
Original Article

Partisanship, leader evaluations and the vote: Disentangling the new iron triangle in electoral research

Diego Garzia* and Andrea De Angelis

European University Institute, Via dei Roccettini 9, I-50014 San Domenico di Fiesole Italy.

E-mails: Diego.Garzia@eui.eu; Andrea.DeAngelis@eui.eu

*Corresponding author.

Abstract This article provides an empirical assessment of the causal structure underlying the core dependent variable of electoral research (the vote) and two of its most notable predictors (partisanship and leader evaluations). A critical review of traditional models of voting highlights the need to account for the reciprocal relationship between the main predictors as well as for the potential feedback stemming from the dependent variable. In the light of these considerations, a new ‘iron triangle’ of electoral research would seem to take shape, with partisanship, leader evaluations and the vote tight to each other by a strong link of reciprocal causation. Making use of pre-/post-election surveys from Britain and Italy, the empirical analysis provides evidence for a strong effect of past behavior on political attitudes. However, past behavior seems to exert its effect mainly on partisan attitudes, whereas party leader evaluations appear only slightly affected. The results point to the considerably weakened role of partisanship as attitudinal anchor of vote choice. Leader evaluations, on the contrary, emerge as a crucial component in the voting decision.

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Introduction

A pervasive phenomenon in both established and newer democracies, the personalization of politics has been subject of intense scholarly debate under a multitude of perspectives (McAllister, 2007; Blondel and Thiébaud, 2010; Karvonen, 2010; Garzia, 2014). Studies of modern electoral campaigns have emphasized the crucial role played by individual leaders in conveying party messages to the public at large (Swanson and Mancini, 1996; Mughan, 2000). Others have stressed the growing



importance of leaders in the executives as well as within their own parties' structures as a result of the increasing complexity of the decision-making process in contemporary democratic systems (Farrell and Webb, 2000; Poguntke and Webb, 2005). However, when it comes to party leader effects on individual voting behavior, no such consensus has been reached (for a review, see Garzia, 2014). Indeed, a vast majority of empirical works on the topic are virtually unanimous in interpreting party leader evaluations as a sort of residual category within the voting equation (see, most notably, King, 2002; Curtice and Holmberg, 2005; Karvonen, 2010; Holmberg and Oscarsson, 2011). At the heart of this dispute lies the consolidated view of voters' behavior – on which all these works are based – set forth by Campbell *et al* (1960) in their 1960's classic *The American Voter*. In the social-psychological perspective, vote choices are to be interpreted mainly as a function of voters' long-term allegiances, whereas more proximate influences on voting behavior (for example, leaders, issues, performance assessments) are subject to explanation in terms of such temporally and causally prior attachments (Campbell *et al*, 1960, pp. 24–37; Thomassen, 2005, pp. 7–17). In other words, the Michigan model would seem to postulate party leader evaluations as a more or less direct consequence of long(er)-term partisan identifications, with the latter at the core of the whole cognitive process leading to the individual voting choice.

In spite of its enduring (albeit often implicit) acceptance within the electoral research community, such an understanding of voting reveals a number of critical shortcomings, as identified by previous scholarship on the topic. For one thing, a number of studies have engendered growing doubts over the (hypothetically) exogenous status of party identification (see Fiorina, 2002; Garzia, 2013a, b). Michigan scholars themselves were not unaware of the latent potential of candidate and issue assessments. As they observe, '[e]ven strong identifiers are not impervious to such influences' (Campbell *et al*, 1960, p. 119). In one of the earliest empirical assessments of this claim, Page and Jones (1979) demonstrate that party loyalties 'do not function purely as fixed determinants of the vote; those loyalties can themselves be affected by attitudes toward the current candidates. Even short of major realignments, party affiliations are effects as well as causes in the electoral process' (p. 1088). Regrettably, only a few empirical analyses of leader effects on voting have implemented this conclusion in the specification of their statistical models (among the few exceptions, see Archer, 1987; Marks, 1993; Midtbø, 1997; Garzia and Viotti, 2011; Garzia, 2014).

Perhaps more fundamentally, the available literature appears to have overlooked another crucial issue: namely, that of a potential *feedback* from the dependent variable. In fact, the Michigan model conceives individual vote choices as a function of 'the cumulative consequences of temporally ordered sets of factors' (Miller and Shanks, 1996, p. 192). In such framework, it is political attitudes (that is, party identification, short-term evaluations) to drive behavior. However, more recent studies from the field of economic voting have shown that individuals' behavior also can lead to changes in political attitudes (Wlezien *et al*, 1997; Anderson *et al*, 2004;



Evans and Andersen, 2006; Evans and Pickup, 2010; Bellucci and Angelis, 2013). The issue of reverse causation (*from* the vote choice *to* voters' political attitudes) has been seldom recognized in empirical assessments of party and leader effects, but there are strong theoretical grounds to believe that such possibility should be taken into account.

In the light of these considerations, a new 'iron triangle' of electoral research (cf. Achen, 1992) would seem to take shape, with partisanship, leader evaluations and the vote tight to each other by a strong link of reciprocal causation (see Figure 1).

Clearly, the very existence of a bidirectional relationship between the vote and two of its core predictors (as well as among predictor variables themselves) raises serious concerns with respect to endogeneity (van der Eijk, 2002), thus putting in question the alleged position of the various explanatory factors within the inner logic of the funnel. Therefore, the aim of this article is to provide an empirical reassessment of the relationship between the three 'vertices' of this triangle in order to reach a more informed understanding of the psychological dynamics underlying voters' choice in contemporary electoral democracies. The solution advanced here consists in focusing on short-term electoral dynamics, taking advantage of pre–post election comparisons at the individual level. We will also stress the importance of controlling for the ballot cast at the previous elections in order to disentangle the relative importance of individuals' attitudes – and to control for the causal feedback described here.

The empirical analysis is based on national election study data from two established parliamentary democracies in Western Europe: Italy and Britain. The choice of two countries characterized by sharp differences in terms of electoral

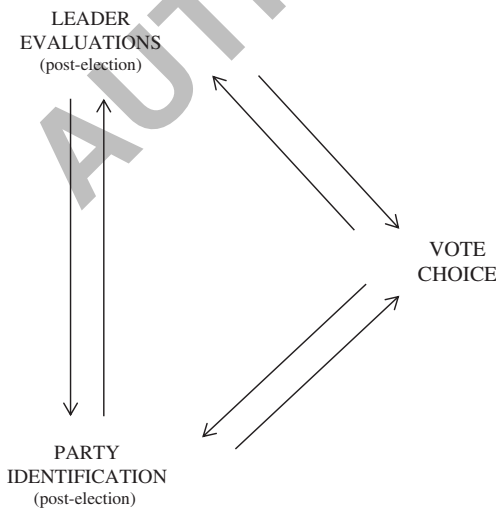


Figure 1: The iron triangle.



system (PR versus FPTP), electoral history of parties themselves (ranging from less than two decades in Italy up to a few centuries in the British case) and a widely different role of party leaders in the political process highlights many crucial variations in the structure of democratic politics, further strengthening the robustness of the empirical findings herewith presented.

This article proceeds as follows: the next section briefly reviews the foundations of classic models of voting, focusing in particular on theoretical and methodological aspects. The following section specifies our empirical model. Next, data and case selection are introduced. The main results of the analysis are presented, extensively checked for their robustness and then discussed in the concluding section along with the main implications for further research.

Theory and Methods

In the classic social–psychological interpretation of voting set forth in *The American Voter* (Campbell *et al*, 1960), electoral choices are conceived as cumulative consequences of a temporally ordered set of factors (for example, voters’ socio-economic status, long-term partisan identifications, and short-term political attitudes in turn). As it stands, however, this theoretical model would appear vulnerable to a potentially fundamental objection from a psychological point of view. A critical aspect of the Michigan model lies indeed in the way in which its authors framed it within mainstream social–psychological theories of the time, and most notably in selective perception theory (Festinger, 1957). The very existence of the path-breaking «funnel of causality» relies heavily on classic attitude–behavior theoretical models, which postulate behavior as driven by individuals’ core attitudes (Fishbein and Ajzen, 1975). Yet, more recent studies show that individuals’ behavior also can lead to changes in attitudes (Eagly and Chaiken, 1993). In the Michigan model, voters are thought to conform their behavior (vote choice) to previous attitudes (party identification) in order to maintain cognitive consistency. Conversely, however, it could be argued that voters’ are actually conforming their political attitudes to past (and hence non-removable) voting behavior right in order to avoid cognitive dissonance. If this hypothesis is correct – as it would appear from a growing number of studies in the field of economic voting (for a review, see: Lewis-Beck *et al*, 2008) – then not taking into account voters’ electoral history within the voting equation is likely to lead to severe bias in terms of theoretical (as well as empirical) underspecification (see, for example, Anderson *et al*, 2004).

Under these conditions it does not surprise political scientists’ growing interest in experimental and counterfactual analytical methods as a way to disentangle this complex set of relationships (Tomz and van Houweling, 2009; van Holsteyn and Andeweg, 2010). However, the alluring perspective of the treatments’ manipulation, substantially improving the internal validity of the inference, comes at the cost of the thorny feasibility issue of dealing with comparative evidence. As a result, the vast



majority of counterfactual and survey experiments are practically bound to provide context-dependent results.

Indeed, virtually all comparative evidence of party leader effects on the vote relies on cross-sectional data sources (post-election surveys). Yet, ‘as long as the data are cross-sectional, any inference about structural effects must remain weak’ (Lewis-Beck *et al.*, 2008, p. 85). When different political attitudes are measured simultaneously to vote recall, their effects are mutually reinforcing and hence not distinguishable – this leading obviously to biased empirical estimates. Another serious problem inherent to cross-sectional analytical strategies lies with their inability to take into account the possible presence of cognitive feedback running from behavior to attitudes. As long as the exogenous status of party identification is taken for granted, then the presence, strength and direction of such feedback cannot be empirically assessed. Unfortunately, scholarship on voting behavior have often failed to take into account properly the reciprocal effect of behavior on attitudes, tending to exaggerate the cross-sectional evidence concerning, for example, the importance of party identification for short-term attitude formation and voting choice (among the few exceptions, see Dinas, 2014). In many cases, the presence of strong and statistically significant cross-section results are retained as evidence to support the powerful role of partisan attachment on individual political behavior, instead of a possible witness for such cognitive feedback at work.

A further point of concern relates to the structure itself of electoral survey research. In post-election surveys, respondents are asked about their vote choice few weeks (and at times few months) *after* the election has taken place. Even assuming that respondents’ vote recall is reported sincerely, such time span may still provide them with a sufficient period of time to ‘shape’ their attitudes in a way that conforms more closely to their past behavior. Indeed, the occurrence that political attitudes are actually being measured after the election provides further ground to believe that, if any, cognitive feedback is actually running *from* behavior *to* attitudes. As a result, concerns about the bidirectional relationship between attitudes and behavior, as well as between attitudes themselves, cast severe doubts on the usefulness of this kind of empirical strategy (cross-section) for assessing the actual contour of party and leader effects in the individual voting calculus.

A different methodological perspective would seem to allow for a direct control of the feedback effect that systematically conflates the estimates of individual attitudes. In electoral research the adoption of simultaneous equation models (SEMs) specification is burgeoning. The basic idea behind this strategy consists in adding a further equation to model explicitly the allegedly endogenous regressor, in order to simultaneously estimate the system and provide exogenous estimates for the variable affected by reverse causality. However, even this seemingly convincing empirical strategy has its drawbacks and can produce flawed evidence. In fact, simultaneous models have been first introduced in economics to model simultaneous choice problems. The classic example dates back to Haavelmo (1943) and considers the



problem of the simultaneous choice of the propensity to consume (made by consumers) and the propensity to invest (made by firms). Another classical example involves the simultaneous choice of labor supply (made by workers) and wage offer (made by firms). In such analytical setting, every equation in the SEM model has an autonomous meaning in isolation from all other equations in the system. This characteristic is often referred to as the ‘autonomy assumption’ (Wooldridge, 2010, p. 239). A problematic aspect of simultaneous models is that whenever each equation in the system cannot be meaningfully interpreted as separated from the other equations, the whole system loses its causal interpretation. As Wooldridge (2010) puts it, ‘causality is closely tied to the autonomy requirement’ (p. 239). Such an assumption is likely to hold in those situations usually treated by economists, and more precisely whenever two different units (for example, workers and firms) are called to make simultaneously the choice of interest. By contrast, the autonomy requirement is unlikely to hold whenever the same unit (for example, voters) is measured to behave (for example, voting) and to subjectively assess an issue of interest (that is, economic situation, party leaders’ personality). In all these cases, the adoption of a SEM framework should not be interpreted as having a causal structure, and infer causality in these cases can be misleading. In order to achieve a deeper understanding of the causal relationships behind the act of voting, it would seem therefore safer not to model individuals’ non-autonomous choices and attitudes by means of a structural model.¹

Model Specification

Against this background, the present study advances a different approach that grounds on two main aspects.

In the first place, we exploit Granger’s (1969) intuition that cause temporally precedes effect – what comes after cannot have produced what comes before. Usually, this would lead to the use of panel data, but this approach is suboptimal for an analysis of the short-term dynamics that shape individual attitudes and drive voting choice. In such context, we cannot consider even the use of panel data as fully satisfying. Willing to be suspicious about the exogenous nature of party identification, it follows that a panel structure might not be able to reveal possible sources of instability occurring in the time span between one election and the next one. On the other hand, measuring individual attitudes at a lower distance in time would allow for this possibility. Therefore we make use of pre–post electoral surveys, which are able to better report the attitude dynamics in proximity to (voting) behavior. This represents a less flawed solution with respect to the cross-section option, since the measurement of political attitudes and behavior in the two time periods is delayed enough to rule out the simultaneity, leading to be somewhat more confident about the fact that what precede is not a consequence of what follows (at least with respect to the cross-sectional design).



In the second place, and in order to deal with the effect proceeding from voting behavior to the definition of individual attitudes, we avoid the SEM specification and opt for the introduction of a statistical control for respondents' voting behavior at the previous elections.² The introduction of this variable is unlikely to control entirely the feedback effect; however, it allows for a significant reduction of this source of endogeneity, further showing (indirectly) on which attitudes the feedback effect is stronger and on which attitudes it is more nuanced. This solution also enables us not to misplace the causal structure of the model, since a simultaneous model of individual voting choice would not have fulfilled the autonomy requirement, thus losing its structural meaning.

Consider the following equations that introduce the 'conventional' way of modeling leader effects (equation (1)) and our alternative specification (equation (2)), respectively:

$$Vote_i = a_1 + a_2PidPost_i + a_3LeaderPost_i + \mathbf{aX} + e_i \quad (1)$$

$$Vote_i = b_1 + b_2PidPre_i + b_3LeaderPre_i + b_4Vote_{t-1,i} + \mathbf{bX} + e_i \quad (2)$$

where: i indexes individual voters; $Vote$ represents the individual vote choice at the election under analysis; Pid represents the individual feeling of attachment toward a political party; $Leader$ is the respondent's thermometer evaluation of the party leader; $Vote_{t-1}$ represents the respondent's vote choice at the previous election; \mathbf{X} is a set of further statistical controls; e_i represents a random error term.

The usual specification (equation (1)) is likely to provide biased evidence of the role played by party identification (coefficient a_2) and leader evaluations (a_3). In fact, this empirical specification is flawed by two different sources of endogeneity. On the one hand, the respective coefficients are conflated due to the fact that these variables are measured after voting behavior (that is, the dependent variable) has taken place. On the other hand, past voting behavior is also likely to play a role: given its high correlations with our key explanatory variables, omitted variable bias do create concerns. Accordingly, in equation (2) these problems are tackled by (i) measuring attitudes in the pre-electoral wave of the survey and (ii) controlling for past voting behavior. As a result, coefficients b_2 and b_3 provide a more exogenous (although not fully exogenous) measure of causal effects because they are not contaminated by the omitted variable bias in b_4 .³ By comparing the magnitude of coefficients b_2 and b_3 we can thus assess the relative strength of party identification and leader evaluations on individual voting behavior.

Figure 2 depicts a graphical representation of this model. Note that the figure features a bidirectional arrow going on between party identification and leader evaluations, a statistical relationship that cannot be directly disentangled through the present specification of the empirical model. However, it must be highlighted that our pre/post specification has already reduced in part the correlation between these two predictors insofar it 'depurated' them from the simultaneous aligning effect exerted

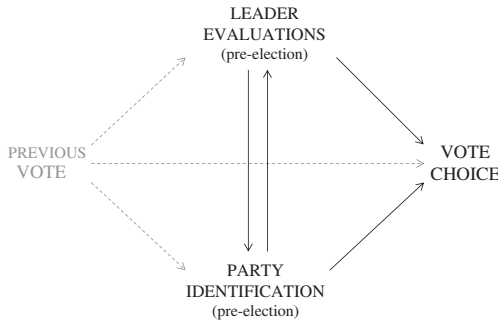


Figure 2: The empirical model.

by voting behavior. More importantly, this specification is especially suited to the purposes of this study for it offers a rather demanding test for the personalization hypothesis. Previous research shows that if the two-way relationship between party identification and leader evaluations is not explicitly addressed in the specification of the empirical model, then ‘the effects of partisanship on the vote are likely to be exaggerated’ (Marks, 1993, p. 143), with leader effects substantially downsized as a result (Dinas, 2008, p. 508). On these bases, our findings are to be interpreted as an intentionally conservative estimate of the electoral effect of leader evaluations at the individual level.

Data and Case Selection

The proposed model will be tested, for exploratory purposes, through a comparison of Italy and the United Kingdom. The choice of these two countries responds to the crucial requirements of the most different system design of comparative research. As a matter of fact, the British and Italian political systems provide a great variability in under a number of crucial respects. At first, Britain’s own first-past-the-post electoral system has resulted in its traditional two-and-a-half party system, whereas the brand-new proportional system in use in Italy since 2005 allows parliamentary representation to all parties above the 2 per cent threshold. Second, the choice of these two countries contrasts a context characterized by an abrupt change in the party system as a result of the mid-1990s breakdown (Italy) with that of a much more enduring party system (UK) (Bellucci, 2006). Furthermore, and although no Western democracies has admittedly been immune from the personalization of politics, the two countries highlight crucial differences in terms of its impact on the electoral competition – still centered around parties to a substantial extent in the United Kingdom (Clarke *et al*, 2004), almost entirely based on party (and coalition) leaders’ personality in Italy



(Garzia and Viotti, 2011). Overall, the British and Italian cases provide a good testing ground for assessing the relative importance of partisanship and leader evaluations within voters' electoral calculus.

The data comes from two national election studies conducted in 2005 (Britain) and 2006 (Italy), respectively.⁴ In the analysis, we will only consider those respondents who accepted to be interviewed twice – during the campaign and again in the weeks following the election day. We employ 'short' panel studies (pre/post) under the expectation that they will favor the stability of partisan ties at the individual level – thus providing a tougher test of the personalization hypothesis. For similar purposes, we have chosen to focus on the Italian election of 2006 for it represents the last one fought by the founding parties of the Second Republic under their original denomination. According to Converse (1969) voters' feelings of attachment to parties develop (and strengthen) hand in hand with the length of affiliation. Thus the choice of this specific election warrants us that the (potential) strength of partisanship within Italian voters' calculus was at its best since the early-1990s breakdown of the First Italian Republic.

The focus of our statistical analysis is on the determinants of vote choice – a nominal variable by definition. Generally, electoral researchers face the problem of the nominal nature of their dependent variable in two ways. A possible manner to deal with the operationalization of the voting choice is to assign a value of '1' if the individual casted its ballot in favor of the incumbent party, and a value of '0' if the voter opted for an opposing party. This approach is fairly common, for instance, in testing economic voting theories, where the performance of the incumbent is usually among the key predictors, or in two-party systems such as the United States. A different solution, particularly suitable in multi-party political contexts, consists in making use of discrete-choice models such as multinomial logit (MNL) or probit (MNP) regression. Yet this second solution can be problematic for at least three orders of reasons. First, as these methods are often employed when dealing with extreme multi-party systems, they can only rarely provide reliable estimates for small parties, whose voting function is extremely skewed (van der Brug and Mughan, 2007). Second, the label 'multinomial' includes a variety of discrete-choice models that presents different peculiarities and drawbacks. In particular, both MNL and MNP modeling techniques share a similar structure with the important difference that the distribution of the error term in the former is assumed to be very simple and tractable (the Type-I Extreme Values) while for the latter is assumed to be normal (Long, 1997). Moreover, the MNL allows only the inclusion of explanatory variables varying across the observations and provides a set of coefficients (one for each alternative) whose identification is heavily dependent on the 'Independence of Irrelevant Alternatives' assumptions, which is unlikely to be satisfied in most political systems.

An alternative analytical perspective consists in measuring party choice on the basis of observed electoral utilities proceeding from political parties and in 'stacking' the data matrix in order to obtain a data structure defined at the level stemming from the interaction of individuals and parties (van der Eijk *et al*, 2006). In our analysis,



we employ such transformation of the data matrix, while retaining the use of actual voting choices rather than electoral utilities as dependent variable (recall that our explanatory model aims at assessing the relationship between political attitudes and voting *behavior*). By stacking our data on a dichotomous dependent variable, we are able to avoid the methodological and theoretical drawbacks of MNL and opt for a less problematic binary logistic model.

Following the logic of the stacked data matrix, the unit of analysis is represented by *respondent*party* combinations. The dependent variable is a dummy that takes the value of '1' if respondents have voted for that party and '0' otherwise. Respondent's electoral choice at the previous national election ($Vote_{t-1}$) is measured exactly in the same way.⁵ All the main predictors are already interpretable in terms of *respondent*party* combinations. Respondents' evaluation of party leaders is tapped by the thermometer score probing their personality assessment on a 10-point scale (question wording in Appendix). Party identification is measured through the usual combination of survey question tapping both the directional and the strength component: respondents are thus assigned a value ranging from '0' (not identified with the party in the specific combination) to '3' (strongly identified with that party). All covariates have been standardized so as to allow for within-data set comparability of the statistical estimates.

One further remark with respect to our control variables concerns the level of analysis (*respondents*parties*). Some of our control variables (for example, issue proximity, measured as the absolute difference between the respondents' placement of the self and each of the parties on the left-right scale) have a direct counterpart at this peculiar level, while others do not. For all the variables belonging to the latter class (age, gender, educational level, social class, church attendance, respondents' assessment of the country's economic situation in the last year) we have computed the so-called *y-hats* – that is, predicted values – regressing our dependent variable on synthetic indexes of the variables of interest though OLS estimation, in order to produce a linear projection (at the *respondent*party* level) of previously individual variables.

Results

The analysis will focus on the Italian and the British case in turn. Table 1 presents the standardized logistic estimates of our statistical model as applied to the Italian data.

Model (1) resembles the conventional way of analyzing leader effects on voting behavior, as from equation (1). It is a fully cross-sectional analysis, with both the dependent variable (vote choice) and the main predictors simultaneously measured in the post-electoral wave of the survey. One observes the conventional result: party identification dominates (albeit only slightly) over leader evaluations. However, this finding cannot be accepted as such because the model from which it stems is unable to control for the likely process of cognitive feedback running from voting behavior to attitudes (that are furthermore measured *after* behavior has taken place).

**Table 1:** Italy (2006)

	(1)	(2)	(3)	(4)
<i>PidPre</i>	—	0.56 (20.10)	0.32 (9.98)	0.28 (8.04)
<i>PidPost</i>	1.09 (29.35)	—	—	—
<i>LeaderPre</i>	—	0.94 (17.83)	0.83 (15.25)	0.62 (9.25)
<i>LeaderPost</i>	1.05 (15.74)	—	—	—
<i>Vote_{t-1}</i>	—	—	0.52 (17.33)	0.49 (14.63)
Controls	SES	SES	SES	SES, ideology, economy
Pseudo R^2	0.56	0.34	0.39	0.45
<i>N</i>	10108	9798	9798	8123

Note: *Vote_t* is the dichotomous dependent variable on a stacked data matrix. Cell entries are standardized logistic regression estimates (z-scores in parentheses). Pseudo *R*-squared is Nagelkerke's coefficient of multiple determination. All coefficients are statistically significant at the 1 per cent level. SES controls include: age, gender, educational level, subjective social class, frequency of church attendance. *Ideology* is the party-voter distance (in absolute value) on the left-right scale. *Economy* is respondents' retrospective socio-tropic assessment of the economy.

In order to control for the feedback of present behavior (that is, vote choice at the election under analysis) we introduce the pre-electoral counterparts of our attitudinal measures (that is, *PidPre* and *LeaderPre*). Estimates are presented in Model (2). Once post-electoral measures are substituted with more appropriate pre-electoral ones, the electoral effect of leader evaluations would seem to overcome that of party identification by a ratio of about 1.7. As it appears, the feedback stemming from the dependent variable is mainly affecting partisan attitudes, whose strength is almost halved. By contrast, the leader variable gets only marginally affected (regression coefficient decreases by a mere 12 per cent across the two models). Notwithstanding, these estimates cannot be still considered fully satisfactory.

As a matter of fact, the specification presented in Model (2) only takes into account the feedback stemming from *present* behavior. However, in the light of our theoretical discussion there are grounds to believe that also *past* behavior might be affecting voters' attitudes toward parties (and leaders). As the act of voting is a behavior that can hardly be changed, it is likely that cognitive dissonance resolution will translate into an attitudinal shift. Therefore, Model (3) introduces a further statistical control that taps voters' choice at the previous national election. The inclusion of this control leads to another twofold reduction in magnitude of party identification's coefficient (from 0.56 to 0.32), whereas leader evaluations' strength decreases again by about 10 per cent (from 0.94 to 0.83). The operational choice to simultaneously rule out the effect of present behavior (through the pre-post structure of the data) and to control for past behavior (through the explicit inclusion of a statistical control) leads us to believe that a considerable portion of the feedback effect has been taken into account. However, one notes that the inclusion of these



controls has affected party identification and leader evaluations in a highly disproportional way.

In order to strengthen the robustness of these empirical findings, we tested them against a fully specified model of voting that includes other relevant component of individuals' electoral decision, namely, ideology (measured as voter-parties proximity on the left-right scale) and retrospective economic assessments.⁶ The results of this specification are presented in the fourth column of Table 1 (Model (4)). Both party identification and leader evaluations highlight a decline in the relative coefficients' magnitude and, admittedly, the decline is slightly more marked in the latter case (34 per cent). Yet our previous observations would seem to hold: if endogeneity is taken into account, then leader evaluations emerge as a much stronger predictor of individual vote choice. As to the electoral effect of party identification, this appears even weaker than past behavior's one (respectively, 0.28 and 0.49), thus lending support to a rather weak anchoring role of this variable – at least with respect to the Italian case.

Evidence from the British case is presented in Table 2, where the very same analytical strategy implemented for the Italian data has been adopted.

Also in the British case, the cross-sectional evidence (as presented in Model (1)) displays the conventionally expected result: party identification dominates the effect of leader evaluations – this time with a ratio of about 1.8. Party identification remains substantially stronger also in Model (2), where the main predictor variables are replaced with their temporally antecedent counterparts. However, once we control for the effect of past behavior we observe a marked drop in the standardized coefficient of party identification (from 1.03 to 0.67) as compared with a reduction of only about 10 per cent in leader evaluation's coefficient (decreasing from 0.64 to 0.58). After controlling for past behavior (Model (3)) the statistical effect of the main covariates on the vote

Table 2: Britain (2005)

	(1)	(2)	(3)	(4)
<i>PidPre</i>	—	1.03 (32.12)	0.67 (18.52)	0.64 (16.38)
<i>PidPost</i>	1.34 (37.26)	—	—	—
<i>LeaderPre</i>	—	0.64 (16.61)	0.58 (14.58)	0.52 (11.55)
<i>LeaderPost</i>	0.72 (16.55)	—	—	—
<i>Vote_{t-1}</i>	—	—	0.72 (22.07)	0.70 (19.74)
Controls	SES	SES	SES	SES, Ideology, Economy
Pseudo R^2	0.53	0.42	0.48	0.49
<i>N</i>	8444	8252	8252	6859

Note: *Vote_t* is the dichotomous dependent variable on a stacked data matrix. Cell entries are standardized logistic regression estimates (z -scores in parentheses). Pseudo R^2 is Nagelkerke's coefficient of multiple determination. All coefficients are statistically significant at the 1 per cent level. SES controls include: age, gender, educational level, subjective social class. *Ideology* is the party-voter distance (in absolute value) on the left-right scale. *Economy* is respondents' retrospective socio-tropic assessment of the economy.



becomes almost comparable. As it appears, the British case confirms that voting behavior bears a strong effect on political attitudes. However, its effect is exerted mainly on voters' partisan attitudes, whose coefficient now almost pairs that of the leader evaluation variable. A more demanding specification of the model featuring also ideology and economic assessments (Model (4)) does not alter these conclusions.

Robustness

The statistical analysis performed in the previous section highlights a relevant – and to some extent unexpected – finding. Once taken into account the feedback effect exerted by voting behavior on political attitudes, the traditional social–psychological anchor of voting behavior (party identification) would seem to play a much weaker role in individuals' voting calculus than often assumed. Party leader evaluations, on the contrary, emerge as a crucial component in the voting decision – a force that parallels that of party identification in Britain and even overcomes it in the Italian case. But are these results sufficiently reliable or do they depend on external factors (for example, model misspecification, political circumstances such as incumbency effects or party specificities, failure to meet logit assumptions)?

In order to check the robustness of our findings, we have put them under extensive statistical testing. Our first exercise focused on the proper measurement of standard errors. As in stacked data matrices the unit of analysis is expressed at the *respondent*party* level, one may be led to argue that standard errors will results artificially reduced, for *intra-individual* observations are by construction correlated. This would violate one of the assumptions of logit models (that is, observations have to be independently distributed) and may provide invalid statistical tests. To rule out this possibility, we re-estimated our models with *clustered* robust standard errors, which explicitly allow for correlation among the error terms. Observations are now assumed to be independent between groups (that is, individuals), but not necessarily within groups (intra-class correlation). The results stemming from this exercise provide virtually identical standard errors with respect to those reported in Tables 1 and 2.⁷

In the second place, we assessed the vulnerability of our results to differences in model specification. Since the influential suggestions of Leamer (1983) it has become frequent among econometricians to explicitly take into account modifications in model specification (for example, by adding or removing regressors) to observe whether the key explanatory variable behave as expected. In such context, either instability or fragility in regressors' coefficients would signal the presence of specification's vulnerability. As it is highly desirable that our coefficients remain stable notwithstanding the empirical specification of the model, we tested the behavior of our key explanatory variables (*PidPre* and *LeaderPre*) when the model was allowed to include *any combination* of our set of further controls



(age, gender, educational level, interest in political matters, subjective social class, frequency of church attendance,⁸ voting behavior at previous election, ideology and retrospective economic assessment). Our statistical tests, as summarized in Table 3, show that the results reported in the previous tables are not vulnerable to changes in the model's specification. The joint consideration of 512 different combinations of regressors highlights that the coefficients of interest vary on a relatively tight range, they never change the expected sign, and – most importantly – they never lose statistical significance.

As a third test, we checked the extent to which our results depend on contingent political circumstances related to the incumbency effect. In order to tackle this possible objection, we replicated our analyses excluding from the data the main ruling party in each country (*Forza Italia* in the Italian case and *Labour* in the British one). The results are presented in Table 4. As it can be easily assessed, the exclusion of incumbent parties from the data provides virtually identical estimates.

Table 3: Robustness checks for the key explanatory variables

	<i>Italy</i>	<i>Britain</i>
<i>PidPre</i>	0.38 [0.24–0.54]	0.81 [0.61–1.04]
<i>LeaderPre</i>	0.77 [0.61–0.96]	0.57 [0.51–0.65]

Note: *Vote*, is the dichotomous dependent variable on a stacked data matrix. Cell entries are the average standardized logistic regression estimates across different model specifications. Squared brackets reports respectively the minimum and maximum coefficient values across the different specifications. The Italian case includes 512 different empirical models, based on all possible combinations of the following list of covariates: age, gender, educational level, interest in political matters, subjective social class, frequency of church attendance, voting behavior at previous election, ideology and retrospective economic assessment. The British case includes 256 combinations (as the frequency of church attendance is not included in the British dataset). Coefficients were statistically significant at the 1 per cent level in every single specification of the model.

Table 4: Incumbent party exclusion

	<i>Italy</i>		<i>Britain</i>	
	<i>All parties</i>	<i>Incumbent party excluded</i>	<i>All parties</i>	<i>Incumbent party excluded</i>
<i>PidPre</i>	0.32 (8.39)	0.35 (8.48)	0.67 (15.96)	0.61 (12.94)
<i>LeaderPre</i>	0.83 (16.90)	0.78 (14.38)	0.58 (14.76)	0.59 (12.39)

Note: *Vote*, is the dichotomous dependent variable on a stacked data matrix. Cell entries are standardized logistic regression estimates (clustered-robust *z*-scores in parentheses). All coefficients are statistically significant at the 1 per cent level. All models have been estimated using the same specification employed in Model (3) (Tables 1 and 2). Controls thus include: age, gender, educational level, subjective social class, frequency of church attendance, voting behavior at previous election.

**Table 5:** One-by-one exclusion of political parties

	<i>Italy</i>	<i>Britain</i>
<i>PidPre</i>	0.28–0.35	0.60–0.76
<i>LeaderPre</i>	0.77–0.93	0.56–0.60

Note: *Vote_{*t*}* is the dichotomous dependent variable on a stacked data matrix. Cell entries report respectively the minimum and maximum coefficient values observed when excluding one-by-one each political party from the dataset. All coefficients are statistically significant at the 1 per cent level. All models have been estimated using the same specification employed in Model (3) (Tables 1 and 2). Controls thus include: age, gender, educational level, subjective social class, frequency of church attendance, voting behavior at previous election.

As a further check we replicated our analyses by excluding one-by-one all other parties, in order to rule out the possibility that a single strong outlier is driving the observed patterns.⁹ Table 5 resumes our findings reporting the range of observed coefficients after this estimation procedure was carried out. In no case the exclusion of a political party is able to subvert our previous considerations, as it is easily assessable looking at minimum and maximum observed coefficients. This further suggests an interesting empirical strategy for comparative party studies, as it is possible to explore between-parties heterogeneities in the average weight of partisan attachment and leaders' evaluation.

Finally, we explicitly took into account the non-linear nature of logit models by inspecting the graphical projection of our key explanatory variables' marginal effects. As widely known, logit coefficients should be interpreted as marginal effects only with respect to the latent dependent variable given by the log of the odds of voting choices. Moreover, its non-linear nature links the explanatory variables in the argument of the distribution function, and hence the marginal effect of one explanatory variable will depend on the values assumed by all the other regressors. Figure 3 presents the marginal effects of changes in our main covariates (from minimum to maximum value) with all other variables set at their mean value, taking into account different specifications of the statistical model (that is, Models (1)–(3) from Tables 1 and 2).

As it appears, marginal effects of leader evaluations on (log odds of) voting choice show a remarkable stability to model changes. Therefore, we can infer that they are much less conflated with the endogenous feedback proceeding from voting behavior than party identification. In both countries (albeit in a more marked way in the Italian case) the strong effect played by party identification is only apparent. Moving from the fully cross-sectional Model (1) (represented with the full line and dark confidence interval) to Model (2) and further to Model (3) the change is brutal, calling for a (very) strong endogenous component in actual partisan measurement which is instead (almost) absent in the individual attitude toward party leaders.

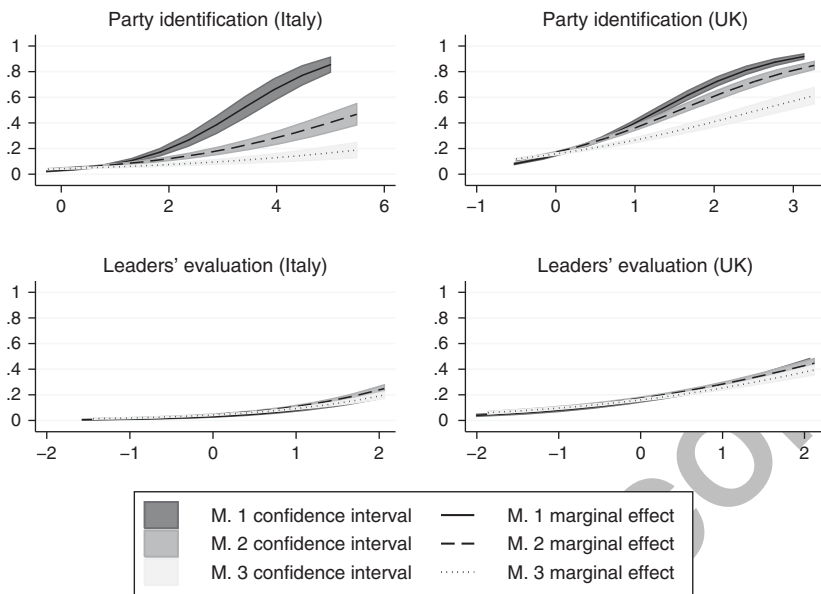


Figure 3: Marginal effects across presented models.

Discussion and Concluding Remarks

The view of an increasing personalization of the political process in contemporary electoral democracies has widely corroborated the idea that individual politicians have gained centrality with respect to party structures as well as in their communication with voters. However, academic research has fallen short of a consensus on whether party leader images have actually gained centrality within voters' electoral calculus. As it appears, the major source of the dispute lies in the wide (and to some extent uninformed) acceptance of the social-psychological interpretation set forth by Campbell *et al* (1960) on behalf of virtually all comparative analyses of leader effects.

In this article we elaborated on this consolidated paradigm, which we specified under a number of respects. In particular, we challenged the classic view of a unidirectional effect of political attitudes on voting behavior. Moving from a number of findings from the field of social and political psychology, we tested the hypothesis that behavior can exert a reverse effect (for example, feedback) on attitudes themselves.

Our empirical strategy took the lead from a critical assessment of the previous literature, with particular regard to its methodological aspects. We have reasons to believe that neither cross-sectional analyses (on which the wide majority of available



evidence is based) nor SEMs are a fully satisfactory means to disentangle the complex set of relationship underlying our ‘iron triangle’. In a similar vein, we find panel data analysis not especially suitable for understanding short-term dynamics of attitude change. For this reason we resorted to short electoral panels featuring the same set of respondents interviewed twice just before and right after the election. In order to make the most of such data source, we modeled individuals’ vote choice as a function of their pre-election attitudes, so as to control for the feedback effect proceeding from *present* behavior. Moreover, our model controls for *past* behavior, with the aim of minimizing this second source of concern.

The empirical findings of our comparative analysis suggest that once the feedback stemming from behavior is fully taken into account, then the estimated effect of attitudes gets substantially downsized. However, not all political attitudes seem to be equally affected by such feedback. The electoral impact of leader evaluations gets only slightly reduced, whereas that of party identification reports an almost twofold diminution as compared with cross-sectional estimates. Leader evaluations, on the contrary, emerge as a strong and significant regressor in the voting equation, whose strength parallels (and, in the Italian case, even overcomes) that of party identification. On the whole, a clear-cut distinction between long- and short-term effects on the vote would seem to be vanishing along with the progressive socio-ideological dealignment going on in established European democracies.

Overall, our results bring party leaders back to the central position assigned them by the ‘common wisdom’ derived from the personalization theory. Most importantly, our findings specify the true nature of partisan attitudes within voters’ political reasoning. The empirical evidence confirms the existence of such an enduring feeling of closeness to a political party as well as its relevance within the voting equation. However, our results highlight the strong part played by voting choice itself as indirect driver of partisanship. As a result, electoral researchers should be cautious in interpreting cross-sectional estimates of partisan effects on the vote, whose strength might be actually conflated by a proper lack of statistical controls for the role played by behavior itself. In this sense, further research on a wider span of time and countries is in urgent need if we are to establish more firmly the actual role of party leader evaluations within voters’ choice in democratic elections.

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About the Authors

Diego Garzia is Jean Monnet Fellow at the European University Institute, San Domenico di Fiesole. His research focuses on the drivers of voters' choices in Western Europe, and in particular on the personalization of politics and its effects on voting behavior at the individual level. On this topic, he recently authored *Personalization of Politics and Electoral Change* (Palgrave Macmillan, 2014). In the last years, he published numerous articles in journals such as *Electoral Studies*, *Party Politics*, *Political Psychology*, *Political Research Quarterly* and *West European Politics*.

Andrea De Angelis is a PhD student in Political Science at the European University Institute, San Domenico di Fiesole. His research is on media, economic information and political behavior in Western democracies. His interests also include Voting Advice Applications, Italian Politics and Quantitative Methods. He received a MSc in 'Economics and Social Science' from Bocconi University, Milan. Part of his work has been published on *Electoral Studies* and other international journals.

Notes

- 1 One of the possible reasons for the generalized expansion of SEM applications in contexts that do not satisfy the autonomy requirement is put forward by Wooldridge (2010), who argues that 'there appears to be a general misperception that "structural" and "simultaneous" are synonymous [...] [while] a simultaneous model is not necessarily structural' (p. 241).
- 2 It must be noted that without the employment of a proper statistical estimator (for example, Anderson and Hsiao, 1981; Arellano and Bond, 1991) the magnitude of the coefficient of previous voting choice is likely to be biased. In this analysis, however, we are not interested in measuring the direct effect of previous vote choice, which is only included as a means for controlling the indirect effect exerted by this variable *through* our key attitudinal measures (that is, party identification and leader evaluations).
- 3 We recognize that these measures are not fully exogenous. However, under the assumption that the proportions of the endogenous parts controlled for in *PidPre* and *LeaderPre* remain constant (that is, they are proportional to the endogenous part that remains hidden), we are allowed to make inference regarding the 'exogenous nature' of our key regressors.
- 4 Harold Clarke, David Sanders, Marianne Stewart and Paul Whitely. *British Election Study 2005*. National Centre for Social Research [P2474 Data file]. Paolo Bellucci and Paolo Segatti. *Italian National Election Study 2006*. Istituto Cattaneo, Bologna [www.itanes.org].
- 5 One problem arising from the use of pre/post election surveys is represented by the necessity to rely on recall data for the past voting choice variable. Admittedly, vote recall questions have been found to be problematic under a number of respects (for a review, see Himmelweit *et al*, 1978). Yet, as Schoen (2011) argues, abandoning recall questions would represent a massive limitation to the study of comparative electoral behavior, given the fact that suitable panel data are 'neither abundant nor without their own problems'. Note that in the election studies at hand, past voting choice is measured in the pre-electoral wave of the survey.
- 6 It is worth noting the highly uneven impact of ideology (0.85) and economic assessments (0.13) as drivers of voting choice in the Italian case. In the British case, the relative effect of ideology and



economic assessments appears more balanced (standardized regression coefficients equal to 0.27 and 0.12, respectively).

- 7 Further than considering clustered standard errors, we enforced a focus on the choices (for example, parties) available to each respondent separately as well as the omission of those respondents who made no choice at all through Conditional Logit estimation. If any, the results of this effort magnify the effect of leader evaluations (Italy = 0.90, Britain = 0.80) *vis-à-vis* party identification (Italy = 0.23, Britain = 0.60). In the case of Italy, we could also compare the findings of our analysis of *synthetic* utilities (that is, vote choice) with those stemming from the analysis of *measured* utilities (that is, propensity to vote scores; for a review, see De Angelis and Garzia, 2013). Once again, the effect of leader evaluations largely dominates that of party identification (standardized OLS coefficients equal to 0.43 and 0.15, respectively). Tables are not shown for parsimony.
- 8 Frequency of church attendance was only available in the Italian dataset.
- 9 As shown by Himmelweit *et al* (1978), vote recall questions may lead to systematic bias favoring large parties (for example voters that cast their ballot for smaller parties might be more prone to attribute their past vote to large parties). Through the one-by-one exclusion of parties from the model, we are able to assess the actual impact of this potential source of bias (if any) on our statistical estimates.

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Appendix

Party identification question

Britain: Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat, or what? If yes, would you call yourself very strong [partyname] fairly strong or not very strong?

Italy: Is there any political party that you feel closer to than others? If yes, would you consider yourself very close to this party, fairly close to this party or only a sympathizer of this party?

Leaders evaluation question

Britain: Now let's think about party leaders for a moment. Using a scale that runs from 0 to 10, where 0 means strongly dislike and 10 means strongly like, how do you feel about [partyleader]?

Italy: I am going to read now a list of some political leaders. For each of them, please tell me if you have ever heard about him/her. If so, please tell me how would you judge him/her giving a mark between 1 and 10: 1 meaning completely negative judgment, 10 completely positive judgment and 6 sufficiently positive.



List of leaders and parties (in parenthesis) included in the analysis

Britain: Tony Blair (Labour), Michael Howard (Conservatives), Charles Kennedy (Liberal Democrats).

Italy: Silvio Berlusconi (Forza Italia), Fausto Bertinotti (Rifondazione Comunista), Umberto Bossi (Lega Nord), Pier Ferdinando Casini (Unione dei Democratici Cristiani di Centro) Piero Fassino (Democratici di Sinistra), Gianfranco Fini (Alleanza Nazionale), Alfonso Pecoraro Scanio (Verdi), Francesco Rutelli (La Margherita).

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