Intuition versus Formal Theory: Tactical Voting in England 1987-1997

Stephen D. Fisher

Intuition tells us that tactical (or strategic) voting is most likely when the voter is indifferent between their first and second preference parties, strongly prefers their second over their third choice, and lives in a marginal constituency where their preferred party is a long way behind the second placed party. But there are two competing rational choice theories. Cox predicts that in any constituency there will either be nearly no tactical voting, or the third and lower placed parties will be almost totally deserted. Myatt criticises the Cox model for failing to account for uncertainty and produces alternative predictions close to the standard intuition, but with important differences. This paper tests these three theories using data on English voters at the 1987, 1992 and 1997 British general elections. The Myatt theory fits the data while the standard intuition and the Cox theory do not. Thus formal theory can improve on intuition in predicting political behaviour.

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Introduction

Duverger's Law - the proposition that the single ballot simple plurality electoral system favours the two party system - is one of the most well know and widely supported propositions of political science (Duverger 1954). As Riker (1982) has described, there is a substantial literature that aims to explain how the law works, concentrating particularly on Duverger's 'psychological effect': the idea that people are less willing to vote for smaller parties in the simple plurality system. This attempt to identify the micro-logic behind the law has accelerated recently with the application of game theory to voting behaviour. In these formal models, voters are assumed to be short-term instrumentally rational, in the sense that they care only about who wins in their constituency in the current election. When voters find it optimal to vote for a candidate other than the one they most prefer they are known as tactical voters (or equivalently strategic or sophisticated voters). A tactical voter is someone who votes for a party they believe is more likely to win than their preferred party, in order to vote effectively.

Although the concept of a tactical vote can be formalised precisely in a rational choice framework, the idea has long been understood on a general level. There are also well established ideas as to why people vote tactically and when people are most likely to vote tactically. Tactical voting is often thought to be motivated by the desire to 'avoid a wasted vote' or to reduce the chances of a disliked candidate. Also, tactical voting is thought to be more likely when the preferred party is 'out of the running' and when there is a close race the leading two parties. Given these common sense intuitions, the question is what can the recent formal theories tell us about tactical voting that is any different.

A formal approach to the voting calculus can be used to show a number of different characterisations of tactical voting are wrong, or at least limited (Fisher 1999). For instance, a vote for a third placed party can be optimal and is no more a 'wasted vote' than any other. Also, it may be that second placed parties also suffer from strategic desertion; something that many commentators do not consider. However, whilst the inspection of the theoretically possible optimal voting strategies is interesting, formal theories are most useful in what they can tell us about what pattern of tactical voting we should expect in practice. In this area there are two notable theories that differ not only from each other but also from the standard intuition. Cox (1994, 1997) predicts that in some districts there will be nearly no tactical voting because voters will not know which of the second and third placed candidates to desert, while in other districts, where the order of parties is clearer, the third and lower parties will be almost completely deserted. Myatt (2000) on the other hand, predicts that the third

placed party always receives some votes and the amount of tactical voting depends on the precision of information available and on the strength of the different parties in the constituency. Both these models contradict the standard intuition. The Cox model does so because the idea of two different equilibria is not part of the standard intuition. The Myatt model does so because it challenges the idea that tactical voting increases with the marginality of the district.

One striking criticism of formal rational choice theory is that it merely formalises intuitive ideas (Green and Shapiro 1994). Here we have an instance where the formal theory generates predictions that differ from the standard intuition. Aside from the specific desire to understand tactical voting further, this is an ideal opportunity to test formal theories against competing intuition to see whether formal theory can teach us anything about political behaviour. To this end, data are drawn from the 1987, 1992 and 1997 British Election Studies (BES) since these surveys collect the necessary information on tactical voting and party preference orderings. Also, England in particular provides a prime example of three party competition under the simple-plurality electoral system. So it is possible to examine each of the three theories in turn using the same data and so provide a fair basis for comparison within an important test case.

The Standard Intuition

There is no solid agreement as to when tactical voting is intuitively most likely, but some consensus is visible in the literature. First, tactical voting is more attractive to voters who are relatively indifferent between their first and second preference parties, but strongly prefer their second choice over their third choice party (Heath *et al.* 1991). Indifference between the first and second preferences facilitates switching from one to the other, while a strong preference for the second over the third choice party can enhance an incentive to vote for the second choice and so reduce the chances of the third choice winning. Whether such an incentive exists depends on voter expectations of the distribution of the vote in the constituency. Typically it is those who support a party that is likely to come third or lower in their constituency who feel the need to make a tactical switch to, 'avoid wasting their vote'. Since people are never sure that their preferred party is in third place, the more a voter feels that their party is out of the running the more willing they should be to switch tactically. Thus tactical voting is expected to increase as the difference in support between the second placed party and the first preference party increases. This statistic is known as the 'distance from contention' and it is well known as a strong predictor of tactical voting. So much so that it became the basis of construct validity tests for different measures of tactical voting (Niemi *et al.* 1992, Evans and Heath 1993, Niemi *et al.* 1993, Franklin *et al.* 1994, Heath and Evans 1994). Finally, tactical voting should intuitively be more likely when the chances of influencing the result are greatest. Thus tactical voting should be greater in more marginal constituencies because the absolute probability of a tie is greater. This is a commonly held proposition (e.g. Cain 1978, Niemi *et al.* 1992, Evans 1994, Fieldhouse *et al.* 1996, Cox 1997) and has even been used to infer tactical voting from aggregate election results (e.g. Curtice and Steed 1988, 1992 and 1997), but the evidence for it has always been very weak. No one has found evidence for a marginality effect that is not specific to supporters of a particular party.

The standard intuition regarding tactical voting can therefore be summarised in four hypotheses. First, tactical voting decreases with the relative strength of preference for the favourite over the second choice party for a voter. Second, tactical voting increases with the relative strength of preference for the second over the third choice party. Third, tactical voting increases with the distance from contention. Fourth, tactical voting increases with the marginality of the constituency. These propositions can be tested individually, but they are best viewed simultaneously. The relative strength of preference for the first over the second choice and that for the second over the third choice are structurally related. Also, distance from contention and marginality are variance dependent and *a priori* negatively correlated. Thus an effect of distance from contention on tactical voting can potentially produce the appearance of a marginality effect and *vice versa*. For these reasons it is very important to analyse the standard intuition propositions simultaneously. Before this can be done there are various methodological issues that must be tackled.

Data and Measurement

As a source of quality survey data, with separate measures of tactical voting, party preferences and vote choice, the British Election Studies (BES) of the 1987, 1992 and 1997 general elections are excellent (Heath *et al.* 1991, 1994, Evans and Norris 1999). This paper is restricted to voters in English constituencies, and where appropriate to supporters of the three major parties (Conservative, Labour and Liberal[1]). This is because three party competition is theoretically and practically simpler to study than systems with more parties. Furthermore, the Myatt model considered below is one of three party competition and should be tested as such. Scotland and Wales were excluded because both have very strong nationalist parties and therefore genuine four party competition. Although there are minor parties in England, there is essentially a three party competition since the same three parties stood at all three elections and took first, second and third places in all but a couple of constituencies (which have been excluded). A further reason why minor party supporters within England are not considered is that, one cannot always tell where they lie in the party preference orderings of the respondents.[2]

If the decision whether or not to vote is considered in the voting calculus, both intuition and formal theory tells us that no one will vote. Since the theories of tactical voting tested here must assume that people do vote, non-voters are excluded from the analysis and to account for an aspect of survey non-response, respondents are weighted to the share of the vote. Much of the analysis concentrates on a still narrower population however. Since the question of whether or not to vote tactically is not relevant to most voters, the most dramatic restriction of the population actually analysed comes with the identification of a risk population for tactical voting. Blais and Nadeau (1996) show that when there is no direct indicator of tactical voting in a survey it is helpful to isolate a, 'pool of potential strategic voters' before attempting to identify tactical voters. However, even when there is a reliable direct measure of

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[1] The Liberal/SDP Alliance that fought the 1987 election were essentially replaced by the Liberal Democrats in the 1992 and 1997 elections. For ease of reference the term Liberal is used to denote both the Liberal/SDP Alliance and the Liberal Democrats.

[2] This is because the strength-of-feeling question (A below) was not asked of all minor parties.

tactical voting it is still necessary to consider what the risk population is for tactical voting. When testing theories of tactical voting one examines how the chances of voting tactically are associated with certain variables. These tests are calculated on a sample from a population about which one wishes to make inference. If that population systematically includes individuals with no chance of voting tactically this would artificially weaken or misconstrue any relationships between tactical voting and the predictor variables of interest. Similarly, if the population studied is too narrowly defined this too may change the nature of the observed relationships. To study tactical voting except with reference to the population at risk of tactical voting makes little sense and may result in false impressions of the processes driving tactical voting.

Suppose for example that one is interested in the effect of the share of the vote for the winning party in the constituency on the incentives for tactical voting. The question one asks of the data is whether the proportion voting tactically increases as the share of the winning party increases. If the population of all voters is used to answer this question the result is likely to be negative. If the winning party's share is greater in constituency 1 than in constituency 2, this is most probably because a greater proportion of voters in constituency 1 have the winning party as their first preference party than in constituency 2. Likewise, constituency 1 will probably have fewer voters supporting the third and lower placed parties. Therefore, fewer voters will be considering a tactical vote in constituency 1. Hence, tactical voting should be lower where the winning party share of the vote is greater. However, the difference in the level of tactical voting is solely due to the difference in the size of the pool of potential tactical voters. If one restricts the analysis to those people who support the third or lower placed parties, this problem does not occur.

Since there is no indicator in the BES cross-section surveys of how voters perceive the chances of their preferred party, it is necessary to use election results to define the risk population. Simply to pick voters whose preferred party came third or lower in their constituency would be inadequate however. To use only the results of the current election would be to assume that voters had very good information about the strength of the parties in their constituency. There is a case for using previous election results or a poll adjusted prediction of the results instead. [3] Since there is no conclusive reason for using any one of these three set of results as a measure of voter expectations, they should all be used. Thus, we define the risk population for Duvergerian tactical voting as all those voters whose preferred party came third or lower in their constituency in the current election, the previous election or the poll adjusted election results.

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[3] The poll adjusted predictions can be made by calculating the difference between the average of the final polls at each election and the actual GB results for the preceding election. This assumes that changes in the share of the vote for each party will be the same in all constituencies. In the absence of constituency level information this is common practice and reflects the type of signals voters receive from the media about the state of public opinion. In 1987 the adjustment applied to each constituency to the 1983 result was Conservative –1.2%, Labour +5.9%, Alliance –4.4% and Other –0.3% (poll figures from Butler and Kavanagh, 1988, p.132). In 1992 the adjustment from the 1987 figures in each constituency was Con –4.9%, Lab + 7.8%, Lib Dem –3.8% and Others +0.9% (poll figures from Butler and Kavanagh, 1992, p.140). In 1997 the adjustment to account for public opinion change from 1992 was Con –12.0%, Labour +11.8%, Liberal Democrat –2.3% and Other +2.5% (poll figures from Butler and Kavanagh, 1997, p.126).

The risk population so defined is too large for two reasons. Firstly, since any of the three results sets can be used to establish third placed party supporter status there is a greater chance of being in the risk population than if only one or all sets of results were required. This feature of the definition is especially relevant in 1997 when there was substantial change in the placing of parties within constituencies between 1992 and 1997. Nonetheless, it is important to retain consistency in the method of analysis across elections. Secondly, as Blais and Nadeau (1996, Endnote 7, p.50) claim, using electoral results rather than direct measures of expectations systematically inflates the population considered at risk of tactical voting because individuals overestimate the winning chances of their preferred party. The 1997 British Election Campaign Study (Norris *et al.* 1999) shows that 68.5 per cent of those in the risk population who expected their preferred party to come second actually found that it came third. This does seem to be evidence that voters inflate the chances of their preferred party. But roughly half of the risk population indicated that they expected their preferred party to come third or lower which, given item non response of 17.5 per cent, shows that expectations of the chances of the preferred party are still pretty accurate.[4]

Despite the problem of an imperfect definition, the concept and use of a risk population is still important. The definition here is a pragmatic one and does not commit us to the claim that those tactical voters outside the risk population cannot be consistent Duvergerian tactical voters. The important point about the risk population is that it enables the analyst to compare the decision to vote tactically among people that could reasonably have voted tactically and only those people. The risk population here includes most of the voters who could reasonably have voted tactically and relatively few for whom it would be unreasonable to vote tactically.

The definition of the risk population assumes that we know the preferred party of each respondent, but there is no item in the BES which asks this directly. Instead, we define the favourite party to be the party voted for except when the vote is tactical or when the voter has a clear (unique) favourite on the strength-of-feeling scores for a party other than that voted for. The strength-of-feeling score for a party is the response coding (1 to 5) from the following question about the party.

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A.    Please choose a phrase from this card to say how you feel about
the (Conservative Party/Labour Party/Liberal Democrats/…)?

1.    Strongly in favour
2.    In favour
3.    Neither in favour nor against
4.    Against
5.    Strongly against
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Tactical voting in British general elections can be measured according to the Heath-Evans technique (Heath *et al.* 1991, Evans 1994) using responses to the following question.

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[4] It should also be noted that although the Liberals came third nationally at each election, they came first or second in roughly half of the constituencies. So the risk population is far from exclusively dominated by Liberals. Excluding minor party supporters the risk population is made of 53.7 per cent Liberal, 41.4 per cent Labour and 4.9 per cent Conservative supporters.

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B.    Which one of the reasons on this card comes closest to the main
reason you voted for the party you chose?

1. I always vote that way
2. I thought it was the best party
3. I really preferred another party but it had no chance of winning
   in this constituency
4. Other (write in)
5. None of these/Don't know
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Tactical voters are primarily identified by response option 3 in question B, but some respondents who gave tactical reasons for their vote in option 4 were also coded as tactical.[5] Those who gave option 3 were asked a follow up question to gauge the preferred party.

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C.    Which was the party you really preferred?
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For those tactical voters who didn't answer or weren't asked question C, the preferred party was imputed from the strength-of-feeling scores. Also some adjustments to the Heath-Evans technique were made to ensure consistency between tactical voting status, vote choice and party preference order according to the strength-of-feeling sores. By definition tactical voters do not vote for their preferred party. If there was any indication that they did so, the respondent was not coded as tactical.[6] As a result of this measurement scheme, tactical voting as a proportion of all English voters was, 5.0 per cent in 1987, 7.7 per cent in 1992 and 8.5 per cent in 1997. However, among major party supporters in the risk population tactical voting was at 12.6 per cent in 1987, 21.4 per cent in 1992 and 24.4 per cent in 1997. So tactical voting is a substantial phenomenon for the population for whom it is a relevant option.

Test of the Standard Intuition

The standard intuition postulates that the probability of tactical voting decreases with the relative strength of preference for the first over the second choice party, and increases with the relative strength of preference for the second over the third preference party. For ease of reference, the relative strength of preference for the first over the second is the first preference gap, while the second preference gap is the relative strength of preference for the second over the third choice party. Measurement of the relative strength of preference with BES data is best done using the strength-of-feeling scores. Thus the preference gap between two parties is the difference in the strength-of-feeling scores for the two parties.

In addition to the relative strength of preference effects, the standard intuition postulates that tactical voting increases with the distance from contention and decreases with the margin of victory. The distance from contention is the difference in the constituency share of the vote between the preferred party and the party placed

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[5] Note that this is a measure of 'Duvergerian' tactical voting, i.e. tactical voting away from a party thought likely to come third or lower, because of the emphasis on the preferred party having 'no chance of winning' (Fisher 1999). Since tactical voting is often assumed to be Duvergerian and since the theories of tactical voting tested here apply mainly to this phenomenon, the restriction is of no real consequence.

[6] See Fisher (1999) for more details.

second in the constituency. The margin of victory is the difference between the share of the vote for the winning and second placed parties in the constituency. Both of these are measured on the percentage share of the vote in the election under consideration, however analyses in which these variables are measured using previous election results or poll estimates of the constituency results produce similar results.

Table 1 below gives the coefficients from logistic regression analyses of the decision to vote tactically that include all four explanatory variables suggested by the standard intuition. [7] The coefficients for the different variables are remarkably similar across the different elections although the p-values do vary. The relationship between tactical voting and the preference gaps is broadly as expected in each election.[8] Also as suggested by the standard intuition, the chances of voting tactically increase as the distance from contention increases. The characteristic proposition of the standard intuition that differs from the new theory is that tactical voting should decrease as the margin of victory increases. However, the relationship observed in the data was the exact opposite in 1987, and in 1992 and 1997 there was no relationship to speak of. In a pooled analysis of the three elections we find similar results – tactical voting actually increases with the margin of victory and the relationship is even significant at the ten per cent level. The remaining three variables in the pooled analysis are statistically highly significant and in the direction expected, but the failure of the marginality hypothesis in the standard intuition is a crucial failing.

Table 1 Logistic regression analyses testing the standard intuition

	1987			1992			1997		
Variable	*B*	*s.e.*	*p*	*B*	*s.e.*	*p*	*B*	*s.e.*	*p*
First preference gap (base=0)			0.00			0.00			0.00
First gap = 1	-0.90	0.31	0.00	-1.18	0.26	0.00	-1.07	0.25	0.00
First gap = 2	-1.81	0.50	0.00	-1.85	0.43	0.00	-2.42	0.67	0.00
First gap = 3 or more	-1.89	1.12	0.09	-7.42	12.28	0.55	-1.92	1.54	0.21
Second preference gap (base=0)			0.05			0.14			0.00
Second gap = 1	0.39	0.52	0.46	0.38	0.42	0.36	1.39	0.48	0.00
Second gap = 2	1.06	0.51	0.04	0.61	0.41	0.14	1.98	0.47	0.00
Third gap=3 or more	1.12	0.55	0.04	1.03	0.47	0.03	2.30	0.48	0.00
Distance from contention	0.07	0.02	0.00	0.09	0.02	0.00	0.05	0.01	0.00
Margin of victory	0.05	0.01	0.00	0.02	0.01	0.15	0.00	0.01	0.76
Constant	-3.75	0.69	0.00	-2.68	0.63	0.00	-2.58	0.58	0.00

Notes: B is the coefficient, s.e. is the standard error and p is the p-value. P-values where no coefficients are show are those from the Wald test of the variable. Sample from the BES includes major party supporters in England in the risk population (see text for further information). In 1987 N=603, in 1992 N=435 and in 1997 N=465.

The surprising inadequacy of standard intuition to fit the pattern of tactical voting provides an impetus to look elsewhere for explanations. In particular, this paper now

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[7] For a description of logistic regression, see for example Agresti (1990).

[8] The magnitude of the estimates and standard errors of the effects when a gap is '3 or more' can be explained by the fact that these are based on low numbers of respondents in these categories. If we make allowances for this then the patterns in the analysis are exactly as expected.

turns to formal theories of tactical voting that differ from the standard intuition in their observable implications.

The Cox Theory

Cox (1994) shows that when voters have independently distributed preferences and voters are commonly aware of the mean level of support for the candidates, then there are two possible types of voting equilibria. The first is a Duvergerian equilibrium in which supporters of the smallest parties all vote tactically for one of the top two candidates. The word 'Duvergerian' is used in the spirit of Duverger's law because bipartism is achieved by supporters of the third and lower placed parties strategically deserting them for one of the top two parties. Although Duverger's law does not imply strict bipartism, the idea is similar and Cox acknowledges that strict bipartism is unlikely to be seen in practice. The second kind of equilibrium in the Cox model is a non-Duvergerian equilibrium in which the second and third placed parties both have equal levels of support. Cox (1997) suggests that this comes about because neither party is deserted by its supporters because no one is sure which party should be deserted strategically. In fact, the non-Duvergerian equilibrium in the Cox model only occurs when there is tactical voting the wrong way (i.e. towards the least preferred party). But this should never happen in the Cox model because the voters can never be mistaken about the order of the parties under sincere voting. Fey (1997) has also shown that Cox's non-Duvergerian equilibrium is unstable. Again, however, Cox realises that his model will not fit reality perfectly, but suggests that some trend towards the two types of equilibria should still be present

Test of the Cox theory

It is the idea of a tendency towards the two equilibria in the aggregate that forms the basis of Cox's test of his own theory (Cox 1997). He claims that constituencies should be clustered in two groups; those in which the top two parties receive nearly all the votes between them and those in which the second and third placed parties have a very similar share of the vote. Cox devised a test of this hypothesis (Cox 1997, pp. 86-89) using the ratio of the share of the vote for the Second loser to the First loser (the SF ratio). If Cox is right, the SF ratio across constituencies should tend to either 0, because the third placed party is strategically deserted, or 1 because it is unclear which party should be deserted.

Before investigating the test further, it is worthwhile noting that there is a problem with the sample selection procedure Cox uses. The test was applied to general election results from British constituencies between 1983 and 1992 in which Labour came third, but there is no good justification for the restriction to seats where Labour came third. The footnote attached to the definition (p. 87) discusses sample selection bias when there is strategic entry to the election. However he goes on to dismiss this, 'since third parties enter in most districts'. In fact the three major parties contested practically every constituency in the elections which Cox considers and so the strategic entry issue is certainly not a reason for restricting the population for analysis.

The conclusion Cox draws from his test does include some acknowledgement of the restriction he has placed on the population he tests.

'Although the evidence just discussed does indicate that there is strategic voting in some British constituencies, the constituencies chosen for inclusion in the analysis were those in which it would have made sense for voters to consider a tactical vote (the strategy of investigation here is similar to that in Blais and Nadeau, 1996). If one looks at the other districts, one finds much less evidence of strategic voting. Just as the survey evidence shows a distinct minority of the electorate voting strategically – many not being in a position that would logically call for a strategic vote – the aggregate evidence shows a distinct minority of districts with substantial levels of strategic voting – many not being in a position that would produce larger levels.' (Cox 1997, pp. 88-89).*

The note that analysis of other constituencies would produce weaker results suggests data dredging. To find a non-random subset of a population that fits a hypothesis is not to find evidence for the hypothesis because sifting through a large data set for long enough frequently produces sets of cases to support many different hypotheses. Cox's claim that the chosen constituencies are, 'those in which it would have made sense for voters to consider a tactical vote', does not provide a justification for the selection. Incentives to vote tactically exist in all constituencies. Although Cox correctly notes that some people are not in a position where it would make sense to vote tactically, he does not actually argue that the risk population is greater in the constituencies he has chosen. Indeed it is impossible to do so without turning to survey data because some measure of party support distinct from the share of the vote is required. One further possibility is that Cox deduces that incentives to vote tactically are stronger in constituencies where Labour came third, because the model fits this set of constituencies. However, this sort of backwards induction is unacceptable in the test of a model. Thus Cox has failed to justify his choice of constituencies for analysis and this failure undermines his conclusion that there is evidence for his theory of tactical voting.

Before replicating Cox's test using all constituencies, I will argue that the test itself is inadequate and that an additional condition is necessary. The Cox test examines the ratio of the share of the vote of the Second loser to that of the First loser. This is the *SF ratio*. If there is a Duvergerian equilibrium the SF ratio is 0 because the third placed party (the second loser) has no votes. In Cox's non-Duvergerian equilibrium the first and second losers have the same share of the vote, so the SF ratio is 1. Cox's hypothesis is that the SF ratio tends to 0 or 1, the Duvergerian and non-Duvergerian equilibria respectively. If this hypothesis is correct then any histogram of the SF ratio calculated on a random sample of constituency results should have two modes: the first at zero and the second at one. For this reason Cox's hypothesis translates to a bimodality hypothesis for the SF ratio.

Cox also suggests that tactical voting will be more frequent in more marginal constituencies. This is not a prediction that he derives from his formal model, but one that he justifies according to the standard intuition: tactical voting is greater when the margin of victory is small because the absolute probability of influencing the result is greater. Using this idea, Cox postulates that the tendency to bimodality in the SF ratio will increase as the margin decreases. Hence, Cox presents histograms to summarise the distribution of the SF ratios for different restrictions on the margin of victory. The results seem to demonstrate exactly what Cox suggested. The SF ratio becomes more

clustered at one and zero when the margin of victory is smaller. Whilst the results seem fairly impressive there is a fundamental question that needs to be addressed: what would the shape of the SF ratio distribution be if there was no tactical voting?

Answering this question is equivalent to specifying the null hypothesis and it is important in order to know what would constitute evidence against the bimodality hypothesis. Cox (1994) argues that there is no apparent reason why the SF ratio should have a bimodal distribution if his theory is not true. Although it is tempting to agree with Cox on this point, it is not clear why the reader should accept the burden of proof that the distribution of the SF ratio under a suitable null hypothesis is not bimodal. The specification of a null hypothesis is difficult, if not impossible, because it effectively requires a prediction of what constituency results would be if there were no tactical voting. Even using available survey data, it is very difficult (if not impossible) to adequately estimate 'sincere' results in any particular constituency. One particular cause for concern is that any prior distribution for the SF ratio must vary with the margin of victory, even assuming sincere voting. Thus, it is unreasonable to compare the histograms Cox displays as if we should *a priori* expect them to be similar. Instead the histograms ought to be different, but it is not clear how different and in what way different.[9]

Without having to specify a distribution for the SF ratio under the null hypothesis, it is reasonable to argue the following. If a bimodal distribution of the SF ratio is the result of tactical voting, then tactical voting should be higher in those constituencies in which the SF ratio is close to zero and lower in those where the SF ratio is near one. This argument suggests that the Cox theory should satisfy two conditions. Firstly, the SF ratio should have a bimodal distribution. Secondly, the proportion of tactical voters in a constituency should decline as the SF ratio increases. This second condition raises the question of whether the distribution of the SF ratio is a function of tactical voting or whether it has arisen by accident or some other cause.

Table 2 below shows the level of tactical voting at the 1987, 1992 and 1997 British general elections among voters in England only according to different levels of the SF ratio. There was nearly no association between tactical voting and the SF ratio in 1987. For 1992 there appears to be a negative association as hypothesised, but the pattern is not monotonic. Moreover, logistic regression of the decision to vote tactically shows that the association is not statistically significant. Finally, in 1997 the association is in the opposite direction to that predicted. So even if the aggregate distribution of the SF ratio is bimodal, the survey evidence suggests that it was not generated by tactical voting. Whether or not the SF ratio has the required distribution remains to be shown.[10]

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[9] Furthermore, one should note that the histograms are all on different scales. Whilst some aspects of the graphs do clearly distinguish the histograms, others are misleading. For example, Cox rightly notes that the shape of the distributions in his Figures 4.1 and 4.2 are clearly different, but he draws too strong a conclusion from his comparison of Figures 4.1 and 4.3 (pp. 87-8).

[10] Tactical votes away from parties that actually came first or second are still included in Table 1. These tactical voters did not affect the SF ratio in the prescribed manner and some may have been

Table 2 Percent tactical by the SF ratio

	1987	(N)	1992	(N)	1997	(N)
0 to 0.25	6.0	(156)	10.3	(240)	4.1	(76)
0.25 to 0.5	4.7	(928)	7.6	(1102)	8.5	(849)
0.5 to 0.75	5.1	(985)	8.5	(466)	8.6	(545)
0.75 to 1	5.0	(585)	3.5	(202)	9.5	(333)
Total	5.0	(2653)	7.7	(2010)	8.5	(1802)

Notes: Source is the British Election Studies 1987, 1992 and 1997. Base is voters in England only. The SF ratio for an individual is the SF ratio in their constituency.

The observed SF ratios in English constituencies for 1992 are presented in a histogram in Figure 1. Those for 1987 and 1997 are very similar but not presented here, since it is only in 1992 that the pattern of tactical voting in the survey data approaches that prescribed by the Cox theory. Also according to the Cox hypothesis this histogram of the SF ratio should be bimodal, but it is clearly single-peaked. So there is no evidence that constituencies tend towards two distinct types of equilibria. The conclusion is, therefore, against Cox's bimodality hypothesis.

Figure 1 Histogram of the SF ratio in English constituencies in 1992

SF Ratio
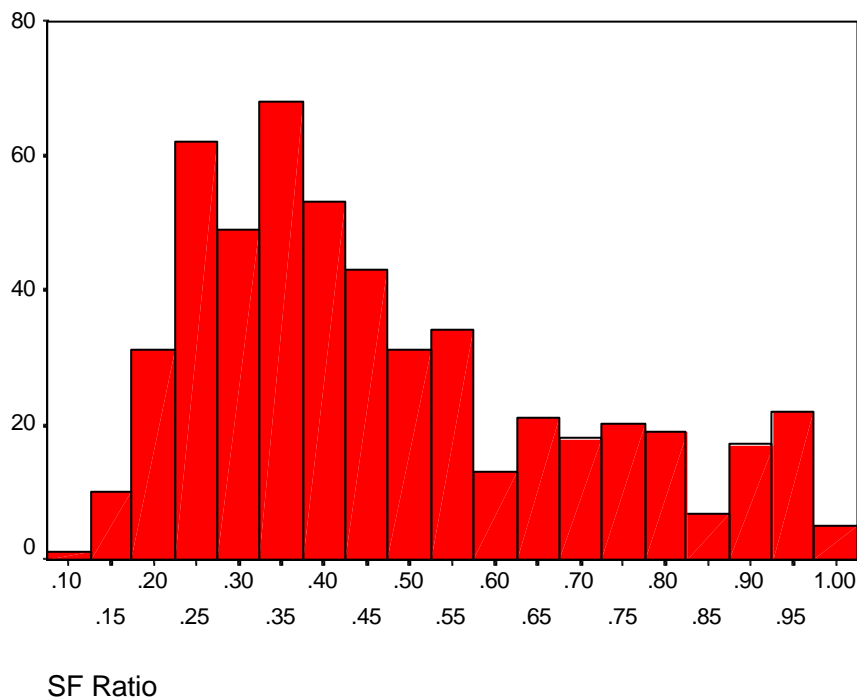
confounding the tendency to a Duvergerian outcome. However, if all the tactical voters outside the risk population are excluded the conclusion is still the same.

The Myatt Theory

The reason why the constituency election results do not fit the pattern Cox describes is given by Myatt (1999). Voters in the Cox model are certain of the average distribution of support for the parties in the constituency. So the probability that any tie for the lead will be between the leading two parties on sincere preferences converges to one as the constituency size increases. Now a voter can only affect the result when they are pivotal, i.e. when the votes of all the other voters result in a tie (or near tie) for the lead. [11] If voters are certain that it will be the top two parties that will be tied for the lead in the event of a tie, then it cannot be optimal to vote for any party other than one of the top two. Thus, all those voters that prefer the third placed party on sincere preferences will strategically desert it and bring about a Duvergerian equilibrium.

In practice voters are uncertain of the distribution of support in the constituency. Cox (1997) argues that this will not affect the fundamental character of the equilibrium outcomes, but Myatt (1999) shows this is not so. If voters are uncertain of the support levels for the parties then voters will not be sure of which pair of parties will be tied for the lead if there is a tie. In particular, there will be some probability that any tie for the lead will involve the third placed party on sincere preferences. So it may be optimal for some third placed party supporters to vote sincerely. Thus the equilibrium outcome when there is uncertainty over the distribution of support in the constituency is non-Duvergerian in the simple sense that all three parties receive some votes (not the special sense of Cox's non-Duvergerian equilibrium with two equal parties). Whilst the Cox model predicts either complete tactical voting in a constituency or no tactical voting at all, the level of tactical voting in the Myatt model varies between these extremes and between constituencies. [12] So what does the level of tactical voting depend on?

Myatt (2000) answers this question in the general case of a three party competition. Without loss of generality, suppose that a voter has utilities u_1, u_2 and u_3, associated with the parties 1, 2 and 3 respectively winning the constituency. Also suppose that $u_1 > u_2 > u_3$. Such a voter will never vote for their least preferred (party 3), but may find it optimal to vote for party 2 rather than their favourite (party 1). If the voter is a utility maximiser then our voter will find it optimal to vote tactically for party 2 whenever:

$$\log\{(u_2-u_3)/(u_1-u_3)\} + \log\{(\Pr[23]+2\Pr[12])/(\Pr[13]+2\Pr[12])\} > 0. \qquad (1)$$

This is essentially the same voting rule as in many formal papers on the subject (e.g. Hoffman 1982, Palfrey 1989, Cox 1994). The first term of this expression depends only on the utilities and it is increasing in the relative strength of preference for the second preference over the third preference (u_2-u_3) and decreasing in the relative strength of preference for the first preference over the third preference (u_1-u_3). Whilst

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[11] A near tie is specifically when two parties are tied but for one vote. The probability of a near tie between two parties is asymptotically the same as that for a tie. For simplicity's sake, the text refers only to ties, since the logic is the same with both ties and near ties.

[12] The Cox model does not generate comparative statics in the way that the Myatt model does. This explains why they are tested in a very different way.

the first of these comparative statics is exactly as predicted by the standard intuition it seems that the second is different because it concerns the first and third preference parties, not the first and second. But since $\log\{(u_2-u_3)/(u_1-u_3)\}= -\log\{(u_1-u_2)/(u_2-u_3) + 1\}$ the comparative statics for the Myatt model and the standard intuition are exactly the same only the Myatt model has given a more explicit functional form. As with the standard intuition, the stereotypical tactical voter in the Myatt model is relatively indifferent between their first and second preference parties, but strongly prefers their second over their third choice.

In the second term of equation (1) $\Pr[xy]$ is the probability of a tie between parties x and y. Myatt (2000) gives an expression for these tie probabilities which is,

$$\Pr[xy] \propto 1/(a_x + a_y - 1) \times b(^1/_2 : a_x, a_y) \times [1 - B(^2/_3 : a_x + a_y - 1, a_z)]. \tag{2}$$

Here $b(:)$ is the beta density and $B(:)$ is its cumulative distribution function. a_x, a_y, and a_z are parameters of the belief distribution of the voter. If we assume that on average voters receive an unbiased signal, then on average $a_x = p_x m + 1$ where p_x is the share of the vote for party x and m is the precision of the information. For those who support the third placed party, the incentive to vote tactically increases with the precision of the information. If one imagines that the only information for each voter is a private poll of voting intention, then m can be thought of as the sample size of that poll. Although this is not the only way people gather information on public opinion in their constituency, thinking of m as a sample size gives us some handle on its meaning.

It is impossible to get a sense of how the Myatt incentive varies with the shares of the vote for the different parties in the constituency. Computing the incentive for members of the risk population shows that it is positively correlated with the distance from contention and negatively correlated with the marginality in the constituency. This appears to be just like the standard intuition, however, there is a strong negative correlation between marginality and distance from contention to consider. A regression of the Myatt tactical incentive on both the distance from contention and the margin of victory shows that after controlling for the positive correlation with distance from contention, there is a positive association between the Myatt tactical incentive and the margin of victory. This completely contradicts the standard intuition.

So how is it that the Myatt model can produce such a counter intuitive result, that incentives for tactical voting can increase with margin of victory? The Myatt model is no different from many other formal models in noticing that the absolute probability of influencing the election result doesn't matter (e.g. Cox 1994, Myerson and Weber 1993, Palfrey 1989 and Hoffman 1982). Since people can only affect the constituency result when they are pivotal, people need to know who they will be pivotal between if they are pivotal. Therefore it is the conditional probability that different pairs of parties are tied for the lead, given that there is a tie, that becomes important. Now when the margin of victory in a constituency widens the absolute probability of a tie for the lead may decrease, but the conditional probability that, if there is a tie, it is between the top two placed parties can actually increase. So, supporters of the third placed party have more incentive to vote tactically, because the probability that any tie for the lead involves their party has gone down. On average this is indeed what

happens in English constituencies, if the difference between the second and third party shares is held constant. Thus, the Myatt model contradicts the standard intuition because the incentive for tactical voting decreases as the chances of influencing the result increases, after controlling for the distance from contention effect.

Another way in which the Myatt model and the standard intuition differ is that incentives for tactical voting in the standard intuition do not depend on which of the leading two parties is the second and which is the third choice party. In the Myatt model this not so. There is no binding rule as to whether the Myatt tactical incentive is greater when the second preference is the winning party or the runner-up, but there is a general tendency. Among those with unique second and third choice parties in the risk population, the Myatt tactical incentive is greater for those whose second choice party was the winning party in their constituency, even after controlling for the distribution of the vote in the constituency. This may be considered surprising because tactical voting is often thought to be aimed at ousting incumbents and incumbent parties, or a least that is how the rhetoric works (Fishman and Shaw 1989). The Myatt model suggests that the structural incentive is generally to the advantage of incumbents. Nevertheless, it is the difference on the question of a marginality effect on tactical voting that mainly distinguishes the Myatt model.

Test of the Myatt Theory

Much of the groundwork for testing the Myatt theory has been laid out above in the test of the standard intuition. Analysis is again restricted to those who have a first preference for a major party and are in the risk population in England. Whereas previously it was only necessary to define a unique first preference party for each voter, to test the Myatt theory a unique second preference party is also necessary. The second preference party is defined, for all respondents, as the party with the best strength-of-feeling score that is not the first preference party. The third preference party is similarly defined *mutatis mutandis*. Where there is a tie between second and third preference on the strength-of-feeling scores, question D below can sometimes show which of the two parties is really the second preference.

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D.   If  the  voting  paper  had  required  you  to  give  two  votes,  in
order  of  preference,  which  party  would  you  have  put  as  your  second
choice?
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In 6.2 per cent of cases it was still impossible to identify which party was the second and which was the third choice. Since the identification of a strict preference order is essential, the ties were broken in such a way as to prejudice the analysis against the Myatt model. To do this, one can assign the second and third party so as to minimise the value of the Myatt tactical incentive for tactical voters and maximise it for non-tactical voters. So, when there is an 'unbreakable' tie for second and third places on the strength-of-feeling scores, for a tactical voter the second choice is defined to be the party with the lowest vote share of the two. For non-tactical voters with a similar tie, the second preference party is the party with the highest vote share of the two parties. The use of this coding provides a 'worst case scenario' test of the Myatt model, but would be of no consequence for the test of the standard intuition hypotheses.

A similar test to that for the standard intuition using logistic regression analysis can be devised for the Myatt model. Here the predictor variables are the strength of preference for the first over the second choice party (first gap), the strength of preference for the second over the third choice (second gap) and the Myatt tactical incentive. The results are shown in Table 3 below. The first two explanatory variables are familiar from the test of the standard intuition. As before, tactical voting clearly decreases with the first preference gap and increases with the second preference gap. The association with the second gap is not significant for 1987 and 1992 and it is certainly weaker than the association with the first gap. However, the coefficients for each election show a monotonic decrease as the second gap increases and the result is significant in a pooled analysis.

Table 3 Logistic regression analyses testing the Myatt model

| | 1987 | | | 1992 | | | 1997 | | |
Variable	*B*	*s.e.*	*p*	*B*	*s.e.*	*p*	*B*	*s.e.*	*p*
First preference gap (base=0)			0.00			0.00			0.00
First gap = 1	-0.89	0.31	0.00	-1.14	0.26	0.00	-1.05	0.25	0.00
First gap = 2	-1.77	0.49	0.00	-1.87	0.43	0.00	-2.44	0.67	0.00
First gap = 3 or more	-1.80	1.12	0.11	-7.37	12.32	0.55	-1.89	1.54	0.22
Second preference gap (base=0)			0.05			0.18			0.00
Second gap = 1	0.36	0.52	0.49	0.34	0.42	0.41	1.49	0.48	0.00
Second gap = 2	1.01	0.51	0.05	0.55	0.41	0.18	2.11	0.47	0.00
Third gap=3 or more	1.16	0.54	0.03	0.96	0.47	0.04	2.43	0.48	0.00
Myatt tactical incentive (m=20)	0.45	0.14	0.00	0.68	0.11	0.00	0.56	0.13	0.00
Constant	-2.39	0.50	0.00	-1.82	0.42	0.00	-2.71	0.49	0.00

Notes: B is the coefficient, s.e. is the standard error and p is the p-value. P-values where no coefficients are show are those from the Wald test of the variable. Sample from the BES includes major party supporters in England in the risk population (see text for further information). In 1987 N=603, in 1992 N=435 and in 1997, N=465.

The Myatt tactical incentive can be computed from equations (1) and (2) above for various different values of the precision of information variable. A range of values between 2 and 100 have been tested and the incentive has a significant positive coefficient in each case. Table 3 gives the results for the Myatt model when the precision of information is 20. This value was chosen because it is a reasonable precision level and the coefficient of the incentive variable seems to have a maximum near 20. The test is also robust to the coding of those with a tie for second and third choice. If the test is repeated with the opposite coding from that discussed above, the results are similar. Furthermore, the Myatt model is in fact more general than the standard intuition because that it specifies a tactical incentive for all voters not just the members of the risk population, assuming that we can make sense of a negative incentive. Testing the Myatt model on all major party supporters again produces similar results, although sensitivity to all variables, especially the incentive variable, is substantially reduced. This is unsurprising given the dominance in the voting population of those who support the first or second placed party in their constituency and do not vote tactically.

Perhaps most remarkable is that a pooled analysis of all three elections including the strength of preference variables, the Myatt tactical incentive, and both the distance from contention and margin of victory, shows that the Myatt tactical incentive is a significant predictor while the distance from contention and margin are insignificant. This is amazing considering the high correlation between the Myatt tactical incentive and both the distance for contention and the margin of victory. In contrast with both the standard intuition and the Cox theory, the relationship between the frequency of tactical voting and the distribution of the vote in the constituency is exactly as specified by the Myatt model. Tactical voting increases with the Myatt tactical incentive as predicted and the association is statistically significant.

Conclusion

This paper has tested three theories of tactical voting. The standard intuition supposes that the most likely person to vote tactically will be indifferent between their first and second choice parties, strongly prefer their second to their third choice, and live in a marginal constituency where their preferred party is a long way behind the second placed party. These ideas are largely right but crucially, tactical voting does not increase with marginality as commonly thought. As an alternative, Cox's formal theory predicts that in any constituency there will either be nearly no tactical voting, because the second and third placed parties are close, or the third placed party will be almost completely strategically deserted. In fact, constituencies do not cluster in either of these two categories. Moreover, the pattern of tactical voting in the BES is not one that would push results in the direction supposed by Cox. The problem with the Cox model is that voters are assumed to be certain of the result in their constituency. The Myatt model incorporates the kind of uncertainty missing in the Cox model and generates a formula for the incentive to vote tactically.

The pattern of tactical voting in England in 1987, 1992 and 1997 does conform to the prescriptions of the Myatt theory. Although it is possible to devise more sophisticated tests, the main comparative statics of the Myatt model have been shown to be accurate. Certainly there is no evidence here against the Myatt model whilst there is strong evidence against the standard intuition and the Cox theory. To this extent the Myatt model is the best account of tactical voting in England yet. However, the formal theory involved is sufficiently complex as to raise important methodological questions. Does it matter that voters do not make the calculations prescribed by the Myatt model? Is predictive efficacy enough for a social scientific theory?

It seems odd to think that some voters are consciously voting tactically, but make a sub-conscious tactical decision according to a mechanism that has not, until recently, been elucidated by anyone. But, it is even more odd to think that people have been making tactical decisions according to some other mechanism, however intuitively obvious, when there is no evidence for it. The basic components of the Myatt model are easily understood and practically impossible to disagree with; voters can only affect the result when they are pivotal and they are always uncertain of the result. What the Myatt model does is show how these features of the voting context affect the optimal vote choice for an instrumentally rational voter. Although the predictions are counter intuitive it should not really be surprising to find that they work, because the premises and the framework of the Myatt model are essentially correct.

One general lesson from this paper is that formal theorising is useful for understanding political behaviour when intuition fails. Previously, authors such as Green and Shapiro (1994) have claimed that rational choice theorists merely formalise the intuitive and show us what we already knew to be true. Here we have a case in which formal theory contradicts standard intuition and better explains the empirical findings. However, another general lesson from this paper is that the success of formal rational choice theorising depends crucially on what is incorporated in the model. In particular, the failure of the Cox model is due to the failure to account for uncertainty within the formal model. When this is corrected, the predictions are radically different and much better.

Acknowledgements

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