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Access details: *subscription number*
Publisher: *Routledge*
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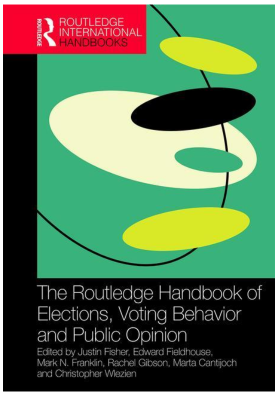

The Routledge Handbook of Elections, Voting Behaviorand Public Opinion

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Election forecasting

Publication details
https://www.routledgehandbooks.com/doi/10.4324/9781315712390.ch39
Stephen D. Fisher
Published online on: 26 Sep 2017

How to cite :- Stephen D. Fisher. 26 Sep 2017, *Election forecasting from:* The Routledge Handbook of Elections, Voting Behaviorand Public Opinion Routledge
Accessed on: 03 Oct 2023
https://www.routledgehandbooks.com/doi/10.4324/9781315712390.ch39

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ELECTION FORECASTING[1]

Stephen D. Fisher

Introduction

Election forecasting is a mug's game, so I'm often told. Maybe. But it is fun, people will do it and it is important enough to try to do well. Election forecasts, even if they are not much reported directly in the media, do get noticed and influence the tone of media election coverage. They set expectations so strongly that people are shocked if the forecasting consensus is seriously wrong and substantial amounts of money are made or lost on various markets.

People both inside and outside academia do election forecasting and this is an area where academics learn a lot from non-academics and vice versa. Sadly much of the non-academic forecasting is not well documented. Descriptions of methods tend to be thin on detail and often disappear after elections with little or no postmortems. For this reason the references in this chapter are dominated by the academic literature, but most of the principles and issues discussed apply to the non- and semi-academic forecasting too.

The bulk of the chapter reviews various different methods being deployed at the time of writing. For reasons of space and because the chapter is required to focus on the state-of-the-art material, older literature and debates have been neglected in favor of citing recent examples of applications of well-established methods. Readers can follow citations within citations to identify the origins and development of particular approaches. Also there are several recent reviews, albeit they are on specific elections, collections or methods (Lewis-Beck and Bélanger 2012; Linzer and Lewis-Beck 2015; Fisher and Lewis-Beck 2016; Ford et al. 2017; Murr 2017; Graefe 2017).

The discussion here is dominated by Britain and the US mainly because those countries are where there has been most methodological innovation that is also documented in scholarly journals. But developments in these countries do not always travel well to other countries with different institutions, politics and especially different data availability. There are significant and fascinating developments across Europe and elsewhere that deserve more attention outside those countries, just some of which have been reviewed here.

This chapter does not cover amusing historical correlations between election outcomes and obviously irrelevant variables, such as the color of cup-winning football tops (Mortimore 2014). No matter how strong they are, such relationships are almost certainly spurious and eventually break down. Instead this chapter covers methods of election forecasting that are of most interest

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either from a substantive electoral behavior point of view or from broader social scientific and methodological perspectives.

After reflecting a little on what forecasting is used for, the bulk of this chapter reviews election forecasting methods. This section is organized somewhat by method and somewhat by information type. Issues of seat forecasting are dealt with in a separate section later. The concluding discussion makes some observations on what more could be learnt and what could be done to improve forecasting further.

What is election forecasting for?

Election forecasts constitute data for analyzing current political situations, performance of political actors and the effect of substantive events. For instance, Berg, Penney and Rietz (2015) use prediction market data to analyze the "political impact" of events. For this approach to be valid requires forecasts to constitute meaningful measures of current political standings for candidates and parties – that is, they have to be based on consistently reliably good forecasting methods.

But what political scientists want to learn from election forecasting is often something more than or different from how to get the most accurate predictions. Election forecasting is also about trying to understand how elections work and how good political science theories are at predicting the future as well as explaining the past. For instance, Lewis-Beck and Stegmaier (2014) provide a list of five main substantive lessons from the experience of forecasting US presidential elections: electoral cycles exist, campaigns matter, the economy matters a lot, voters are retrospective and myopic, and voters cannot easily be swayed.

In truth this list could be disputed, but even to the extent that the forecasting literature does show us these things, they have also been established without forecasting. What is not clear is what scholars are learning from the experience of forecasting that they cannot or are not learning from traditional theory testing research. The idea that there are some things that can only be learnt from forecasting is perhaps too tall an order. But forecasting does help focus the mind as to what is important for really influencing election outcomes. While traditional post-election survey research points to many powerful predictors of vote choice at the individual-level that suggest macro-level predictors such as partisanship, it turns out that changing macro-partisanship is not necessarily a powerful predictor for forecasting election outcomes (Campbell and Garand 2000). This has important implications for the ways in which we should interpret findings from individual-level analysis. Predicted probabilities from individual-level regression models cannot necessarily be interpreted as estimates of effects on election outcomes.

Rightly or wrongly, the demand for forecasts sets an academic agenda. For example, key to some debates on US election forecasting is the question of how much weight to put on vote intention polls at what stage in the campaign. This problem is part of the motivation for some of the research on campaign dynamics. This may be the tail wagging the dog for some, but it is useful knowledge generation for others.

How is election forecasting done?

This section starts with more traditional social science theory based models before turning to models based on vote-intention polls, primarily because the most prominent vote-intention models now build on the traditional models. We then turn to prediction markets, citizen forecasting, experts and other more diverse sources. Processes of synthesizing different methods and combining forecasts are also discussed in this section, but some methodological issues are held over to the following section on seats forecasting.

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Structural models and the "fundamentals"

Structural models are the archetypal political science forecasting models. They are traditionally based on fitting a parsimonious theoretically informed regression model to an historical set of elections and then using the resulting equation for prediction. The most common predictors are economic and political factors: the fundamentals. The term structural refers to the fact that the predictors structure the vote in the sense of there being a causal relationship.

The predictors in structural models are referred to as the "fundamentals" on the basis that they are powerful exogenous factors driving election outcomes. However, the term fundamentals is increasingly used very loosely to refer to any variable in a forecasting model that might possibly be related to vote choice. This may not matter for forecasting purposes but it does undermine the idea that the models are simultaneously telling us something about causal processes. Many so-called fundamentals are either not very powerful predictors (Lauderdale and Linzer 2015), or not mainly exgenous and so not really fundamental to vote choice.

Electoral pendulum, election cycles, incumbency and the cost of governing

Shakespeare wrote about "a tide in the affairs of men." The notion that there is an electoral pendulum that swings back and forth between the two main parties/blocks competing for power has a long heritage and is commonly assumed among political elites and commentators. Helmut Norpoth and his colleagues have shown that in Britain and the US the pendulum swings between the two main parties roughly every two and half elections (Norpoth 2014; Lebo and Norpoth 2016). By this account, it is entirely unsurprising that Cameron and Obama should have won re-election.

For some the pendulum is driven by the "costs of governing" (the accumulation of resentment), which is often operationalized as just time in office. But note that some versions of the pendulum model involve not steady deterioration but a positive incumbency advantage for the head of government for first re-election, turning to disadvantage in subsequent attempts.

Incumbency effects at the district level are vital to forecasting in the US congressional elections (Cox and Katz 2014) and important components in some other majoritarian systems (Fisher 2016). They too have their own dynamics over elections, but different from those affecting heads of government.

The pendulum effect is often referred to as an election cycle, but that term is also used to refer to the period between two elections. In Britain, at least, there is common folklore that governments have a honeymoon just after winning an election, then suffer mid-term blues, from which they recover (somewhat) in the final months. These kinds of cyclical dynamics can be used in forecasting, but to different effect from the multiple-term pendulum model (Fisher 2015).

Moreover, the word cycle has been used to refer to poor government performance in mid-term and/or second-order elections (Bellucci 2010). In US mid-term election forecasting the same idea is used but without the same language of cycles (Silver 2014).

Economic voting models

The main idea behind these is that the voters' tendency to reward or punish governments based on the performance of the economy is so strong that we can anticipate election outcomes on the basis of objective macro-economic indicators. Economic evaluations are also used but they do raise questions about endogeneity when it comes to interpretation.

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Economic indicators have been particularly popular for forecasts of US presidential elections, especially more than a couple of months in advance because the identity of both main candidates is not (well) known and opinion polls are still relatively uninformative (Jerôme and Jerôme-Speziari 2012).

In parliamentary democracies, the polls are generally much more informative about the eventual result earlier on (Jennings and Wlezien 2016), and so there may be less need to use economic indicators instead of polls. Furthermore, retrospective economic voting depends on the kind of clarity of responsibility that is in relatively short supply in more consensual proportional systems. So forecasting models in these places may struggle to identify who will be held accountable for economic outcomes.

There are various intriguing forecasting papers that address these issues. In Norway, low unemployment helps the social democrats whether they are in or out of government (Arnesen 2012). In Spain it is just the center-right in government that is affected (Magalhães, Aguiar-Conraria and Lewis-Beck 2012). More strikingly in Austria the combined share of the two main parties (at least one of which is always in government, sometimes both) drops with rising unemployment (Aichholzer and Willmann 2014). Economic indicators even work for forecasting in francophone Belgium for a long period when voters did not have the opportunity of voting for the (Dutch-speaking) prime minister's party (Dassonneville and Hooghe 2012). A key lesson here is that the appropriate forecasting model with economic indicators is highly context dependent.

Leadership evaluations

No party has lost a British general election when it was ahead on both leadership and economic competence evaluations (Kellner 2014). In the run-up to an election, we expect the party with the best thought of leader to be most likely to win and some forecasting models use leader ratings to capture this – for example, in Germany (Norpoth and Gschwend 2010) and Britain (Lebo and Norpoth 2016). Similarly government evaluations are successful predictors in Italy (Bellucci 2010), but this measure comes closer to being effectively a proxy for vote intention than do leadership ratings.

A rather different approach uses primaries and internal party leadership contests as measures of relative candidate qualities. Those candidates and leaders who won their party competition more comfortably are more likely to be clear election winners. Thus the "primary model" for US presidential elections is based on evidence that those candidates that win their early primaries most emphatically are more likely to win the general election (Norpoth and Bednarczuk 2012). In Britain the party leader who won their internal leadership among MPs contest most clearly tends to become prime minister (Murr 2015a).

Vote-intention poll-based forecasting

The prevalence of vote-intention polls and the media demand for continually updated forecasts mean that aggregation of opinion polls is the main (but usually not sole) basis for the most high-profile election forecasts. In the US, these include Drew Linzer's votamatic.com, Sam Wang's Princeton Election Consortium (election.princeton.edu), Simon Jackman's HuffPollster forecasts and most famously Nate Silver's fivethirtyeight.com. There have similarly been poll aggregation based forecasts in Britain (electionforecast.co.uk, electionsetc.com and Polling Observatory), Germany and elsewhere. This broad approach was also applied to the UK's referendum on EU membership (Fisher and Renwick 2016).

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Naturally, forecasting based on poll aggregation faces two main challenges: how to aggregate and how to forecast. Poll aggregation ranges from simple averaging of recent polls to technically sophisticated Bayesian state-space models. There are a lot of modeling choices to be made in the process, including whether and how to measure pollster quality, how to weight pollsters, how to estimate differences between pollsters (house effects) and how much they change, how to forecast overall polling industry accuracy and how to estimate uncertainty in all these things. Much of the analysis of these things is inherently technical with conclusions highly contingent on historical experience and context rather than on theoretical grounds. Perhaps the most important of these issues is the extent to which polls are on average right, or wrong, in a predictable manner.

For instance, the successful poll aggregation based forecasts of the 2012 US presidential election are rightly acclaimed (see, for example, Linzer 2013). But it is worth noting that they relied on there being no significant bias for the average pollster; a reasonable assumption given the historical experience in the US. By contrast the same assumption was disastrous in the 2015 British general election, which saw massive industry bias which was only partially anticipated and only by some (Fisher and Lewis-Beck 2016).

Another salutary lesson from recent British politics regards house effects. Most state-of-the-art poll aggregators use some variation of the Kalman filter or Bayesian state space model. These models treat polls as noisy indictors of true public opinion. In trying to estimate levels of party support they take into account the sample size of polls and corresponding sampling variation, and they simultaneously estimate the extent to which different pollsters tend to produce systematically high or low estimates for particular parties (house effects). Such differences typically exist because of methodological choices by pollsters, not least the mode of interview (face-to-face, telephone or online). To estimate these models, house effects need to be assumed to be stable, or at least evolve only slowly. It takes quite a few polls to distinguish between a modest change in house effect from statistical noise. However, both the 2014 Scottish independence referendum and the 2015 British general election saw house effects collapse with apparent herding at the end of the campaign (Fisher 2016; Sturgis et al. 2016). In the UK's EU referendum there were many methodological changes within the final weeks, and especially for the final polls (Curtice 2016), which meant that house effects could not adequately be estimated. All three of these events also saw a dwindling in the final weeks of what were earlier in the campaign relatively stable differences between telephone and internet polls (Fisher and Renwick 2016; Sturgis et al. 2016). With significant risk of rapidly changing and unidentifiable house effects, more simple poll averaging is likely to be more robust than a model which assumes stable house effects.

Aggregating polls to form estimates of current vote intention provides a now-cast. There remains the question as to how to forecast the future. Information about how things are likely to change by election day essentially comes from history, including some structural models of the kind discussed in the previous section. So while the prominent poll aggregator forecasts are almost entirely dominated by polls close to the election, they usually depend substantially on a structural model when forecasting from several months out. So while they are often called aggregators, they are actually synthesizers.

Synthetic models

Synthetic models typically seek to incorporate the best of both the polling and the structural models. They are typically built using historical evidence of the relative weight to put on economic data and polling data, which varies according to how far away the election is (Linzer 2013; Lewis-Beck and Dassonneville 2015; Lewis-Beck, Nadeau and Bélanger 2016). Arguably

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many structural models comprised various different components (e.g., economic and political) and so could be thought of as synthetic models. But this term is not used in that way.

There was considerable debate in the run-up to the 2014 US Midterms as to whether a synthetic or polls-only model would be best (Blumenthal, Edwards-Levy and Lienesch 2014; Wang 2015). Given that the case for a synthetic model is stronger further out from the election, this debate raises important questions about how to evaluate dynamically updated forecasts. Some methods may be better at making forecasts at some times than others.

Betting and prediction markets

Prediction markets are a favored source of forecasts by many economists and business people who believe in the power of markets to aggregate information. Betting markets are similar but involve a bookmaker setting odds rather than participants trading directly with each other. With enough participants, betting odds are primarily driven by what the punters think collectively about the relative chances of different outcomes, once you account for the bookmaker's over round (setting odds with implied probabilities that sum to greater than 100 percent).

The theoretical argument for political prediction markets is that they involve people staking their own money, sometimes serious amounts of it, and, if they are rational actors, they will be taking account of all the information available to them. One of the intriguing issues for election forecasters is that the information punters use partly comes from opinion polls. So it is not clear that betting and prediction markets do any better than polls properly interpreted (Erikson and Wlezien 2012). UK constituency betting markets have been found to have various systematic biases (Wall, Sudulich and Cunningham 2012), and there are examples of poor performance of markets, such as Germany in 2013 (Graefe 2015b) and the UK's EU referendum (Fisher and Shorrocks 2016). One concern when they do go wrong is that we typically do not know why. More research is needed on who bets, why and on what basis. However, while acknowledging that they can sometimes be manipulated, Graefe (2017) argues that prediction markets have a better track record than forecasts based on opinion polls or structural models.

Citizen and expert forecasting

While the implied forecasts from betting markets are based on the money staked on each side winning, a related process of aggregating individual forecasts is known as citizen forecasting. The central theoretical idea here is that the average (or median) guess from a large (ideally representative) sample, referred to as a voter expectation survey, will be close to the truth even if most guesses are wide of the mark. This "wisdom of crowds" principle works if people on average have a better chance of getting the answer right than wrong. Citizen forecasting has a very good track record in a number of countries (Murr 2017), including Britain (Murr 2016) and especially the United States. Expectation surveys in the US arguably outperform vote intention polls, prediction markets, structural models and expert judgments across the final 100 days of the seven presidential elections between 1988 and 2012 (Graefe 2014).

The relative performance of expectation surveys and prediction forecasts raises particularly interesting questions about information aggregation. Advocates of betting markets suggest that the risks that bets involve concentrate the mind and people are only willing to place them if they feel they know enough to think that the bet is worthwhile. The supposed advantages of betting markets depend on the participants being unrepresentative and exclusive of the ignorant. Citizen forecasts by contrast make a virtue of having a much more representative sample of the population at the expense of numerous potentially poor quality responses.

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What meta-analyses there are suggest that citizen forecasts are typically more accurate than prediction markets (Graefe 2014, 2017; Murr 2017). This finding would seem to vindicate those who argue for collective democratic decision making against those who worry about voter ignorance.

This is not to say that some citizens are not better than others at forecasting elections. Some are and forecasts can be improved by identifying the better forecasters and weighting their forecasts according to a measure of competence (Murr 2015b). Amongst other characteristics, Murr found that better forecasters tended to be older and more educated and less likely to have a strong party identification.

Intriguingly, interest in politics was not consistently linked to forecasting success among citizens and professional political experts are not necessarily the best. Indeed the meta-analyses typically show voter expectation surveys out-performing expert surveys (Graefe 2014, 2017; Murr 2017). At the 2015 British general election expectations of academics, pollsters and journalists were very similarly wrong, perhaps because of looking at the same sets of polls and forecasts on social media, but journalists were slightly less wrong than the other two groups (Hanretty and Jennings 2015). In a similar expert survey before the UK's EU referendum in 2016 it was the academics who were slightly less wrong than the journalists (Jennings and Fisher 2016).

One of the issues that this EU referendum expert survey raises is whether respondents should be asked who is more likely to win or what the probability is of each side/candidate/party winning. Eighty-seven percent of the experts thought that Remain was most likely to win, but the average probability was assessed to be just 62 percent. More research is needed to see if citizen forecasts might be similarly more informative and improved if citizens too were asked to assess probabilities. Also, as with prediction markets, it would be helpful to know more about how people make their forecasts and on what basis. Currently we know little about why citizen forecasts sometimes do well and sometimes do not.

Big data

With the growth of social media there have been recent attempts to forecast elections with content from Twitter and other platforms, but with little success (Huberty 2015; Burnap et al. 2016). A lot of learning about the relationships between online content and election outcomes is still needed, presumably involving cross-national analysis because of the paucity of elections with enough online content in any single country. However, since this is a fast-changing area with changing platforms and participants, what holds about social media for one election may not do so for the next.

Combining forecasts

What aggregators are to pollsters, combiners are to forecasters. Whereas synthetic models include different kinds of predictors in one model, combining forecasts is a process of averaging the forecasts of different models (often based on different predictors). For example, PollyVote.com averages forecasts from polls, prediction markets, expert judgments, citizen forecasts and various quantitative models with considerable success in the US (Silver 2012; Graefe 2015a; Rothschild 2015) and Germany (Graefe 2015b). A similar approach was also taken at ElectionsEtc.com to forecasting the UK's EU referendum (Fisher and Shorrocks 2016).

Naturally it matters how forecasts are combined. Both PollyVote and ElectionsEtc first identified categories of forecast type (e.g., betting markets, prediction markets, citizen forecasts, etc.) and averaged the forecasts within each type before averaging across types. This can effectively

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mean that various different econometric models each carry very little weight in the overall forecast. The advantage of this approach should be that it ensures that lots of forecasts based on just one source of information do not swamp a series of individual forecasts each based on its own particular source. However, there are major judgment calls required regarding when to consider two forecasting methods sufficiently similar to constitute examples of one type, or when they should be considered instances of two different types.

Another related issue is what weight to give each forecast (or forecast type). Montgomery, Hollenbach and Ward (2015) advocate ensemble Bayesian model averaging whereby forecasts are weighted according to prior performance. This is not always possible, but even when it is Graefe et al. (2015) argue that weights should only be used if there is strong evidence for them.

How to balance forecasts based on recent information with those that are older is another important issue. There is plenty of scope for fruitful work in this area, perhaps also more broadly in thinking about how to combine both forecasts and raw information, maybe with a neural network approach (Borisyuk et al. 2005).

Forecasting seat outcomes

The audience for election forecasts is much more interested in who gets elected and who ends up governing than they are in the share of the vote. So this section discusses how seat forecasting is done.

Votes-to-seats and multi-level votes and seats forecasting

In most proportional representation systems, forecasting the number of seats for each party nationally is relatively straightforward given a forecast of the number of votes for each party nationally. For single member district systems, uniform change (or swing) is a traditional model that, for all its imperfections, is not a bad starting point (Jackman 2014; Fisher and Lewis-Beck 2016). Arguably uniform change is not a simple votes-to-seats translation because it relies on constituency-specific information, but that information is static and prior to the campaign.

Many of the most complicated forecasting models in Britain and the US generate probabilistic seat (or electoral college) forecasts by having vote forecasts at different levels based on dynamic data at different levels. We might term these multi-level votes-to-seats forecasting methods. At recent US presidential and senate elections, there have been enough state-level polls to predict each state outcome separately but borrow strength from information from others and the national level (Linzer 2013). For the US House of Representatives, a different approach is needed (Bafumi, Erikson and Wlezien 2014).

In Britain, constituency polls were rare until 2015 but were still not ubiquitous. Still it was possible to model the GB share of the vote at the constituency level. A further complication was that in 2015 changes in party support were so different in Scotland from the rest of Britain that forecasts needed to respect this and use information from Scotland-only polls. Reconciling information at these different levels is tricky, but Hanretty, Lauderdale and Vivyan (2016) present a thoughtful and sensible solution. They adjust a predicted constituency pattern to fit the national forecast by assuming the same pattern of turnout at the previous election and shifting the vote shares in each constituency using a generalized normal distribution. Most of the time the effect of this is close to that from applying a uniform change adjustment, but for small parties it ensures against predicting negative vote shares.

To the extent that multi-level forecasting models use individual-level data, they typically model it to inform their district-level models, perhaps along with district-level opinion polls

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(Fisher 2016; Ford et al. 2016; Hanretty, Lauderdale and Vivyan 2016). However, Mellon and Fieldhouse (2016) present a model which reconciles district-level variation in individual voter transitions with national-level vote share forecasts.

There is still more potential to develop this framework for national elections. For example, the results of local/municipal elections have been used to forecast national elections (Prosser 2016; Rallings, Thrasher and Borisyuk 2016), but not simultaneously with national-level polling data.

A multi-level strategy, together with sub-national election modeling, has been advocated as a solution to the small N problem for estimating structural models for recently democratized countries (Turgeon and Rennó 2012). Taking the multi-level approach to a level above the national, Simon Hix and Michael Marsh use both national and Europe-wide factors to forecast outcomes of European Parliament elections for the whole of the EU (Hix and Marsh 2011; Hix, Marsh and Cunningham 2014).

Seats bypassing votes

It is possible to forecast seat outcomes without forecasting vote shares. In the case of citizen forecasting, this is unavoidable: surveys ask who will win, not what the shares of the vote for each party in their constituency will be (Murr 2017). In some other models, vote shares are essentially bypassed on the basis that the main target of the forecast should be modeled directly (Whiteley et al. 2016; Lebo and Norpoth 2016). What is not clear are the circumstances under which this is a more successful strategy than providing an equivalent model for votes and then translating votes to seats. This would seem to be a safer strategy, respecting the logic of elections that seats depend on votes.

Forecasting government formation

While the public are most interested in who will form the next government, forecasters rarely attempt to tell them this when there is a significant chance that no party will win a majority. In multi-party systems where coalition government is common, uncertainty over seat outcomes implies uncertainty over governing outcomes, even if there is assumed to be certainty over government formation conditional on any particular seats outcome. While there are examples of this kind of government forecast (see, for example, Fisher 2016), there are none that I know of which allow for the possibility that different governments might form out of the same election outcome. It is not clear what models would inform the estimates of the conditional probabilities required for such a forecast, and they may need to be partially subjective.

Conclusion

Philip Tetlock is famous for dismissing "expert" forecasters as worse than dart-throwing chimps. His most recent book makes it clear that he does not intend this remark to apply to those, including "polling analysts like Nate Silver," with forecasting models that have been tested and revised to calibrate accuracy (Tetlock and Gardner 2015). While Tetlock is right to try to distinguish systematic scientific forecasting from the more ad hoc kind, it is not clear that any election forecasters have models they can confidently say are well calibrated for future political events.

For most there is a lot of room for improvement in uncertainty estimation. Lauderdale and Linzer (2015) make a powerful argument that established structural models in the US understate

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prediction uncertainty in various ways. Correcting for these problems they show that, "there is not sufficient historical evidence to warrant strong, early-campaign assessments about the probable outcome of a presidential election." This is a major challenge for all structural models to improve our understanding of how much election outcomes can really be said to be predictable from particular factors.

More generally, uncertainty estimation is important for well-calibrated forecast probabilities for all methods. Ultimately though, forecasting, including uncertainty calibration, is about assuming the future will be like the past. There is plenty of reason to expect that electoral politics, surveys and markets will not continue to operate just as always. This additional uncertainty is something producers and consumers of forecasts need to be aware of.

There is hope for more accurate and precise forecasts though. The academic literature reviewed above shows increasing methodological sophistication, richer data becoming more available, understanding of relative advantages of different methods becoming stronger and forecasting quality improving. Election forecasting should continue to improve with more research.

Not least of the priorities should be more meta-analysis. Those that have been conducted so far have been helpful for giving us a broad overview of the relative performance of different methods. For example, looking across forty-four elections from eight countries, Graefe (2017) argues that prediction markets beat structural models and opinion polls, but not citizen forecasts, with combination forecasts doing best of all. Further meta-analyses would be helpful for understanding the circumstances under which particular methods do relatively well. We need to know more about the extent to which particular political systems are best served by particular forecasting methods and how much the answers depend on how close the election is. The well-known idea that opinion polls are best very close to an election but citizen forecast and prediction markets do better further out could do with more systematic assessment.

While the performance of synthetic models and particularly combined forecasts is impressive overall, it is not yet clear when and why these forecasts go seriously wrong. Many election forecasts do worse than assuming the election outcome will be the same as the last one. We need to understand the contextual factors influencing the absolute performance of forecasting methods better. While some methods did better than others in forecasting the 2015 British general election, the main problem was that all the models, methods and markets were way out, failing to predict the Conservative majority by a large margin (Fisher and Lewis-Beck 2016). Similarly, all the forecasting methods before the UK's EU referendum pointed toward Remain (Fisher and Shorrocks 2016). Forecasts from different methods should be more like Tolstoy's happy and unhappy families: they should either be accurate in the same way or inaccurate each in their own particular ways. It is easy to see how the serious 2015 British polling miss affected all poll-based forecasts, but more difficult to identify is how and why the betting markets and expert and citizen surveys were similarly misleading. What matters in this context is not who did best but why they all failed, and how the failures of different methods are related to each other.

Despite this and other major failures that loom large in the public mind, election forecasts are still much better than Tetlock's dart-throwing chimps. Most of the time they manage to predict the right winner and get the flavor of the outcome well enough. There is an interesting question here as to how good forecasts ought to be. Forecasts the day before ought to be spot on; there shouldn't be much if anything left to change minds and generate late swing. Sadly final forecasts often fail to come as close as they should to eventual outcomes. Forecasts further out from an election should stand a high chance of being indicative of the outcome. But they should not be expected to be spot on. Voters are not and should not be entirely predictable.

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Note

1 Thanks to Andreas Murr for comments and to John Kenny and Eilidh Macfarlane for help compiling literature searches.

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