Using the Rolling Reinterview Design to Study Political Campaigns and Electoral Behavior

Thomas J. Leeper and Rune Slothuus
Department of Political Science
Aarhus University

August 11, 2016

Word Count: 7,685 (including references)

The study of political campaigns inevitably requires the measurement of public sentiment at multiple points in time. Researchers often face a choice between one of two methodological approaches. One involves a single cross-section of interviews or, perhaps repeated cross-sectional interviewing of different respondents at several points in time. The other involves a panel, where the same respondents are interviewed repeatedly. The latter has desirable properties with regard to cost of implementation and statistical efficiency, but introduces concerns about attrition, representativeness, and panel conditioning. We discuss an alternative, called the rolling reinterview design, which combines desirable features of both approaches in a way that reduces concerns about respondent fatigue and panel conditioning while retaining the ability to make within-subjects comparisons and with only a modest loss of statistical efficiency. This paper introduces the design, compares it to available alternatives, discusses several statistical and practical considerations, and demonstrates the value of the design for studying campaigns through a large-scale implementation during the 2014 European Parliamentary Election campaign in Denmark.

The study of electoral behavior is dominated by two survey-based empirical approaches: repeated cross-sectional surveys and multi-wave panels. Single cross-sections are also prevalent (such as the Comparative Study of Electoral Systems; CSES 2015). The repeated cross-sectional approach is well-suited to the study of aggregate changes in the public's preferences and for predicting turnout and vote choices in elections. It is consequently popularly used in commercial polling and has been applied in places such as the Canadian Election Study (Johnston and Brady 2002). The multi-wave panel – which in practice often involves just pre-election and post-election interviews – is better suited for the study of individual-level, within-subjects changes over the course of a campaign. The panel has thus been a staple of election research for seven decades (Lazarsfeld 1948; Berelson et al. 1954; Campbell et al. 1960). Large, well-funded initiatives aside (e.g., national election studies), researchers must balance these design trade-offs against the finite resources available for interviewing. Individual research projects aiming to study campaigns, elections, and public debates without the resources of national election studies choosing between repeated cross-sections and panels must weigh the advantages of panels against their cost and the accompanying risks of conditioning and attrition.

Balancing these trade-offs, we discuss the value of an alternative approach to studying opinion dynamics over time: the rolling reinterview design. The RRD attempts to maximize the advantages of both cross-sectional and panel designs by offering both repeated cross-sectional samples and within-subject analysis without needing to reinterview each respondent at every panel wave. We begin by situating the RRD within the broader set of available survey research methods and describe its strengths and weaknesses. We then demonstrate the usefulness of the approach through a case study of the 2014 European Parliamentary Election in Denmark.

Studying Campaigns and Elections with Panels and Cross-Sections

Since its invention over 70 years ago (Lazarsfeld & Fiske 1938), the panel design has become a powerful and prevalent research tool, particularly in the domains of political behavior and public opinion. Indeed, the most seminal research in this area was conducted using repeated interviewing of the same survey respondents over time (see Lazarsfeld, Berelson & Gaudet 1948; Berelson, Lazarsfeld & McPhee 1954; Campbell et al. 1960; Converse 1964). A panel allows researchers to study individual-level, within-subjects changes. In the absence of experimental manipulation, this design can help to unpack the causal effects of events and campaign dynamics by using observations as their own controls. And, causal inference aside, changes in a measure over time will tend to have lower measurement error than a cross-sectional measure of the same construct (Ansolabehere, Rodden & Snyder 2008). Particularly in the online era where respondents can be recruited from standing online pools, panel research is more feasible than ever (Iyengar & Vavreck 2011; Vavreck & Iyengar 2011; Callegaro et al. 2014).

Yet the costs of panels are substantial. One relates to the high cost of completing a full set of interviews for every panel wave (due to interviewer time and/or respondent incentives). These costs are lessened by online modes of interviewing and recruitment from stable panels of respondents, but remain problematic even in those contexts. For example, the 2008-2009 ANES online panel study used incentives of $10/respondent/month for 21 months, and experimented with $30/respondent/month and $50/respondent/month incentives for dropouts, and still experienced a decline in overall response rate from 42% at recruitment stage to 23% by the end of the panel (DeBell, Krosnick, and Lupia 2010).

Additional concerns with panel studies relate to the concern that panel participation itself modifies respondents' behavior. This "panel conditioning" can come in various forms, including introducing *consistency biases* where respondents' responses are more stable and consistent than

if they were not being repeatedly interviewed, social desirability biases that lead respondents to misreport opinions on sensitive items because they know they are being monitored, or even changes in behavior such that respondents engage with a political campaign differently than if they were not being studied (Bartels 1999; Clinton 2001; Kalton & Citro 1995; Kruse et al. 2009; Sturgis, Allum & Brunton-Smith 2009; Warren & Halpern-Manners 2012). Such effects are typically thought to increase as a function of "time-in-panel" (Sharot 1991; but see Clinton 2001). For example, by asking participants to complete a two-wave, pre- and post-election survey might lead respondents to report post-election vote choices more consistent with pre-election intentions, report voting when they did not actually vote, or follow the campaign more than they otherwise would have in order to be prepared for the post-election interview.

The extant evidence for panel conditioning is somewhat mixed (see, for example, Clinton 2001). But Bartels (1999) shows that panel participation appeared to increase self-reported campaign interest and turnout intention during the 1992-1996 American National Election Studies panel. That finding means that panelists may have changed their engagement with the object of study – the 1996 U.S. Presidential election – as a result of empanelment. Hillygus, Jackson, & Young (2014) find that while repeated survey participation did not affect satisfying, it was associated with lower political knowledge, interest, engagement, and ideological extremism. That this contradicts Bartels' conclusions suggests that the precise impact of conditioning is unclear, and there is unfortunately little research on conditioning in panels outside of election contexts. One recent study using an online subject pool showed that participating in a large number of studies can produce "non-naivete" where respondents become aware of research paradigms and anticipate researchers' intentions, thereby inviting forms of socially desirable or hypothesis-confirming response behavior (Chander, Mueller, & Paolacci

2013). Together, these results suggest that panel conditioning is at least a potential risk even if the precise form of its effect remains unclear. We suspect that the risks of conditioning are even higher in cases where the study's focus is a lower-profile issue, such as a local election or referendum campaign, given the lower levels of interest and turnout in such elections compared to the national elections studied by Bartels and Hillygus et al.

These problems have been well-known since the earliest days of panel research: ''the big problem, as yet unsolved is whether repeated interviews are likely, in themselves, to influence a respondent's opinion'' (Lazarsfeld 1940). Yet panels are attractive for their ability to provide within-subjects comparisons. Alongside their prevalent use in elections research, panel designs have been seen as uniquely capable of settling the long-standing debate about the stability of citizens' opinions (Converse 1964; Converse 1970; Wiley & Wiley 1970; Achen 1975; Dean & Moran 1977; Erikson 1978; Feldman 1989; Hill & Kriesi 2001). A flip side of the issue of panel conditioning is that of panel attrition or mortality, wherein panelists leave the panel before its completion or cease to be available for interviews at all. Panel conditioning is seen as problematic because it results in collection of data that are biased by past study participation. Attrition replaces potentially biased data with data missing *not at random* (Little & Rubin 2014).[1] Attrition most obviously invites the possibility of a demographically unrepresentative sample at each wave of interviewing but can also affect other notions of representativeness (for a complete review, see Frankel & Hillygus, 2014).

The alternative to a panel design is the use of one or more cross-sectional surveys, such as a single pre- or post-election interview, the use of repeated cross-sections (such as in "trial

[1] One possible solution to the problem of panel attrition is the use of "refreshment samples" which stack a fresh cross-section onto a later panel wave that has been affected by attrition (Schifeling et al. 2015). Unfortunately, this approach has not seen sufficient use to know whether it is effective in practice.

heat" polling), or *rolling cross-sections* where a random sample of respondents is interviewed across overlapping multi-day windows (Johnston et al. 1992; Johnston & Brady 2002). Iyengar & Simon (2000) argue that the rolling cross-section provides "the greatest potential for the study of campaigns" (153), yet it – like all cross-sectional designs – sacrifice the ability to study individual-level, within-subjects changes over-time and thus offer a much weaker tool for causal inference. The advantage is a fine-grained insight into aggregate dynamics over the course of a campaign without the challenges of panel conditioning or the need to consider panel attrition and the ongoing representativeness of the panel. This approach is increasingly embedded as part of larger studies of elections in the form of "rolling thunder" study, with daily interviews of the entire course of an election (see, for example, British Election Studies in 2010 and 2015; Pickup & Johnston 2007).

While cross-sectional approaches may be particularly useful for election forecasting and inferring the aggregate impact of campaign events, many political science questions require the additional insights available from the within-subjects comparisons afforded by a panel design. How then can the limitations of the panel be overcome? One answer is the *rotating panel design* in which a multi-wave panel begins at time 1 with a fixed number of reinterviews at regular intervals over following months until time *t* and a fresh sample of respondents is empaneled at each period and are reinterviewed at the same intervals until time *t+1*, and so on. The rolling panel is used, for example, in the U.S. National Crime Victimization Study to produce a continuous panel that is refreshed monthly by interviewing each monthly subpanel eight times over a three-year period. The rolling panel is useful in large government surveys like the NCVS because the study's purpose is continuous monitoring over an indefinite period of time, but the overhead of constructing and maintaining such a complex panel does not make sense for

campaign research that is temporally bounded over a relatively short period of time.

The Rolling Reinterview Design

We argue that the best way to avoid the risk of panel conditioning and attrition and to reduce the cost of panel implementation without substantial loss of information is to interview each respondent fewer times without sacrificing the number of time periods observed. The canonical pre/post-election study achieves this with the fewest possible numbers of panel waves, but at the expense of any amount of continuous monitoring over the course of the campaign. To study both aggregate influences of a campaign and individual-level changes over-time, additional waves of interviewing are required. This approach, which we call the *rolling reinterview design* (RRD), adds these additional waves of interviewing to a panel study without increasing the response burden placed on individual panelists. The RRD method has seen some use in well-funded national election studies (e.g., British Election Study 2015).

Specifically, the rolling reinterview design involves an initial, early-campaign interview of a sample of respondents who are then empaneled for one or more waves of reinterviewing. In contrast to a traditional panel approach, however, in the RRD the reinterview waves consist only of randomly selected subsamples of the panel. Each reinterview wave thereby provides a representative, albeit smaller, sample of the population. In contrast to a rotating panel design, the RRD includes one initial cross-section in which all respondents are interviewed. Because the rotating panel design never observes all panelists at the same point in time, baseline characteristics across the rotating subpanels may differ for reasons endogenous to the campaign (i.e., subpanels that start later are only observed after the campaign begins; for an application of this approach, see Sides & Vavreck [2013]). The initial cross-sectional interview in the RRD

provides the further advantage of allowing more complex sampling designs when constructing the reinterview subsamples, something we return to at the end of the paper.

In the simplest RRD, two reinterview waves are conducted so that representative samples are interviewed at three points in time (initial panel interview, first reinterview with a half-sample, and second reinterview with the other half-sample) so each panelist is interviewed twice. As the number of subsamples increases, the number of time periods observed increases while holding response burden on the individual respondent constant. Tautologically, adding an additional wave of interviews adds one time period observation, but the division of the panel into RRD subpanels increases the number of observed time periods by one for each additional subsample. The substantial advantage of the RRD approach is thus achieved through an increase in both the number of subsamples, *k*, and the number of reinterview waves, *t*. The number of time periods observed is simply: $k*t+1$. As such, an RRD with three subsamples and only one reinterview round (i.e., each respondent is interviewed twice), yields 4 time period observations of the campaign with 50% fewer per-respondent interviews than a traditional panel. A design with five subsamples and three reinterview rounds (i.e., four total interviews per respondent) yields 16 time period observations with a 75% reduction in per-respondent interviews compared to a traditional panel.

This shows that an RRD can provide substantially more information about aggregate over-time dynamics in a campaign than a traditional panel with the same number of total interviews. The RRD therefore provides the possibility of a potentially fine-grained examination of aggregate changes over the course of a campaign (similar to repeated cross-sections) without many of the risks that accompany longitudinal panels with high numbers of per-respondent interviews. By retaining the core panel structure, however, individual-level, within-subjects

changes can still be examined. As an example, campaign studies are also often interested in changes over time (i.e., making comparisons between estimates obtained at separate points in time, such as before and after the campaign). Consider the simple case of seeing whether the debate increased or decreased support for a referendum question. In a repeated cross-sectional design, the estimate of this change would simply be: $\Delta = \Sigma X_2/n - \Sigma X_1/n$. In a panel, such as the RRD, this change can also be estimated using a within-subject difference estimator ($\Sigma(X_{i2} - X_{i1})/n$), which is identical. The variance in either case is:

$$\text{Var}(\Delta) = \text{Var}(\bar{X}_1) + \text{Var}(\bar{X}_2) - 2\text{Cov}(\bar{X}_1, \bar{X}_2)$$

In the cross-sectional design, each sample is independent, so $2Cov(\bar{X}_1, \bar{X}_2) = 0$. In the RRD, however, the observations of referendum support are paired over-time. To the extent that these are positively correlated (which is likely), the variance of the panel estimate is much lower than in a repeated cross-sectional design. Compared to a traditional panel, however, this covariance in an RRD is unlikely to be inflated by consistency bias (a form of panel conditioning) because respondents on later reinterview waves have not been repeatedly asked campaign-relevant questions.

A comparison of RRD to a traditional panel with respect to attrition is more complicated. If the number of response waves increases the rate of attrition, then the RRD reduces the likelihood of attrition by lowering the response burden for each panelist. On the other hand, if frequent participation helps to retain panelists (e.g., by keeping them engaged and interested in the study), then the RRD potentially increases attrition, particularly for individuals in late reinterview waves. Given evidence that questionnaire length appears to have little effect on reinterview response rates (Lynn 2014), we believe that the RRD is no more susceptible to attrition (or conditioning) than any other panel design. If one is particularly concerned that cross-

sectional estimates at each reinterview period are biased due to panel attrition, one possibility would be to supplement the RRD with a small replenishment sample (Deng et al. 2013). The empirical realities of attrition in the RRD merit further investigation.

The major disadvantage of an RRD is the trade-off between sample size and sampling variance of survey estimates.[2] As the number of subsamples increases, there is an attendant loss of precision, meaning that researchers must either accept lower precision of estimates during reinterview waves or increase the total panel size in order to retain a desired level of sampling error. Fortunately, however, sampling variance – in a simple random sample – is not linearly related to sample size but instead a function of the square-root of sample size, so the sampling variance for the each of two subsample reinterviews is only modestly larger than for estimates obtained from reinterviewing the sample as a whole. Figure 2 displays the standard error of a proportion (assuming $p=0.50$), such as the proportion voting "yes" on a referendum, given different sample sizes. The solid black line is the standard error for the proportion in the initial full-sample interview and the various dashed lines represent the standard errors for subsample reinterview waves associated with 2, 3, 4, or 5 evenly-sized sized subsamples. As should be clear, as the total sample size increases, the standard errors of estimates from each reinterview wave are almost no larger than for the sample as a whole. For example, for a panel with a total $n=3000$, the standard error for the initial interview would be 0.009 (or just less than 1-percentage point), whereas the standard error for each of five subsample reinterviews ($n=600$) would be 0.02 (or just over 2-percentage points). This hypothetical RRD could estimate vote intention at

[2] Another limitation relates to the loss of a balanced panel, something that may be particularly consequential for estimating panel regression models such as Arellano-Bond models or "mover-stayer" mixed markov latent class (MMLC) models (e.g., Hagenaars and McCutcheon, 2002). Given that these models tend to require many complete waves of data (at least four) and therefore are likely feasible only for researchers with substantial resources, they remaining fairly rare in the campaigns and elections literatures.

six points in time with the same precision as a traditional panel at the first wave and only a one-percentage point loss of precision at the other five points in time. Given the previously highlighted advantages of estimating within-subject changes, this loss of precision due to subsampling could be easily made up for through the use of lower-variance within-subjects estimators. A larger total panel size would of course reduce the marginal loss of precision even further. Thus, regardless of whether data from an RRD are treated as repeated cross-sections or as a panel, the statistical properties of the design are quite good.

One special advantage of the RRD design compared to a repeated cross-sectional or panel design is that it generates missing data for a subsample for respondents (those not interviewed in a given reinterview wave) that is *missing completely at random* (Little & Rubin 2014) but identical, in expectation, to the observed data for respondents interviewed in that wave. In other words, because interviews and non-interviews at a given reinterview wave are random samples of the whole panel, they are identical to that whole sample and to each other, in expectation. Because the missing and observed data are identical on average, the MCAR assumptions of out-of-the-box missing data imputation techniques are – atypically – satisfied and it should be possible to design and estimate an imputation model based on observed covariates from the first interview wave and apply it to infer the missing values for those not selected for interviewing, creating a further gain in statistical power.[3] Indeed, were one to use a multiple imputation procedure to completely replace the values variables unobserved due to respondents not being invited to a particular reinterview wave, a potentially substantial gain in statistical precision is possible. This gain in precision can nearly make up for the intentional nonresponse of non-

[3] One particularly useful way of conducting an RRD may be to select respondents for reinterviewing using a stratified survey design to ensure subgroup comparability across waves. In our example study we relied on simple random sampling to select respondents for each reinterview wave.

interviewed respondents, at the cost of potential bias in sample estimates.

As an example, Figure XX displays the results of monte carlo simulations wherein observed data from subsamples are used to multiply impute (k=5) missing values for randomly assigned nonrespondents in that subsample.[4] The figure shows, on the x-axis, bias in the estimated sample mean (compared to the true population mean ~ N(0,1)) and, on the y-axis, the reduction in uncertainty in the estimate compared to analyzing the data using casewise deletion. Lower values on the y-axis indicate gains in precision due to imputation. The results show, in one case, that using three reinterview subsamples and multiply imputing the missing two-thirds of data in that wave offers a standard error for the sample mean that is nearly half as large as when using casewise deletion (and thus nearly as small as when interviewing the full sample) at the expense of a possible 10% bias in the estimated sample mean.[5] Of course, this process of "making up data" has a certain lack of intuitive appeal, but is fully supported by the design features of the RRD.

The 2014 European Parliament Elections in Denmark

We applied the RRD method to studying vote intentions in the 2014 elections for the European Parliament (EP) held in Denmark on May 25, 2014. The elections saw the far-right Danish People's Party (*Dansk Folkeparti*) win a plurality of votes (26.6% of the total) for the first time in history. The election was held concurrent with a national referendum on Denmark's participation in the European Unified Patent Court (UPC), a multinational court meant to

[4] A very simple multiple imputation procedure is used here, wherein missing values are replaced with random draws from the observed values. This provides a conservative tests of the reductions in uncertainty possible through imputation; further gains could be made using a more sophisticated imputation model (e.g., leverage Wave 1 covariates).

[5] This bias depends, of course, on either (1) no attrition in a given reinterview wave, or (2) that non-response (other than those intentionally not interviewed) is MCAR or ignorable.

standardize legal issues relating to patents issued in European countries. Turnout in the election was 56.3%. Our study involved a two-wave RRD wherein the full sample was recruited for a very early pre-election interview in the summer of 2013 and three rounds of further pre-election interviews with random subsets of this initial sample were conducted over the ensuing months.[6] The RRD approach is particularly important in this context because our interest was not only in the EP elections but also in opinion formation surrounding the lower-salience UPC referendum. We were particularly concerned about panel conditioning and interview-induced sample attrition as respondents may have been inclined to learn about the issue more than they otherwise would, respond differently than they otherwise would, or drop out of the panel entirely because the questions about the UPC were seen as too obscure (Halpern-Manners et al. 2014; Warren and Halpern-Manners 2012).

Implementation

Figure 3 displays the overall design and the timing of the three waves in which subsamples of the panel were reinterviewed. The initial wave of interviewing took place between July 10 and August 28, 2013 just before the opening of the Danish parliamentary session and a total of 6,418 respondents were interviewed online.[7] Respondents were recruited from GallupForum, a nationally representative online panel. This sample was representative of the Danish population with respect to sex, age, region, and education. Respondents were randomly assigned to three subpanels and reinterviewed in three waves, with Wave 2A occurring between

[6] There was no post-election survey.
[7] Additionally 1187 individuals invited to participate in Wave 1 refused, and Gallup further excluded 422 potential participants who were screened out prior to beginning the Wave 1 questionnaire (e.g., because they entered a sex or age that mismatched their profile data or because they were ineligible to participate) and 783 respondents who began but did not finish Wave 1.

September 26 and October 23, 2013, Wave 2B occurring between April 28 and May 11, 2014 and the final Wave 2C occurring in the final days before the election (May 12-25, 2014). This left us with four time period observations with each respondent interviewed only twice. Response rates were high across all waves. Of the initial 6418 respondents to Wave 1, 1900 were invited to complete Wave 2A, of which 1670 (or 89.0%) did so. Of the 1975 respondents invited to Wave 2B, 1569 (80.8%) responded. Of the 1971 respondents recruited for Wave 2C, 1508 (76.5%) responded. Thus, a total of 4745 respondents (73.9%) completed both waves.[8] Individuals were invited to participate in each wave via email and reminders were sent via both email and SMS to initial nonrespondents. For Wave 2C, phone calls were additionally made to encourage responding given the short field window and the proximity of the field dates to the election.

In the first panel wave, we additionally measured respondents' general orientation toward the Europe, questions about European identity, party identification, left-right self-placement, and assessments of government and economic performance. From the GallupForum profile data, we additionally obtain basic demographic measures, including sex, age, region, employment status, union membership, personal and household income, and media use measures. These measures were not repeated in the reinterview waves.

We focus here on respondents' vote intentions in the EP election and their vote intention on the UPC as well as three hypothetical referenda. Denmark is a typical multi-party parliamentary democracy with eight parliamentary parties.[9] Seven of these parties (all but the

[8] Of those not responding to Waves 2A and 2B, 163 respondents were re-invited to participate in a later reinterview wave did so but we exclude them from the analysis. Thus a total of 1510 of the original respondents did not complete any follow-up wave. Of those not responding to a reinterview wave, 572 left the GallupForum panel entirely after Wave 1, making them ineligible. As such, the cross-sectional response rate for all Wave 2 reinterview rounds combined is 81.2% once these ineligibles are excluded.

[9] MPs of national parties for Greenland and the Faroe Islands also sit in the Danish Parliament (*Folketinget*).

far-left Unity List) competed for the EP election, as did the People's Movement against the EU, an anti-EU coalition. At each wave, we asked respondents "If there was an election to the European Parliament tomorrow, which party would you vote for?" The response options included all groups competing in the election, along with "will not vote," "will vote blank," "will not answer," and "don't know." Those who said "don't know" were asked a follow-up question: "Even if you are in doubt about who you would vote for if there was an election to the European Parliament, we would still ask you if there is a party that you are leaning most toward?" with the same response options as before.

For the UPC referendum, we asked respondents "Will you vote yes or no if there is a referendum on Denmark's joining the common European patent court tomorrow?"[10] Responses were recorded on a four point scale: "will vote yes," "don't know but leaning toward voting yes," "don't know but leaning toward voting no," and "will vote note" along with a "don't know" option. On the reinterview waves, we additionally asked respondents about three hypothetical referenda on: 1) Denmark's participation in the common currency, the Euro; 2) Denmark's participation in a common EU defense policy; and 3) Denmark's participation in a common EU refugee policy. These issues were not specifically features of the election campaign or public debate at the time, thus providing placebo tests where we would expect little opinion movement over the course of campaign.[11]

Results

While response rates in the reinterview rounds decreased slightly over the course of the panel

[10] Note that "common European patent court" is a direct translation of the Danish term for the UPC (*Den fælles europæiske patetdomstol*).
[11] We additionally measured a number of other items related to the UPC, which we report elsewhere (REDACTED).

(from 89.0% at Wave 2A to 76.5% at Wave 2B), there was relatively little total attrition. Of the 1510 individuals who failed to complete a follow-up interview, fully 572 of these individuals had left the GallupForum panel entirely, making them ineligible for further participation in the study. Was this attrition consequential for the representativeness of the panel? The RRD uniquely enables us to compare the demographic characteristics of respondents at each reinterview wave to both the 572 respondents who left the panel entirely and to remaining individuals who – despite remaining in the panel – chose not to respond to their Wave 2 survey invitation.

The Wave 1 sample was broadly representative of the Danish population as a whole and each of the subpanels reinterviewed during Wave 2 retained much of this face valid representativeness. Indeed, we see almost no identifiable attrition with regard to sex, education, region, employment sector, personal or household income, ideology, or attitude toward the European Union. The only appreciable pattern of attrition is with regards to age, wherein the individuals responding to any of the Wave 2 reinterview rounds are slightly older than the panel as a whole and seven years older, on average, than individuals who fail to respond to a Wave 2 survey due to either attrition from the GallupForum panel or unit nonresponse. Looking more closely at age, we see that the individuals assigned to Wave 2A-2C who remained in the GallupForum panel were 42-43 years of age on average. Had they responded, the reinterview waves would have matched the Wave 1 age distribution. Among the 572 individuals that left the GallupForum panel entirely, their average age was 37.2 (SD=11.7), suggesting that age-related attrition was almost entirely due to younger individuals fully exiting the panel rather than unit nonresponse during the Wave 2 rounds of reinterviewing. This should perhaps be taken as a cautionary note for the implementation of campaign studies of all kinds using respondents from a broader online panel.

How useful was the RRD panel for examining vote choices over the course of the campaign? Figure 4 displays estimated support for each party at each panel wave, with 95% confidence intervals for the proportions. The official results of the parliamentary election were 26.6% for the Danish People's Party, 19.1% for the Social Democrats, 16.7% for the Liberals, 11.0% for the Socialist People's Party, 9.1% for the Conservatives, 8.1% for the People's Movement Against the EU, 6.5% for the Social Liberals, and 2.9% for the Liberal Alliance. Estimated support and the official results are also displayed in Table 2.

The RRD waves accurately capture vote shares for the various parties, including a mid-campaign shift toward increased support for the anti-EU DPP at the expense of support for the center-right Liberals. While the Wave 2C survey underestimates the vote share for the DPP by about 4 percentage points, the figure clearly displays the surge in support for the DPP over the course of the campaign. If we had only interviewed our respondents early and late in the campaign (e.g., at Round 1 and Round 2C), a traditional panel would have missed the timing of the change in support for the DPP, which came neither early nor late in the campaign but rather occurred more than a month before the election (before Wave 2B). It is also worth highlighting the statistical efficiency of the RRD for estimating these vote shares. Despite Waves 2A-2C relying on subsamples that are less than a third the size of the full Wave 1 sample, the standard errors for the Wave 2 estimates are not even twice as large as those for Wave 1 (see Table 2). The RRD – compared to a traditional panel or repeated cross-section – provides fairly precise estimates of party support at numerous points in time.

Turning now to the referenda on the ballot, the UPC referendum passed with 62.5% approval. Figure 5 displays support for the UPC and the three hypothetical European referenda. As should be clear, the campaign surrounding the UPC led to a substantial increase in the

number of respondents intending to vote "yes" on the UPC and essentially no change in intentions for the other referenda questions, which reflects the lack of debate on these topics over the course of the campaign. Indeed, an estimated 37.7% of the Wave 1 sample indicated they did not know how to vote on the UPC referendum, but this number decreased to 12.3% by Wave 2C. The Wave 2C sample slightly underestimated the "yes" vote for the UPC referendum, but if we exclude these "don't know" respondents, estimated support for the UPC is 62.6% with a 95% confidence interval ranging from 59.8% to 65.4%, neatly capturing the final election result.

To put all of these results into perspective, we construct a simple model explaining anti-EU voting during the election, as measured by support for the Danish People's Party and voting "no" in the UPC referendum as a function of EU attitude, left-right ideology, an evaluation of government performance, and various demographics.[12] We estimate these results using cross-sectional and random-effects panel logit regression and present average marginal effects (on the predicted probability scale) in Table 3. Column 1 shows average marginal effects on support for the DPP in Wave 1 and Column 2 does the same for Waves 2 (with dummies for each wave). Column 3 shows the random effects specification. All three sets of results converge on the same conclusion. With EU attitude, left-right ideology, and government performance evaluation each scaled from -1 to +1, those with strongly negative EU attitudes are far more likely to vote the DPP than those with strongly positive EU attitudes. Similarly, those on the political far-right are much more likely to vote for the DPP than those on the far left. And, those who view see the government having performed well are less likely to vote for the DPP than those view government performance more negatively. In essence, voting for the DPP reflects a mixture of

[12] The full model includes EU attitude, left-right ideology, evaluation of government performance, sex, age, education, and region as right-hand variables. EU attitude and ideology are also included as squared terms to address non-linearities, along with an interaction between EU attitude and left-right ideology.

voting against the EU, against the center-left coalition government, and in-line with one's ideology. Higher levels of education are also associated with a lower tendency vote for the DPP.

The results for the DPP correspond quite closely to the results for voting against the UPC referendum (see columns 4-6 of Table 3). Again, it is clear that those with strongly negative views toward the European Union are much more likely to vote "no" on the UPC referendum. This effect is large than voting the comparable effect on support for the DPP. Voting "no" is also a reflection of government performance evaluations. Unlike voting for the DPP, however, anti-EU voting on the UPC is essentially unaffected by left-right ideology and the effect of education is reduced. Overall, these results should not be surprising, as they are consistent with much previous research (Hobolt 2005; Hobolt 2007). Our intention with the Danish case, however, was not to identify a novel explanation for "voting against Europe" but rather to demonstrate the RRD approach is a viable method for studying campaigns and vote choices. That the results closely mirror the results of studies using more traditional repeated cross-sectional and full panel methods suggests that the RRD is a useful tool for studying campaigns and elections.

Conclusion

The rolling reinterview design provides a cost-effective and statistically efficient means of studying political campaigns and debates, or any political process playing out over time. In contract to traditional cross-sectional research designs, the RRD offers many of the advantages of panel research – especially within-subjects comparisons – while making a reasonable trade-off between statistical efficiency and the cost of interviewing a complete panel. By obtaining a large, one-time sample of the public, the RRD provides a meaningful and consistent set of baseline for all panelists from a single point in time, unlike rolling panel designs that never observe the

complete panel together. Similarly, by interviewing random subpanels, the RRD provides the same advantages as repeated cross-sectional or rolling cross-sectional designs for observing multiple points in time during a campaign, without the loss of the ability to make within-subject comparisons. The design is flexible, generalizable, and easy to administer.

Indeed, the Danish study was a relatively simple implementation of the rolling reinterview design. It involved only two panels waves, three rounds of reinterviewing, and the reinterview sampels for each round were constructed by simple random random sampling from the initial panel of respondents. This design might be generalized in a number of ways, depending on the particular context and research question, including expanding the number of subsamples or reinterview rounds, creating subsamples through sample stratification, using missing data imputation, or embedding survey-experiments in the reinterviews.

First, as already mentioned, substantial gains in the number observed points in time can be had by both increasing the number of subsamples and increasing the number of interviews per subsample. As the number of subsamples increases, however, so does sampling error, and researchers will need to weigh the benefit of additional time period observations against both the greater uncertainty in estimates and the cost savings of conducting many fewer total interviews over the course of the panel. For example, the Danish study involved three reinterview subsamples, meaning four time periods were observed with a fifty-percent reduction in total and per-respondent interviews compared to a traditional panel. Because the total sample was large (over 6000 respondents in the first wave and approximately 1500 in each subsample), there was only a minor increase in sampling error. When per-interview survey costs are high, this can mean obtaining substantially more information about a campaign at a cost savings.

Second, sample stratification might be used to construct the subsamples. Given that the

RRD involves a reduction in the precision of estimates due to the loss of sample size in each reinterview wave, stratification may make up for some of this loss (particularly if the study is concerned with one focal variable). As a general rule, a stratified sample will have equivalent or lower sampling variance than a similarly sized simple random sample. The precise reduction in uncertainty from stratification in any particular study would depend on the amount of within- and between-strata variance. A more qualitative impact of stratification would be to guarantee that particular subgroups are included in each reinterview round. And in resource terms, stratification can be useful in targeting efforts at addressing unit nonresponse to minimize total survey error.

Third, because the RRD involves random subsampling of the initial panel respondents for each reinterview subsample, much of the missing data in later panel waves is *missing completely at random* (Little & Rubin 2014). Satisfying this minimal assumption means that it is in principle quite easy to impute missing data for these observations using data from the initial panel wave and information from respondents at each time period. Figure 2 displayed a simple example of the reduction in uncertainty that could be gained from multiple imputation; further gains might be possible using a more complex imputation model leveraging Wave 1 covariates. The complication is that unit and item nonresponse among those selected for each reinterview may be non-ignorable, but such is the case in any survey with nonresponse.

Finally, it is possible to embed survey experiments in the reinterviews. As respondents complete their final (or only) reinterview, they can participate in experimental studies that will not risk downstream contamination of the panel. This is particularly useful if contextual factors may influence experimental effects (see (Gaines, Kuklinski & Quirk 2007) because the same experiment can be conducted at multiple points in time during the campaign. Indeed, in the Danish study, we embedded experiments at the end of the reinterview waves in order to see how

sensitive respondents were to partisan policy endorsements in terms of their attitudes on the Unified Patent Court issue (see REDACTED).

In conclusion, we have argued that the RRD offers a cost-effective and statistically efficient method for studying political campaigns. Relative to repeated cross-sectional surveys, the RRD provides the opportunity for the within-subjects comparisons while retaining observation of public attitudes at multiple points in time during a campaign. Compared to traditional panel, the RRD reduces the burden placed on respondents and the costs of interviewing a complete panel with only a modest penalty in terms of statistical efficiency. With an application to the 2014 European Parliamentary election in Denmark, we demonstrated the utility of the RRD for studying campaign dynamics. Considering the high cost of obtaining a large number of survey interviews, broadly held concerns about the consequences of panel conditioning, and statistical advantages of within-subject analyses, the RRD is likely to become a valuable method for political research moving forward.

References

Achen, Christopher H. 1975. "Mass Political Attitudes and the Survey Response." *American Political Science Review* 69(4):1218. http://www.jstor.org/stable/1955282?origin=crossref

Ansolabehere, Stephen, Jonathan Rodden & James M. Jr. Snyder. 2008. "The Strength of Issues: Using Multiple Measures to Gauge Preference Stability, Ideological Constraint, and Issue Voting." *American Political Science Review* 102(02):215–232. http://www.journals.cambridge.org/abstract_S0003055408080210

Bartels, Larry M. 1999. "Panel effects in the American National Election Studies." *Political Analysis* 8:1–20.

Berelson, Bernard R., Paul F. Lazarsfeld & William N. McPhee. 1954. *Voting: A Study of Opinion Formation in a Presidential Campaign*. Chicago, IL: The University of Chicago Press. http://www.amazon.com/Voting-Opinion-Formation-Presidential-Campaign/dp/0226043509

Callegaro, Mario, Reg Baker, Jelke Bethlehem, Anja S. Göritz, Jon A. Krosnick & Paul J. Lavrakas, eds. 2014. *Online Panel Research: A Data Quality Perspective*. West Sussex, UK: Wiley.

Campbell, Angus, Philip E. Converse, Warren E. Miller & Donald E. Stokes. 1960. *The American Voter*. Chicago, IL: The University of Chicago Press. http://www.amazon.com/American-Voter-Angus-Campbell/dp/0226092542

Chandler, Jesse, Pam Mueller, & Gabriel Paolacci. 2013. "Nonnaveté Among Amazon Mechanical Turk Workers: Consequences and Solutions for Behavioral Researchers." *Behavior Research Methods*.

Clinton, Joshua D. 2001. “Panel Bias from Attrition and Conditioning: A Case Study of the Knowledge Networks Panel.” Unpublished paper.

The Comparative Study of Electoral Systems. “CSES Module 4 Second Advance Release [dataset].” 20 March 2015 version. doi:10.7804/cses.module4.2015-03-20

Converse, Philip E. 1964. The Nature of Belief Systems in Mass Publics. In *Ideology and Discontent*, ed. David Apter. New York: Free Press pp. 206–261.

Converse, Philip E. 1970. Attitudes and Non-Attitudes: Continuation of a Dialogue. In *The Quantitative Analysis of Social Problems*, ed. Edward R. Tufte. Reading, Massachusetts: Addison-Wesley pp. 168—-189.

Dean, Gillian & Thomas Welling Moran. 1977. “Measuring Mass Political Attitudes: Change and Unreliability.” *Political Methdology* 4(4):383–413.

Debell, Matthew, Jon A. Krosnick, and Arthur Lupia. 2010. “Methodology Report and User’s Guide for the 2008–2009 ANES Panel Study.” Palo Alto, CA, and Ann Arbor, MI: Stanford University and the University of Michigan. Retrieved 22 January 2016 from http://www.electionstudies.org/studypages/2008_2009panel/anes2008_2009panel_MethodologyRpt.pdf.

Deng, Yiting, D. Sunshine Hillygus, Jerome P. Reiter, Yajuan Si & Siyu Zheng. 2013. “Handling Attrition in Longitudinal Studies: The Case for Refreshment Samples.” *Statistical Science* 28(2):238–256.

Erikson, Robert S. 1978. “Constituency Opinion and Congressional Behavior: A Reexamination of the Miller-Stokes Representation Data.” *American Journal of Political Science* 22(3):511–535. http://www.jstor.org/stable/2110459

Feldman, Stanley. 1989. “Measuring Issue Preferences: The Problem of Response Instability.”

Political Analysis 1(1):25–60. http://pan.oxfordjournals.org/cgi/doi/10.1093/pan/1.1.25

Frankel, Laura Lazarus & D. Sunshine Hillygus. 2014. "Looking Beyond Demographics: Panel Attrition in the ANES and GSS." *Political Analysis* 22: 336-353.

Gaines, Brian J., James H. Kuklinski & Paul J. Quirk. 2007. "The Logic of the Survey Experiment Reexamined." *Political Analysis* 15(1):1–20. http://pan.oxfordjournals.org/cgi/doi/10.1093/pan/mpl008

Hill, Jennifer L. & Hanspeter Kriesi. 2001. "Classification by Opinion-Changing Behavior: A Mixture Model Approach." *Political Analysis* 9(4):301–324. http://pan.oxfordjournals.org/cgi/doi/10.1093/oxfordjournals.pan.a004872

Hobolt, Sara Binzer. 2005. "When Europe Matters: The Impact of Political Information on Voting Behaviour in EU Referendums." *Journal of Elections, Public Opinion & Parties* 15(1):85–109. http://www.informaworld.com/openurl?genre=article&doi=10.1080/13689880500064635&magic=crossref||D404A21C5BB053405B1A640AFFD44AE3

Hobolt, Sara Binzer. 2007. "Taking Cues on Europe? Voter Competence and Party Endorsements in Referendums on European Integration." *European Journal of Political Research* 46(2):151–182. http://doi.wiley.com/10.1111/j.1475-6765.2006.00688.x

Iyengar, Shanto & Adam F. Simon. 2000. "New Perspectives and Evidence on Political Communication and Campaign Effects." *Annual Review of Psychology* 51:149–69. http://www.ncbi.nlm.nih.gov/pubmed/10751968

Iyengar, Shanto & Lynn Vavreck. 2011. Online Panels and the Future of Political Communication Research. In *Handbook of Political Communication Research*, ed. Holli A. Semetko & Margaret Scammell. New York: Sage Publications pp. 225–240.

Johnston, Richard, André Blais, Henry E Brady & Jean Crête. 1992. *Letting the People Decide: Dynamics of a Canadian Election*. Cambridge University Press.

Johnston, Richard & Henry E. Brady. 2002. "The Rolling Cross-Section Design." *Electoral Studies* 21(2):283–295. http://linkinghub.elsevier.com/retrieve/pii/S0261379401000221

Kalton, Graham & Constance F. Citro. 1995. "Panel Surveys: Adding the Fourth Dimension." *Innovation: The European Journal of Social Science Research* 8(1):25–39.

Kruse, Yelena, Mario Callegaro, J. Michael Dennis, Charles DiSogra, Stefan Subias, Michael Lawrence & Trevor Tompson. 2009. "Panel Conditioning and Attrition in the AP-Yahoo! News and Election Panel Study." Paper presented at the *AAPOR Annual Conference*.

Lazarsfeld, Paul F. 1940. "'Panel' Studies." *Public Opinion Quarterly* 4(1):122–128. http://www.jstor.org/stable/2744725

Lazarsfeld, Paul F., Bernard R. Berelson & Hazel Gaudet. 1948. *The People's Choice: How the Voter Makes up His Mind in a Presidential Campaign*. New York: Columbia University Press. http://books.google.com/books?id=S-lnIFR02FIC&pgis=1

Lazarsfeld, Paul F. & Marjorie Fiske. 1938. "The 'Panel' As a New Tool for Measuring Opinion." *Public Opinion Quarterly* 2(4):596–612. http://poq.oxfordjournals.org/cgi/content/summary/2/4/596

Little, Roderick J.A. & Donald B. Rubin. 2014. *Statistical Analysis with Missing Data*. 2nd ed. John Wiley & Sons.

Lynn, Peter. 2014. "Longer Interviews May Not Affect Subsequent Survey Participation Propensity." *Public Opinion Quarterly* 78(2):500—509. http://dx.doi.org/10.1093/poq/nfu015

Pickup, Mark & Richard Johnston. 2007. "Campaign Trial Heats as Election Forecasts: Evidence

from the 2004 and 2006 Canadian Elections." *Electoral Studies* 26: 477-91.

Schifeling, Tracy A., Christine Cheng, Jerome P.Reiter, & D. Sunshine Hillygus. 2015. "Accounting for Nonignorable Unit Nonresponse and Attrition in Panel Studies with Refreshment Samples." *Journal of Survey Statistics and Methodology* 3(3): 265-295.

Sharot, Trevor. 1991. "Attrition and Rotation in Panel Surveys , Vol. 40, No. 3, Special Issue: Survey Design, Methodology and Analysis (2) (1991), pp. 325-331 Published by: Wiley for the Royal Statistical Society Stable URL: http://www.jstor.org/stable/2348285 Accessed: 18-01-2016 10:39 UTC." *Journal of the Royal Statistical Society. Series D (The Statistician)* 40(3):325–331. http://www.jstor.org/stable/2348285

Sides, John & Lynn Vavreck. 2013. *The Gamble: Choice and Chance in the 2012 Presidential Election*. Princeton, NJ: Princeton University Press.

Sturgis, Patrick, Nick Allum & Ian Brunton-Smith. 2009. Attitudes Over Time: The Psychology of Panel Conditioning. In *Methodology of Longitudinal Surveys*. John Wiley & Sons chapter 7, pp. 113–126. http://privatewww.essex.ac.uk/ nallum/SturgisAllumandBrunton-Smith2009.pdf

Vavreck, Lynn & Shanto Iyengar. 2011. The Future of Political Communication Research: Online Panels and Experimentation. In *Oxford Handbook of Public Opinion and Media Research*, ed. Robert Y. Shapiro & Lawrence R. Jacobs. New York: Oxford University Press.

Warren, John Robert & Andrew Halpern-Manners. 2012. "Panel Conditioning in Longitudinal Social Science Surveys." *Sociological Research & Methods* 41(4):491–534. http://smr.sagepub.com/content/41/4/491.full.pdf

Wiley, David E. & James A. Wiley. 1970. "The Estimation of Measurement Error in Panel

Data." *American Sociological Review* 35(1):112–117.

Table 1. Demographics by Panel Wave

	Wave 1	Wave 2A	Wave 2B	Wave 2C	Wave 2 Non-Respondents	*Danish Population*
Female	.50 (.50)	.50 (.50)	.49 (.50)	.49 (.50)	.50 (.50)	*.50*
Age (mean)	45.6 (14.2)	47.2 (14.0)	47.2 (13.9)	47.3 (14.2)	40.6 (13.3)	*40.4*
Region						
Copenhagen	30.1	30.4	30.5	29.3	30.0	*31.1*
Zealand	15.1	14.7	14.9	14.7	15.9	*14.5*
S. Denmark	21.6	22.1	21.4	22.4	20.4	*21.4*
Mid-Jutland	23.0	22.6	23.3	23.3	23.4	*22.7*
North Jutland	10.2	10.3	10.0	10.4	10.3	*10.3*
Employment Sector						
Private	38.2	34.9	39.1	37.7	41.1	*46.9*
Public	24.4	27.2	24.2	22.7	23.3	*22.8*
Income (,000 DKK)						
Personal	303 (168)	304 (164)	315 (170)	300 (166)	292 (169)	*295*
Household	521 (305)	518 (301)	536 (308)	517 (303)	507 (302)	*474*
Education (mean)	4.4 (1.5)	4.4 (1.5)	4.4 (1.5)	4.3 (1.6)	4.6 (1.4)	--
Left-Right Ideology (mean)	.18 (.46)	.16 (.46)	.19 (.47)	.17 (.46)	.17 (.46)	--
EU Attitude (mean)	.04 (.52)	.03 (.53)	.02 (.54)	.03 (.53)	.07 (.50)	--

Note: Cell entries are proportions, unless otherwise noted, with standard deviations in parentheses. Education is measured on a 1-6 scale. Danish population numbers were retrieved from Statistics Denmark's StatBank for the year 2014. Population income numbers are based on total pre-tax income and employment sector is based on working-age population (19-69 years of age).

Table 2. Estimated Party Support (%), by Panel Wave

	Wave 1	Wave 2A	Wave 2B	Wave 2C	Election Results
Danish People's Party	16.6 (0.53)	19.0 (1.10)	24.3 (1.20)	22.4 (1.15)	26.6
Social Democrats	24.0 (0.60)	25.4 (1.22)	21.9 (1.16)	22.8 (1.16)	19.1
Liberals	25.1 (0.61)	22.6 (1.18)	21.0 (1.14)	17.7 (1.05)	16.7
Socialist People's Party	9.5 (0.42)	9.0 (0.80)	7.2 (0.73)	9.5 (0.81)	11.0
Conservatives	4.7 (0.30)	4.7 (0.59)	5.7 (0.65)	7.1 (0.70)	9.1
People's Movement Against the EU	7.7 (0.38)	7.6 (0.75)	10.7 (0.87)	9.7 (0.82)	8.1
Social Liberals	8.3 (0.39)	7.8 (0.75)	5.7 (0.64)	7.3 (0.72)	6.5
Liberal Alliance	3.4 (0.26)	3.0 (0.48)	2.8 (0.46)	2.9 (0.46)	2.9

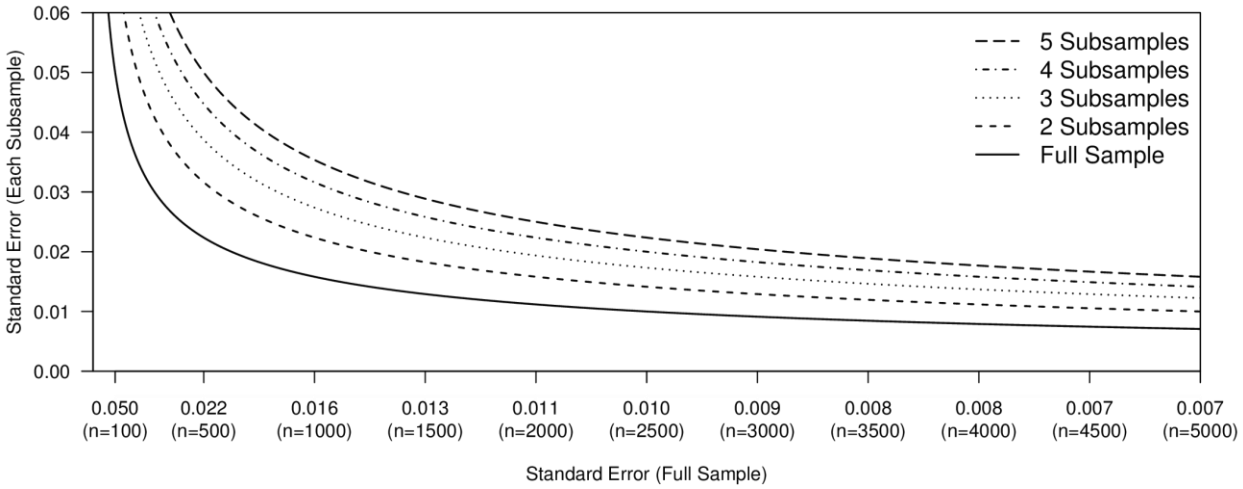
Note: Cell entries are percentages (with standard errors in parentheses) of those expressing a preference among the parties. The percentage of respondents expressing no preference decreased from 22.6% in Wave 1 to 12.8% in Wave 2C.

Table 3. Effects on Support for the DPP and Voting Against the UPC Referendum

	(1) Vote DPP	(2) Vote DPP	(3) Vote DPP	(4) Vote No on UPC	(5) Vote No on UPC	(6) Vote No on UPC
	Wave 1	*Wave 2*	*Random Effects*	*Wave 1*	*Wave 2*	*Random Effects*
Pro EU	-0.17*	-0.18*	-0.14*	-0.29*	-0.32*	-0.31*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Left-Right	0.08*	0.10*	0.10*	-0.01	-0.04*	-0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Gov't Performance	-0.13*	-0.17*	-0.12*	-0.05*	-0.08*	-0.06*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Female	-0.01	-0.01	-0.01	-0.01	0.02*	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age	0.00*	0.00*	0.00*	0.00	-0.00*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Education	-0.02*	-0.01*	-0.02*	-0.01*	-0.01	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Region Zealand	-0.00	-0.01	-0.00	-0.00	-0.01	-0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Region Southern Denmark	0.00	-0.01	0.00	0.03*	-0.00	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Region Mid-Jutland	-0.01	0.00	-0.00	0.02	0.01	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
Region North Jutland	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Wave 2A	--	--	0.00	--	--	0.01
			(0.01)			(0.01)
Wave 2B	--	0.02	0.04*	--	0.04*	0.05*
		(0.01)	(0.01)		(0.01)	(0.01)
Wave 2C	--	0.04*	0.05*	--	0.09*	0.10*
		(0.01)	(0.01)		(0.01)	(0.01)
N	4643	3586	8229	5711	4149	9860

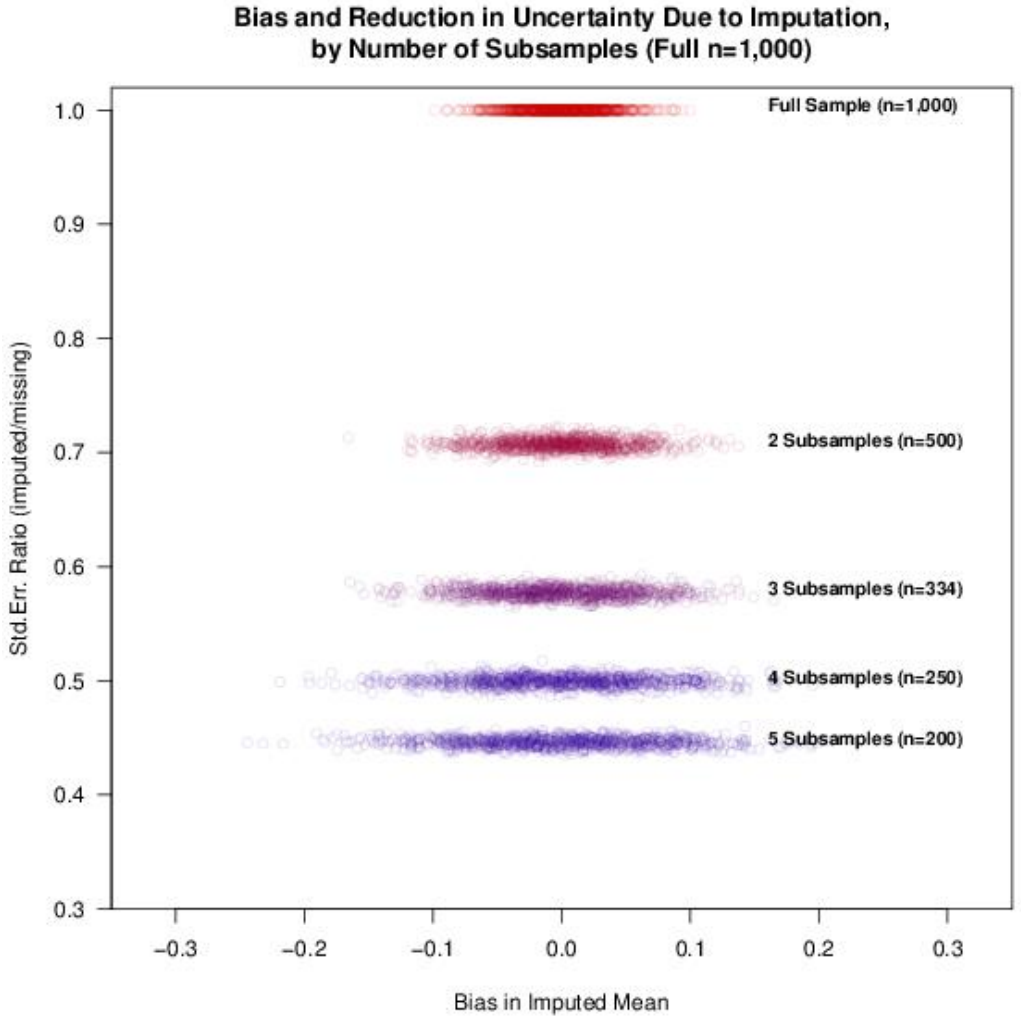
Note: Cell entries are average marginal effects (with associated standard errors in parentheses) estimated from cross-sectional logit regression estimates (Columns 1,2,4,5) and random-effects panel logit regression estimates (Columns 3 and 6).

Figure 1. Standard Errors for Estimated Proportion from Rolling Reinterview Design



Note: Figure displays the standard error of an estimated proportion, *p*, for a simple random sample of a population, assuming a maximum element variance (*p=0.50*) for a full sample of size *n* and for various numbers of random reinterview subsamples thereof.

Figure 2. Bias and Reduction in Uncertainty Due to Imputation of MCAR Responses for Non-Subsampled Respondents



Note: Figure displays the results of a monte carlo simulation demonstrating a bias-variance trade-off related to imputation of missing completely at random data in the RRD for a full sample (n=1,000; no missingness) and for various numbers of random reinterview subsamples thereof. The y-axis displays the ratio of standard errors of an estimated sample mean from a multiply imputed subsample compared to the estimate drawn from that subsample using casewise deletion. Lower values indicate reductions in uncertainty compared to using casewise deletion. On the x-axis is the bias in the estimated sample mean (compared to the population mean) from the imputed subsample.

Figure 3. Rolling Reinterview Design for the 2014 Danish Study

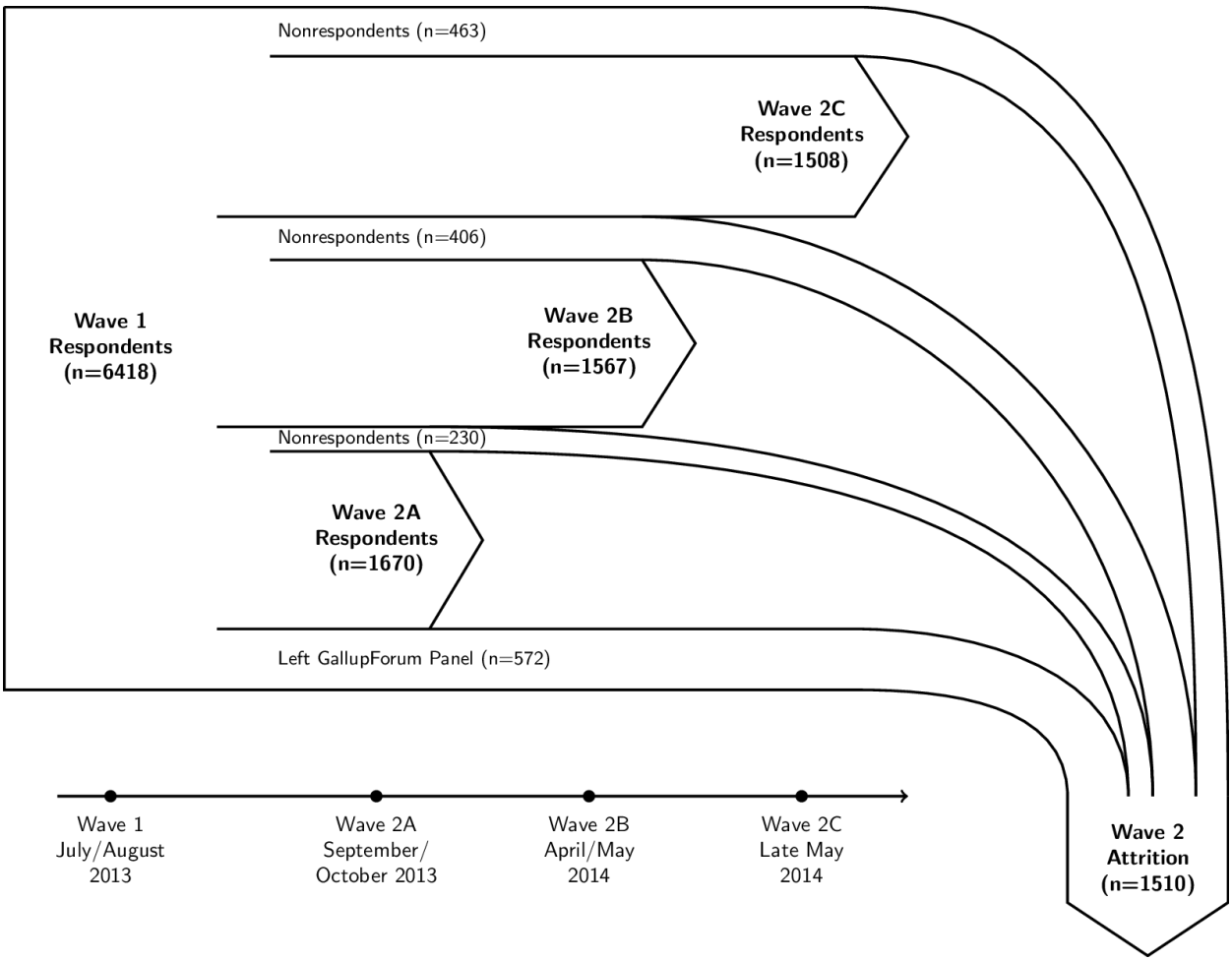
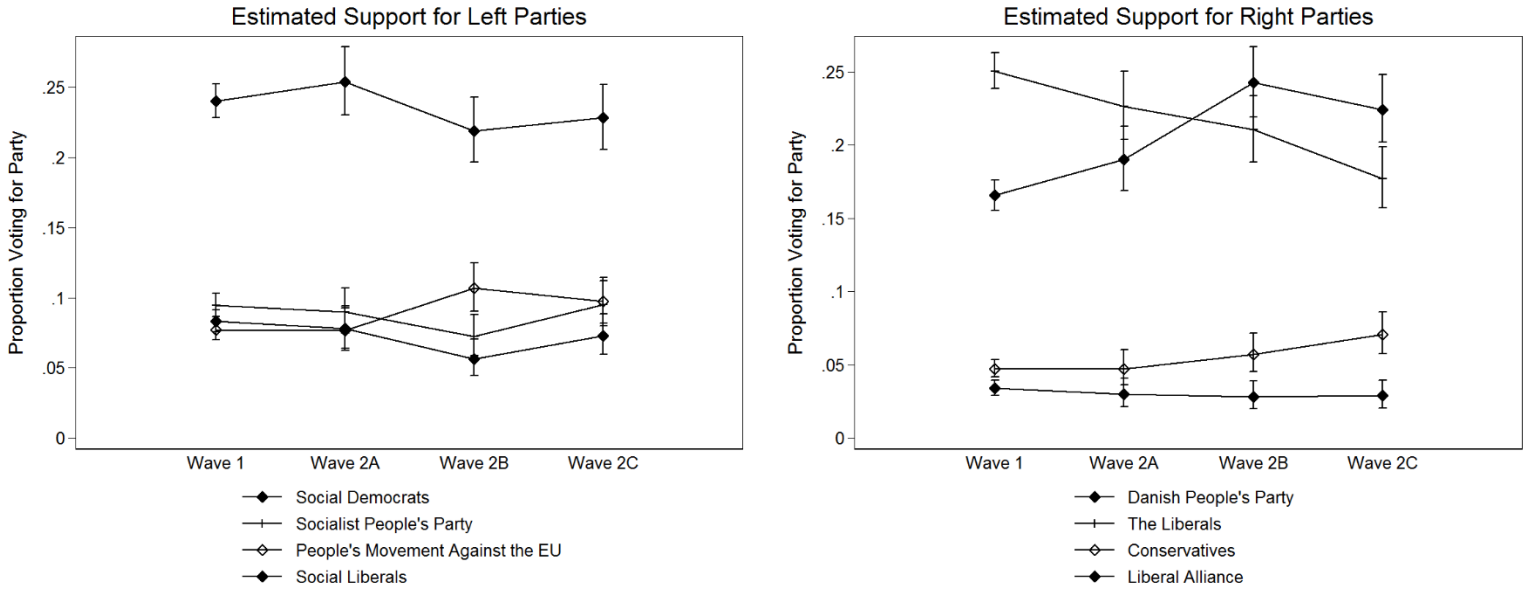
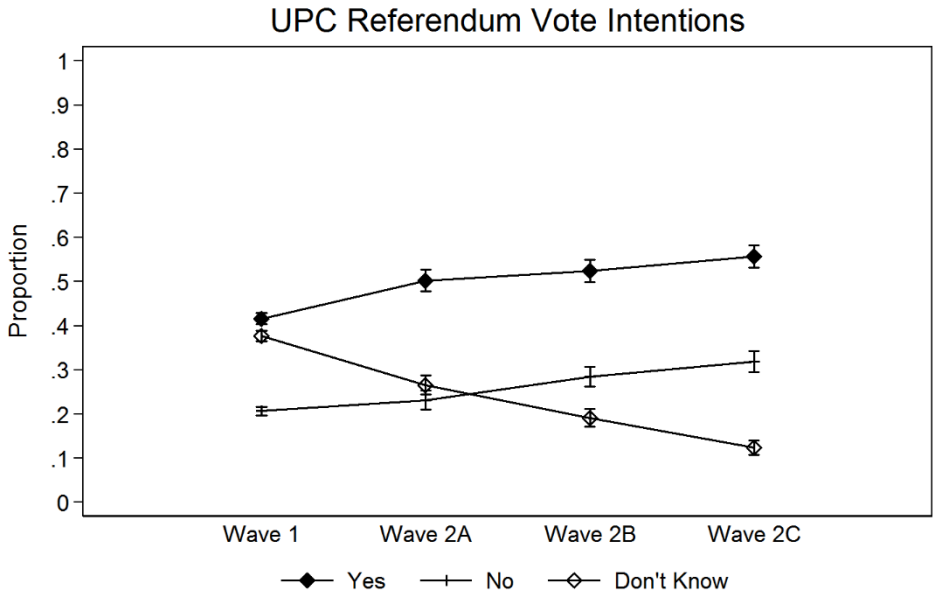


Figure 4. Estimated Party Vote Shares, by Panel Wave

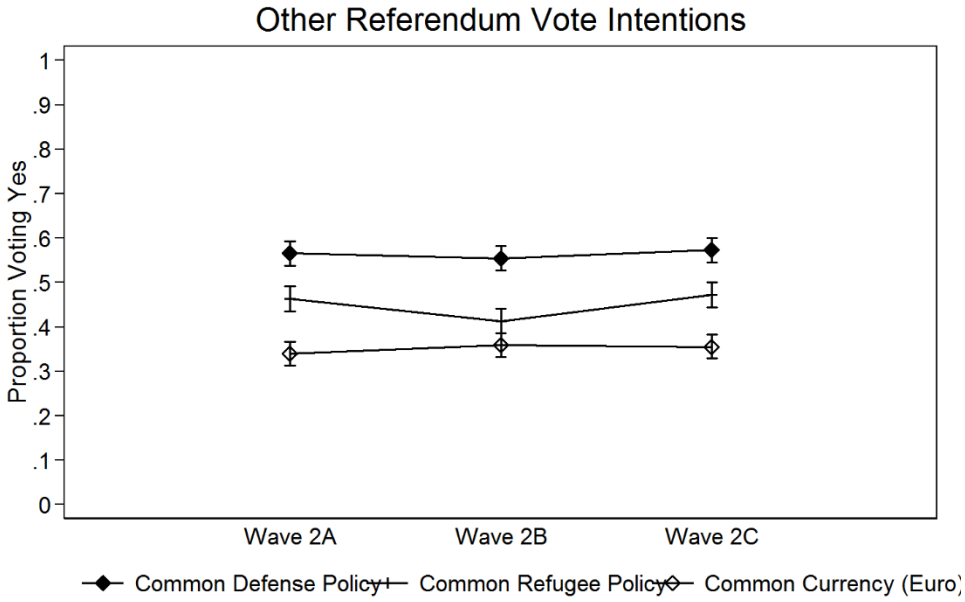


Note: Figure displays estimated proportions voting for each party, with bars representing 95% confidence intervals.

Figure 5. Estimated Vote Intentions in the UPC and Hypothetical Referendums



(a)



(b)

Note: Figure displays estimated proportions, with bars representing 95% confidence intervals.