the order that they appear on the ballot (casting a first preference vote for the first candidate, a second preference vote for the second candidate, etc.). Mackerras (1968) estimated the "donkey vote" effect to be responsible for 1 to 3 percent of the total formal vote cast in Australia.

What makes the Australian situation particularly interesting is that voting is compulsory. In most other democracies, voters may simply choose to stay home on election day, as an increasing number have chosen to do in recent decades (International IDEA, 1997:77). By contrast, Australia levies a fine on citizens who do not vote. In the 2004 election, the fine was A$20, approximately the average hourly wage (Australian Electoral Commission, 2006). Since around 95 percent of adult citizens vote, the Australian case provides an opportunity to estimate ballot order effects across the full population, not merely across the subpopulation who choose to go to the polls in countries with voluntary voting.[1]

Prior to 1984, the ballot order of candidates in Australian Federal House of Representative elections was determined by alphabetic order. Mackerras (1970) has shown that due to firm belief in the power of the "donkey vote" and a desire to maximize their candidates' chances of being placed high in the ballot order, political parties actively chose candidates with surnames early in the alphabet (see also Orr, 2002). Using data from the 1974, 1977, and 1980 Australian federal elections and the 1974 British general election, Kelley and McAllister (1984) estimated that having a surname in the first third of the alphabet was worth an additional 1.3 percentage points for Australian candidates, but had no effect on candidates in the British election. They conclude that one possible explanation for this difference may be the fact that voting is compulsory in Australia but not in Britain.[2]

Partly as a result of such studies, Australian House of Representatives elections switched from alphabetical to random ballot ordering in 1984.[3] So far as we are aware, this is the first comprehensive study since the advent of randomization to test the effect of ballot order in Australian federal elections. Using Australian data has three advantages. First, randomization creates an ideal natural experiment on the effect of ballot order (as distinct from the alphabetic analysis that has been conducted with respect to other countries). Second, because Australia was one of the first countries to randomize ballot order, we have data from a larger number of elections than any other study of which we are aware (e.g., our sample size is nearly 20 times larger than that of Ho and Imai (2008)). And third, because voting in Australia is compulsory, we are able to observe the effect of ballot order for the typical citizen (as distinct from the typical voluntary voter).

[1]According to data produced by the Australian Electoral Commission, the turnout rate in the 1984–2004 elections ranged from 93.8–95.8 percent (measured as a share of enrolled voters). Judging from years in which Australia conducted both a census and a federal election, there does not appear to be an enrollment gap. The number of enrolled voters *exceeded* the number of adult citizens by 0.8 percent in 1996, and by 3.2 percent in 2001 (though the latter may be explained by the fact that the census was held in August, and the election in November). Among those who voted, the share who cast an informal vote (either accidentally, or because they deliberately spoiled their ballot paper) ranged from 3.0–6.3 percent in these elections.

[2]More recently, Jackman (2005) analyzed the factors affecting the swing toward or away from candidates between the 2001 and 2004 elections, and concluded that a one position movement up the ballot paper was associated with a 0.13 percentage point increase in vote share.

[3]A process of double randomization is used to determine ballot order. The Divisional Returning Officer first allocates a number to each of the candidates listed (to do this, wooden balls consecutively numbered are drawn by lot from a rotating container), before a second number is listed alongside the candidates' names assigning them a place on the ballot (the same wooden balls are returned to the container and then redrawn to assign the second set of numbers) (Australian Electoral Commission, 2004).

Using a very large data set of elections conducted with random ballot ordering, our aim is to estimate the size of the ballot position effect (which we define as being placed at the top of the ballot), and to see how this effect varies across time, across different types of candidates, and according to voter demographics. To preview our results, we estimate that the ballot position effect in Australian federal elections is in the order of 1 percentage point—a figure that appears to be stable over time, and does not seem to differ for male and female candidates. However, as a proportion of the total vote, the ballot order effect is much larger for smaller parties and independents. Comparing across electorates, we find that in areas where the median age is lower, and where fewer people are fluent in English, there is a larger premium to topping the ballot paper.

The remainder of our article is structured as follows. The next section estimates the magnitude of the "basic" ballot order effect, and analyzes whether it has changed over time. The following section tests whether the ballot order effect differs according to characteristics of the candidate or the voter. We conclude with a discussion of the implications of our results for election outcomes.

How Large is the Ballot Order Effect?

This study uses data from the eight federal elections to the Australian House of Representatives between 1984 and 2004 (1984 was the first federal election in which ballot position was randomized). There were 148 House of Representatives seats in the federal elections from 1984–1990 and 1996–1998; 147 seats in 1993; and 150 seats in the 2001 and 2004 elections. Between 2 and 14 candidates contested each seat. In total, our sample covers 1,187 separate contests and 7,113 candidate × election observations. Since 995 candidates in the sample ran for office more than once, there are a total of 5,430 different individuals in the sample. Table 1 presents summary statistics.

TABLE 1

Summary Statistics

Variable	Mean	*SD*	*N*
Vote share	0.1669	0.1934	7,113
Log(vote share)	– 2.7893	1.6129	7,113
First position	0.1669	0.3729	7,113
Female candidate	0.2412	0.4278	7,090
Share of electorate not fluent in English	2.4295	3.1217	7,113
Median age in electorate	34.2382	2.6065	7,113
Median weekly family income in electorate	811.0403	209.0128	7,113

Throughout this article, our dependent variable is a candidate's share of the primary vote. Although we have data on the full preference distribution in each seat, we do not exploit this feature of our data. This is because parties play a major role in affecting preference ordering, through their use of "How to Vote" cards. To reduce the risk that voters will accidentally spoil their ballot, these cards frequently number down the ballot. Therefore, without comparing the parties' recommended preference ordering with the actual results in each seat, we would be unable to distinguish so-called donkey voting from voters merely following their party's recommendation.

We use two variations on the dependent variable: the actual vote share and log(vote share). Note that each assumes a different functional form for the ballot order effect. Using the vote share assumes that being at the top of the ballot has the same percentage point effect. For example, a 1 percentage point effect means that a candidate who would otherwise have received 10 percent of the vote receives 11 percent, while a candidate who would otherwise have received 50 percent of the vote receives 51 percent.

By contrast, using log(vote share) assumes that being at the top of the ballot has the same percentage effect. For example, a 10 percent effect means that a candidate who would otherwise have received 10 percent of the vote receives 11 percent, while a candidate who would otherwise have received 50 percent of the vote receives 55 percent. Theory offers little guidance as to which of these measures should be preferred, so we test both and allow the data to tell us which is most appropriate.

Our main specification is as follows:

$$Voteshare_{ijst} = \beta FirstPosition_{ijst} + C_{jt}^{TotalCandidates} + I_t^{Election} + I_s^{Party} + \varepsilon_{ijst}, \quad (1)$$

where i, j, s, and t index candidates, electorates, parties, and elections, respectively, *FirstPosition* is an indicator that is 1 if the candidate is on the first position of the ballot, and 0 otherwise, $C^{TotalCandidates}$ is a vector of indicators for the number of candidates standing in the electorate (since more candidates will mechanically reduce the vote share), I^{Party} is a vector of party indicators, $I^{Election}$ is a vector of election indicators, and ε is a normally distributed mean-zero error term. All specifications are estimated using ordinary least squares. Standard errors are clustered at the electorate $\times$ election level, to allow for the fact that the total vote share of candidates in the same race must sum to 1.

Since the ballot position is determined randomly, it is unnecessary to include further controls. Just as in a randomized medical trial, where patients are allocated randomly to a treatment group or a control group, so a system of random ballot ordering assigns candidates to the treatment group (in first position on the ballot paper) or the control group (in second position and lower down on the ballot paper).

Ballot order is—by construction—orthogonal to all candidate characteristics. However, as a robustness check, we also estimate specifications with candidate fixed effects. This specification is identified only from within-

TABLE 2

Basic Ballot Order Effects

	1	2
Panel A: Dependent Variable is Vote Share		
First position	0.0097***	0.0047
	[0.0032]	[0.0054]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Candidate FE	No	Yes
Observations	7,113	7,113
R^2	0.80	0.99
Panel B: Dependent Variable is Log(Vote Share)		
First position	0.2813***	0.1748***
	[0.0196]	[0.0434]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Candidate FE	No	Yes
Observations	7,113	7,113
R^2	0.84	0.99

NOTE: Standard errors, clustered at the electorate × election level, in brackets.
***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

candidate variation, using the 995 candidates who ran for office more than once during this period.

$$Voteshare_{ijst} = \beta FirstPosition_{ijst} + C_{jt}^{TotalCandidates} + I_t^{Election} + I_s^{Party} + I_i^{Candidate} + \varepsilon_{ijst} \quad (2)$$

Note that we would not necessarily expect the coefficient on first position to be the same in a specification with candidate fixed effects, due to the potential for attrition from the sample. For example, if candidates are more likely to run again when they are placed first on the ballot, this will lead to an underestimate of the true ballot order effect.

Table 2 presents the results from these specifications. In Panel A, the dependent variable is vote share. On average, we estimate that being at the top of the ballot boosts a candidate's vote share by about 1 percentage point. When we estimate ballot order effects using only within-candidates variation, we find a smaller impact of being in first place: just 0.5 percentage points.

In Panel B of Table 2, we use log(vote share) as the dependent variable. We find that being in the first position on the ballot boosts a candidate's share of the vote by 28 percent. When we estimate ballot order effects using only within-candidates variation, the effect of being at the top of the ballot is to raise a candidate's vote share by 17 percent. The R^2 is higher when the dependent variable is the log of the vote share (Column 1 of Panel B) than when it is the level of the vote share (Column 1 of Panel A), suggesting that, on average, the log specification better fits the data. We return to this issue below.

Do these effects vary over time? If donkey voting arises from a lack of information, then the rising educational levels in Australia over the period 1984–2004 should cause the effect to decline.[4] On the other hand, if donkey voting reflects voter apathy, then one might think that just as turnout levels have fallen in many other democracies, the equivalent under compulsory voting might be a rise in donkey voting over time.

To test this theory, we interact the first position indicator with a dummy variable for each election. With categorical data, there are two ways of estimating a model with interactions. Suppose we wish to look at how *FirstPosition* varies across K elections. One approach is to include the *FirstPosition* variable, plus interactions of *FirstPosition* with $K-1$ of the election indicators.

$$\begin{aligned} Voteshare_{ijst} = {} & \beta_0 FirstPosition_{ijst} + \sum_{t=1}^{K-1} \left(\beta_k FirstPosition_{ijst} \times I_t^{Election}\right) \\ & + C_{jt}^{TotalCandidates} + I_t^{Election} + I_s^{Party} + \varepsilon_{ijst} \end{aligned} \tag{3}$$

Equivalently, we can omit the *FirstPosition* variable, and include interactions of *FirstPosition* with K election indicators.

$$\begin{aligned} Voteshare_{ijst} = {} & \sum_{t=1}^{K} \left(\beta_k FirstPosition_{ijst} \times I_t^{Election}\right) + C_{jt}^{TotalCandidates} \\ & + I_t^{Election} + I_s^{Party} + \varepsilon_{ijst} \end{aligned} \tag{4}$$

In Equation (3), the coefficients on the interaction terms show how ballot order effects differ across elections. In Equation (4), each interaction coefficient shows the magnitude of the ballot order effect in that election. We opt for the latter approach, and also provide a formal test of the hypothesis that the ballot order effect is the same in all elections. Other interaction specifications in this article (except for those in Table 6, where the interacted variable is continuous) follow a similar approach.

The results from this specification are shown in Table 3. Using vote share as the dependent variable, the first position effect is positive in all elections except 2001, and largest in 1990 and 1998. We then test two hypotheses: that the ballot order effect is unchanged over time, and that the ballot order effect is not systematically trending upward or downward. The first is simply an F test that the eight first position coefficients are equal, while the second involves running another regression with (*FirstPosition* × *Year*) used in place of the individual election interactions. With vote share as the

[4]For example, the share of Australian adults with less than a high school education declined from 73 percent in the 1981 census to 54 percent in the 2001 census.

TABLE 3

Do Ballot Order Effects Differ Across Elections?

	1	2
	Dep Var: Vote Share	Dep Var: Log(Vote Share)
First position × 1984	0.0197	0.2374***
	[0.0124]	[0.0597]
First position × 1987	0.0028	0.1321**
	[0.0117]	[0.0632]
First position × 1990	0.0303***	0.3318***
	[0.0097]	[0.0582]
First position × 1993	0.0034	0.3641***
	[0.0083]	[0.0571]
First position × 1996	0.0069	0.2716***
	[0.0073]	[0.0432]
First position × 1998	0.0156**	0.3502***
	[0.0080]	[0.0542]
First position × 2001	−0.0022	0.1813***
	[0.0056]	[0.0474]
First position × 2004	0.0025	0.3638***
	[0.0068]	[0.0563]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Observations	7,113	7,113
R^2	0.80	0.84
F test. H0 is that all *FirstPosition* coefficients are equal	1.65	2.46
	$p = 0.12$	$p = 0.02$
F test. H0 is no linear time trend in *FirstPosition* effect	2.36	2.02
	$p = 0.13$	$p = 0.16$

NOTE: Standard errors, clustered at the electorate × election level, in brackets.
***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Note that the main effect of first position is omitted because the ballot order effect is interacted with all election indicators. The second *F* test is from a separate regression testing the significance of the coefficient on (*FirstPosition* × *Year*).

dependent variable, the *F* tests do not reject the hypothesis that the first position effect is the same in all elections, and is not trending upward or downward ($p = 0.12$ for equality; $p = 0.13$ for a linear trend).

With log(vote share) as the dependent variable, the first position effect is statistically significant in all elections, and largest in 1993 and 2004. In this case, it is possible to reject the hypothesis that the ballot order effect remains constant over time ($p = 0.02$), but while the effect does seem to fluctuate, it is not systematically trending upward or downward. A second *F* test cannot reject the absence of a significant linear trend in the log ballot order effect ($p = 0.16$).

Does the Ballot Order Effect Differ Across Candidates or Voters?

In this section, we test whether the ballot order effect differs across candidates or voters. To compare across candidates, we estimate ballot order effects between parties, and then between male and female candidates. To compare across voters, we test whether a relationship emerges between the demographic characteristics of the electorate and the strength of the ballot order effect.

First, we ask: Is the ballot position effect constant across parties? One factor that might cause differences is the fact that major and minor parties have very different baseline votes. Another is the higher public prominence of major party candidates—which may mean that ballot order is less salient for them than for minor party and independent candidates. Our data allow us to explore differences across parties but, unfortunately, do not allow us to distinguish between these explanations.[5]

To test for differences across parties, we divide parties into three groups: major parties (the Australian Labor Party, Liberal Party, and National Party), minor parties (the Australian Democrats and the Australian Greens), and candidates from smaller parties or running as independents. The mean vote share of the three groups is 39 percent, 6 percent, and 3 percent, respectively. As with election effects, we then interact the first position indicator with an indicator for being from one of these three party groups.

The results are presented in Table 4. Using vote share as the dependent variable, the effect of being first on the ballot is between 0.7 and 1.1 percentage points, and we cannot reject the hypothesis that ballot order effects are identical across major party, minor party, and independent candidates. However, when the dependent variable is log(vote share), the effect of being first on the ballot is indistinguishable from zero for major parties, 27 percent for minor parties, and 61 percent for independents. An F test easily rejects the hypothesis that these effects are the same.

Even interacting the ballot order effect with party groups, the R^2 is higher in the logged specification than the unlogged specification, indicating that the latter is a better fit to the data. To explore this issue further, we separately estimated the regression for each group of parties (full results not shown). For major parties and independents, the R^2 from the logged specification was substantially higher than in the unlogged specification, while for minor parties, the R^2 was similar in most specifications.[6] This suggests that even if one were to focus only on large parties, or only on independents, using logged vote share as the dependent variable would provide a better fit to the data than using the unlogged vote share.

[5]To distinguish between these two theories, one would need some variation in candidates' prominence that was unrelated to their baseline vote.

[6]For major parties, the R^2 was 0.18 (unlogged) and 0.31 (logged). For minor parties, the R^2 was 0.39 (unlogged) and 0.35 (logged). For independents, the R^2 was 0.40 (unlogged) and 0.56 (logged).

TABLE 4

Do Ballot Order Effects Differ Across Parties?

	1	2
	Dep Var: Vote Share	Dep Var: Log(Vote Share)
First position × Major party	0.0109	−0.0026
	[0.0074]	[0.0287]
First position × Minor party	0.0113***	0.2709***
	[0.0022]	[0.0323]
First position × Independent	0.0072***	0.6089***
	[0.0019]	[0.0329]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Observations	7,113	7,113
R^2	0.80	0.84
F test. H0 is that all *FirstPosition* coefficients are equal	1.10	99.34
	$p = 0.33$	$p = 0.000$

NOTE: Standard errors, clustered at the electorate × election level, in brackets.

***,**, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Party group FE denotes fixed effects for the three party groups—major party, minor party, and independent. Note that the main effect of first position is omitted because the ballot order effect is interacted with all party types.

Next, we test whether ballot order effects differ across male and female candidates, by interacting the effect of being in first position with being a female candidate. We identify male and female candidates using their first name, cross-checking with publicly available records for names that are used by both genders. In this manner, we are able to identify the gender of all but 23 candidates (most of whom listed only their initials on the ballot paper).

The effects of first position for male and female candidates are shown in Table 5. Note that our focus here is not on the raw gender gap, but on the relative benefit that male and female candidates receive from heading the ballot. In separate work, we directly analyze the differences between the electoral performance of male and female candidates (King and Leigh, 2007).

Looking at all candidates (Panel A of Table 5), we find no significant gender difference in ballot position, with both male and female candidates obtaining about a 1 percentage point (or 30 percent) increase in their vote from being placed atop the ballot paper. To take account of the fact that female candidates are more likely to represent minor parties, we then split the sample and analyze only major party candidates (Panel B), and only minor party candidates (Panel C). Again, we find no evidence that the ballot order effect differs systematically for men and women, with p values well above 0.1 on the F test for equality of the gender coefficients.

TABLE 5

Do Ballot Order Effects Differ Between Male and Female Candidates?

	1	2
	Dep Var: Vote Share	Dep Var: Log (Vote Share)
Panel A: All Candidates		
First position × Male candidate	0.0093**	0.2672***
	[0.0037]	[0.0225]
First position × Female candidate	0.0109**	0.3358***
	[0.0044]	[0.0370]
Female candidate	− 0.0135***	− 0.0113
	[0.0024]	[0.0191]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Observations	7,090	7,090
R^2	0.80	0.84
F test. H0 is that all *FirstPosition* coefficients are equal	0.09	2.60
	$p = 0.76$	$p = 0.11$
Panel B: Major Party Candidates Only		
First position × Male candidate	0.0093	0.0360
	[0.0081]	[0.0310]
First position × Female candidate	0.0047	0.0509
	[0.0147]	[0.0602]
Female candidate	− 0.0414***	− 0.1433***
	[0.0068]	[0.0280]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Observations	2,629	2,629
R^2	0.19	0.31
F test. H0 is that all *FirstPosition* coefficients are equal	0.08 $p = 0.78$	0.05 $p = 0.82$
Panel C: Minor Party Candidates Only		
First position × Male candidate	0.0121***	0.2353***
	[0.0024]	[0.0392]
First position × Female candidate	0.0105***	0.1957***
	[0.0033]	[0.0541]
Female candidate	0.0029*	0.0358
	[0.0016]	[0.0308]
Total candidates FE	Yes	Yes
Election FE	Yes	Yes
Party FE	Yes	Yes
Observations	1,585	1,585
R^2	0.39	0.35
F test. H0 is that all *FirstPosition* coefficients are equal	0.14	0.35
	$p = 0.70$	$p = 0.55$

NOTE: Standard errors, clustered at the electorate × election level, in brackets.

***,**, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Note that the main effect of first position is omitted because the ballot order effect is interacted with both male and female indicators.

Finally, we attempt to better understand ballot order effects by looking at whether there is any relationship between electorate-level demographics and the ballot order effect. We test three hypotheses about why people might be more likely to vote for the candidate who heads the ballot paper.

- It reflects an inability to distinguish the parties and/or candidates, and is therefore higher in electorates where fewer voters speak English;
- It arises from uncertainty over the lifecycle, and is therefore lower in older electorates; and/or
- It is due to a disenchantment with the political outcomes produced, and is therefore lower in more affluent electorates.

Ideally, we would like to be able to know each voter's demographic characteristics, and whether he or she voted for the first candidate on the ballot. However, because voting is secret, this information is not available. We therefore use demographic information about the electorate as a proxy for the individual voter's characteristics. Doing so increases the potential that we will attribute ballot order effects to the wrong trait, or commit the so-called ecological fallacy. Suppose we were to observe that the ballot order effect is stronger in an electorate where there are more voters with Trait *X*. This might be because: (1) Trait *X* causes voters to prefer the first candidate on the ballot; (2) individuals with Trait *X* are more likely to have unobserved Trait *Y*, which causes voters to prefer the first candidate on the ballot; or (3) people who live in neighborhoods where more residents have Trait *X* tend to have Trait *Z*, which causes people to vote for the first candidate on the ballot. Given the impossibility of obtaining individual-level voting data, it is hard to see how these limitations can be surmounted. Nonetheless, these limitations should be borne in mind when interpreting the results below.

Electorate-level characteristics are taken from the 1996 and 2001 censuses, matched onto electorates by Kopras (1998, 2003). The geographic match is quite precise, since the match is done at the collection-district level. For earlier elections, we match using the 1996 census characteristics (within electorates, demographic characteristics are very stable over time). For electorates abolished prior to 1998, we assign the demographics of the closest electorate existing in 1998.[7] Election fixed effects ensure that the estimation is unaffected by demographic changes over time. For ease of interpretation, all demographic characteristics are normed to a mean of zero and a standard deviation of unity. Since the demographic characteristics for each electorate are held constant over time, standard errors are clustered at the electorate level (rather than the electorate × election level, as in other specifications).

Table 6 shows the results of the demographic interactions. In general, the sign of the coefficients accords with the hypotheses: the first position effect is

[7]Respectively, we use the characteristics of the electorate of Fraser for Namadgi (ACT); Berowra for Dundas (NSW); Hindmarsh for Hawker (SA); Hotham for Henty (Vic); Grayndler for Phillip (NSW); Barton for St. George (NSW); and La Trobe for Streeton (Vic).

TABLE 6

Do Ballot Order Effects Differ Across Demographic Groups?

	1	2	3	4
Panel A: Dependent Variable is Vote Share				
First position	0.0097***	0.0096***	0.0097***	0.0096***
	[0.0032]	[0.0032]	[0.0032]	[0.0032]
First position × Non-English	0.0022			0.0013
	[0.0033]			[0.0032]
First position × Median age		−0.0055**		−0.0052*
		[0.0027]		[0.0027]
First position × Median income			0.0013	0.0009
			[0.0036]	[0.0036]
Main effects of demographics	Yes	Yes	Yes	Yes
Total candidates FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes
Observations	7,113	7,113	7,113	7,113
R^2	0.80	0.80	0.80	0.80
Panel B: Dependent Variable is Log(Vote Share)				
First position	0.2813***	0.2806***	0.2822***	0.2815***
	[0.0201]	[0.0206]	[0.0207]	[0.0200]
First position × Non-English	0.0696***			0.0636***
	[0.0155]			[0.0162]
First position × Median age		−0.0389**		−0.0255
		[0.0178]		[0.0182]
First position × Median income			0.0263	0.0198
			[0.0227]	[0.0218]
Main effects of demographics	Yes	Yes	Yes	Yes
Total candidates FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes
Observations	7,113	7,113	7,113	7,113
R^2	0.84	0.84	0.84	0.84

NOTE: Standard errors, clustered at the electorate level, in brackets.

***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels respectively. *Non-English* is the share of the electorate that are not fluent in English. *Median age* is the median age in the electorate. *Median income* is the median weekly family income. All electorate demographics are normed to a mean of zero and a standard deviation of unity.

larger in electorates with more non-English-speaking voters (significant in the log specification), and smaller in older electorates (significant in both specifications). The income of the electorate is not systematically related to the ballot order effect. When all three interactions are included together, only the age effect is significant in the vote share specification; and only the non-English-speaking voter interaction is significant in the log vote share specification. Both these effects are quantitatively as well as statistically significant. In the vote share specification, a one standard deviation increase in the median age of the electorate (three years) halves the ballot order effect. In the log vote share specification, a one standard deviation increase in the share of the electorate that is not fluent in English (3 percentage points) reduces the ballot order effect by about one-quarter.

Our finding that ballot order effects are larger in electorates where fewer voters are fluent in English accords with the cross-county results of Miller and Krosnick (1998), who have shown that ballot order effects are stronger in counties where voters are less knowledgeable about politics. Intuitively, it makes sense that the lower the information or understanding about a set of candidates and their policies, the greater the chance that cues such as ballot order will influence the decision-making process of voters.

Conclusion

The decision by the Australian parliament to switch in 1984 from an alphabetically ordered ballot to randomized ballot ordering was in recognition of the existence of this ballot order effect and the unfair advantage it afforded candidates whose names appeared high in the alphabet. This switch did not abolish ballot order effects—it merely allocated the ballot order effect randomly among those candidates standing for election.

We estimate the effect of being listed first on the ballot in Australian federal elections to be approximately 1 percentage point for the average candidate. Our estimate is similar to past Australian estimates, including Mackerras (1968) (1 to 3 percentage points) and Kelley and McAllister (1984) (1.3 percentage points). Since we find no evidence that the ballot order effect in Australia has risen or fallen over the period 1984–2004, our findings—together with those of earlier studies—suggest that the ballot effect may well have been stable since the 1960s.

In general, our estimates of ballot order effects are smaller than those that have been observed in the United States by Miller and Krosnick (1998) (2.3 percentage points) and Ho and Imai (2008) (2 to 4 percentage points). The larger ballot order effects in the United States seem to assuage the concern of Kelley and McAllister (1984) that compulsory voting in Australia may lead the ballot order effects to be larger than in Britain or the United States. Related to this is the criticism that suggests compulsory voting merely raises the number of disinterested or ignorant voters going to the polls. Our results indicate that (at least relative to the United States) this has not been the case for compulsory voting in Australia.

The presence of any statistically significant ballot order effects is at odds with a model of perfectly rational voters. Rational voter models suggest that voters should reward candidates who perform well, but should ignore factors such as ballot ordering, which are unrelated to performance. This motivates our analysis of whether ballot order effects differ systematically across candidates or voters.

Across parties, we find that ballot order has a similar effect in the linear specification, but a larger effect on minor parties and independents in the log specification (either because their baseline share of the vote is lower, or because the "cue" of ballot order matters less for prominent major party candidates). Although the logged specification is less intuitive, it does provide a better fit to the data (i.e., a higher R^2), suggesting that ballot order studies should at the very least show models with both logged and unlogged vote share as the dependent variable.

We also observe that the effect of being first on the ballot is higher in younger electorates, and in electorates where fewer people are fluent in English. This is consistent with younger voters and migrant voters being more susceptible to ballot ordering. Such a finding also accords with prior research that has demonstrated that younger voters and overseas-born voters are more likely to switch their vote from one election to the next (Leigh, 2005).

How pivotal have ballot order effects been in recent elections? To analyze this issue, we turn to look at the two-party preferred vote, which is the salient indicator of the winning margin in a preferential voting system. In the 1996, 1998, 2001, and 2004 Australian elections, 6.7 percent of contests (40 out of 595) were decided by a margin that was smaller than our estimated effect of being placed first on the ballot (1 percentage point). If a candidate from one of the major parties had been placed first on the ballot in these elections, our results imply that it would have changed the outcome.

In particular, we are able to identify five contests in recent Australian history where a major party candidate was placed first on the ballot and won by a margin of less than 1 percentage point. We list below our best estimates of the "lucky" beneficiaries of ballot order effects since 1996, with their share of the two-party preferred vote in parentheses.

- Andrea West, the Liberal Party candidate for Bowman in 1996 (50.9 percent).
- Kim Beazley, the Labor Party candidate for Brand in 1996 (50.2 percent).
- Michael Lee, the Labor Party candidate for Dobell in 1996 (50.1 percent).
- Gary Nairn, the Liberal Party candidate for Eden-Monaro in 1998 (50.2 percent).
- Paul Neville, the National Party candidate for Hinkler in 1998 (50.3 percent).

This is a small list, but it does suggest that ballot order has had an effect on the course of Australian political history. Had Kim Beazley not been randomly selected for the first position on the ballot in 1996, it is quite possible that he would not have been in a position to serve as the Leader of the Opposition in 1996–2001 and 2005–2006.

Finally, our results suggest a possible reform to the Australian system for conducting federal elections. Although the present system of random ballot ordering is fair before the ballot draw occurs (since every candidate has the same chance of heading the ballot), it is manifestly unfair once ballot order has been determined (since the candidate who draws first position is more likely to win). A fairer system would be to print multiple versions of each ballot paper, rotating the ballot positions so that every candidate tops the ballot as often as every other candidate. Such a rotating ballot—presently used in some state and territory elections in Australia, as well as in the 2003 California recall election—would more accurately reflect underlying voter preferences than the present Australian federal electoral system.

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