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Abstract

Motivated by recent interest and initiatives taken by several governments and international organizations to come up with indicators of well-being to inform policy makers, we test if subjective well-being measures (SWB) can be employed to study voting behaviour. Controlling for financial and economic circumstances, we find that when citizens are more satisfied with their life, they are also more likely to cast their vote in favor of the ruling party. We address the possible concern of reverse causality in the relationship between SWB and political support by (i) analysing the political behaviour of a sample of ideologically neutral voters, and (ii) by identifying the effect of SWB on voting intentions in individuals' response to an exogenous shock of (un)happiness (i.e. the death of husband or wife). We conclude that SWB explains voting decisions, even when the event affecting well-being is beyond government's control.

JEL-Code: H100, D600, D000, D100.

Keywords: subjective well-being, happiness, retrospective voting.

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1 Introduction

There is a wide consensus in economics and political science that past outcomes affect current voting decisions. In particular, according to the retrospective voting literature (e.g., Kramer, 1971; Fiorina, 1978, 1981; Kinder and Kiewiet, 1981; Markus, 1988; Lewis-Beck, 1988) voters compare past levels of utility and evaluate diagnostic information, such as macroeconomic trends and personal financial circumstances, to finally re-elect good incumbents and punish those who are believed to be corrupt, incompetent, or ineffective. At the same time the political business cycle literature (e.g. Frey and Lau, 1968; Nordhaus, 1975) has shown that policy makers, aware of this phenomenon, aim to stay in power by maximizing voters' utility before each election. The common denominator of most of the empirical studies in these literatures is the use of financial and economic indicators as proxy for voters' utility.

More recently, the idea that policy makers should consider not only monetary and financial indicators, but also rely on more comprehensive measures of well-being has become highly debated among western policy makers and scholars. Steps in this direction have been taken by the British and French governments as well as international organizations such as the World Bank, the European Commission, the United Nations, and the OECD.[1]

The first aim of this paper is to investigate if subjective well-being (SWB) measures can be used to proxy for utility in addition to financial and economic indicators to infer voters' behavior. In this respect, there is growing consensus that indices of SWB constitute a reasonably good proxy for utility.[2] For example Rabin (1998) makes explicitly the connection between happiness and experienced utility; Benjamin et al. (2012) use laboratory experiments to demonstrate that SWB is a good approximation for the modern concept of utility by showing that 80% of the time individuals choose the alternatives that maximize their SWB.

In particular, we add indicators of well-being as additional explanatory variables in standard models of retrospective voting to proxy for utility and explain individuals' voting decision, in addition to the traditionally used measures of financial and economic conditions. We construct measures of voting intentions and SWB using the British Household Panel Survey (BHPS), a rich database started in 1991 containing information on over 10,000 British individuals on a yearly basis. Consistently with the retrospective voting

[1]For example, in 2008, the French government set up a Commission led by Joseph Stiglitz for the measurement of economic performance and social progress. The aim of the commission was to make proposals about incorporating the new indicators of economic outputs in national accounts. In the UK, following the initiative taken by the current Prime Minister, David Cameron, the Office for National Statistics initiated the National Wellbeing Project, culminating with the construction of a "happiness index."

[2]These indices can be understood as an application of experienced utility that, as discussed in Kahneman and Thaler (1991), is the pleasure derived from consumption.

hypothesis, we find that that SWB affects the probability of supporting the party of the Prime Minister together with and independently from a variable reporting the perceived improvement or worsening in family finances. Our estimates suggest that the probability of supporting the incumbent is around 1.2% higher (lower) for those individuals whose financial situation has improved (worsened) in the last year while individuals who are satisfied with their life are 1.6% more likely to support the incumbent.

An obvious source of concern in exploring the relationship between voting and well-being is reverse causality: those citizens, whose favorite party is in power, might become happier just because of this political success, and not as a consequence of good policies being implemented, as Di Tella and MacCulloch (2005) have shown. We address this concern in two different ways: (i) by analyzing the responses of a sub-sample of ideologically neutral individuals (i.e. who do not have *a priori* party bias), whose well-being should not be affected by the identity of the ruling party *per se*; (ii) by identifying the effect of SWB on voting intentions analyzing individuals' response to an exogenous shock of (un)happiness. We consider them in turn.

Reverse causality between SWB and voting intentions can occur because some voters may have ideological preferences for one party. Our idea is to replicate our estimations only for a subsample of respondents who are ideologically neutral (following the literature we call them *swing* voters henceforth). Selected questions asked in the BHPS allow us to identify these individuals: our swing voters subsample covers about 30% of the full sample. SWB measures remain very significant for this second set of estimations, but their magnitude is much larger: swing voters who are satisfied with their life are 2.4% more likely to support the incumbent. Furthermore, for the full sample, an increase of 1 unit in the reported life satisfaction raises the probability of supporting the incumbent by 0.013 standard deviations, while for the swing voter subsample this increment is nearly double. Interestingly financial situation measures become not significant.

We also carry out additional tests to compare the explanatory power of financial situation and SWB measures and their correlation. Our findings suggest that they both contributes to explain voters behavior and both should be included as regressors in the final econometric model. However SWB measures appear to be more robust.

The second way we address the concern of reverse causality is by analyzing variation in respondents' voting intentions due a shock of SWB. We exploit the fact that during the period covered by the BHPS some respondents have become widows. We take the spouse's death as an exogenous variation of SWB and we show that this variation has a negative effect on voting intentions. As widely recognized by the existing literature, widowhood has a large and temporary negative effect on well-being. We use difference-in-differences (DiD) analysis and propensity score matching to identify this effect. That is, we take those respondents in the BHPS whose spouse died during the period available in our dataset; this constitutes our treated group. We then select a matched sample of individuals who never lost their spouse, but who had the same *ex ante* probability of experiencing this shock. Last, we compare *before*- and *after*-the-shock changes in political support responses of affected individuals to changes in political support responses of unaffected individuals.[3] We find that subjects in the treated group are about 8% less likely to be pro-incumbent than individuals in the control group, in the following two years after the death of their spouses. A validation test for our DiD approach is provided by the estimation of a recursive bivariate probit model on the probability of incumbent support as a function of well-being, where widowhood is used as an instrument for well-being. We find that the shock on SWB instrumented in this way has a significant positive effect on voting intentions.

The above set up not only provides a way of testing for reverse causality in the relationship between voting and SWB but also allows us to address another important question still open in the literature: "Are voters able to make policy makers accountable only for increased well-being that is the direct effect of government policies?" In other words, are individuals rewarding policy makers only for the increase in SWB they are directly responsible for, or are they also responding to events independent from government actions. We assume that becoming a widow is an event largely beyond government control. Our conjecture is that if voters were able to separate the sources of their well-being, we should not observe any variation in government support after this type of event, especially after controlling for related financial aspects. Our results suggest that voters are not able to do so because they drastically reduce their support for the government after the spouses's death. Gurdal, Miller, and Rustichini (2013) suggest a rational explanation for this mechanism; they argue that blaming others for events they are not responsible for is efficient because it induces the appropriate incentive for an agent (in our case, the politician), when effort is not observable.

There is a related literature consistent with our conclusions. Achen and Bartels (2004) show that voters are more likely to oust incumbents for the economic consequences of natural disasters. Healy, Malhotra, and Hyunjung Mo (2010) explore the electoral impact of local college football games just before an election and find that a win in the ten days before Election Day causes the incumbent to receive an additional 1.6 percentage points. In the same vein, Wolfers (2009) measures the extent to which voters in state gubernatorial elections irrationally attribute credit to the state governor for economic fluctuations unrelated to their actions. However, this literature does not analyse the role of SWB in mediating the voting choice.

To the best of our knowledge, we are the first to analyze the effect of SWB on incumbent's support. Several contributions have analyzed the effect of SWB on political

[3]We use *experiencing widowhood* as a shock that has a strong and significant impact on well-being and that is arguably independent from government actions. Widowhood (and widowerhood) is largely beyond individuals' or government control and is well known to have a deep impact on SWB (Clark and Oswald, 2002; Clark et al., 2006).

participation rather than voting decision (e.g., Dolan, Metcalfe, and Powdthavee, 2008; Killian, Schoen, and Dusso, 2008; Weitz-Shapiro and Winters, 2010; Flavin and Keane, 2012; Pacheco and Lange, 2010). These contributions indicate a positive link especially going from SWB to participation.

A related literature looks at the relationship between partisanship and well-being; notably, Di Tella and MacCulloch (2005) show that left-wing voters' well-being is positively affected by left-wing party victory and *left-wing* policy outcomes (like unemployment), and the right-wing voters' well-being, by right-wing electoral victories and *right-wing* policy outcomes (inflation targeting). Powdthavee and Oswald (2010, 2013) and Giuliano and Spilimbergo (2013) show that exogenous shocks affect individuals' political stances. Following these contributions, we test the hypothesis that the effect of SWB generated by a spouse's death on voting is different when the incumbent is left- or right-wing. We do not find any significant difference.

The remainder of the paper is organized as follows. Section 2 presents and discusses the data; Section 3 is devoted to the estimation the political support model; Section 4 presents the analysis of the effect of widowhood on voting intention. In Section 5, we estimate a recursive model where the equation determining how the shock affects the SWB and how the SWB affects the voting intension are estimated together. Section 5 concludes the paper.

2 The Data

The empirical work is based on data from the 18 existing waves of the BHPS, spanning the period 1991–2008. The BHPS is a rich database collecting information on over 10,000 British residents on a yearly basis. It contains, beside well-being questions, information on political orientation and participation, voting behavior and intentions, as well as personal information on finances, jobs, family status, and region of residence.

Note that the same individuals are interviewed every year and our main variable of interest, a measure of voting intention, is asked every year: this allows us to exploit the properties of a panel. We construct this measure by aggregating the responses from two questions available in the BHPS. First, if respondents declare not to be close to or support any political parties, they are asked "If there were to be a General Election tomorrow, which political party do you think you would be most likely to support?" Second, if respondents declare to have some political bias, they are asked to express their party preference. By merging these two pieces of information together, we construct the variable *SupportInc* (support incumbent). The variable takes a value equal to 1 if the named party is the same as the national government party (i.e., Conservative Party in the period 1991–1997, and the Labour Party from 1997 onwards) and zero otherwise.

Moreover, the fact that questions on party support and closeness are asked allows us to identify two groups of citizens: following the literature we define *swing* those respondents who are not close to any particular party (and therefore, they are likely to swing their vote from one party to the other), and *partisans* those respondents who have strong *ex ante* political preferences towards one party. The identification of these two groups will be discussed in detail in Section 3.2 and will be important for the analysis developed later in the paper.

Our key explanatory variable to analyze voting intentions is SWB. We use different proxies for it. We derive the main measures of well-being from the responses to the question *"How dissatisfied or satisfied are you with your life overall?"* This question is asked to all respondents every year in the BHPS starting from 1996 (with the exclusion of 1997). Respondents have seven possible categories from among which to choose, these go from 1 to 7, where #1 is "not satisfied at all", #4 "not satis/dissat", #7 "completely satisfied".

Figure 1 shows the distribution of life satisfaction across British individuals interviewed between 1996 and 2008. The unconditional mean for life satisfaction reported over these years is 5.2, with a median of 5. Table 1 shows the mean of life satisfaction during the different legislatures covered by the period 1996–2008, conditional on the respondents' political ideology (they have been classified according to their answer to the above mentioned questions on political partisanship).

These statistics lead to some preliminary observations: nonpartisan voters report, on average, a lower life satisfaction than partisan voters (independent of their political orientation), and Labour partisan voters report, on average, a lower life satisfaction than Conservative partisan voters. Both observations suggest there could be reverse causality between political ideology and life satisfaction, which provides valid support to our strategy of conducting the baseline analysis on the split sample of swing voters only.

As mentioned earlier, the literature on retrospective voting has recognized the importance of monetary and financial indicators in determining voting choices. Following Fiorina (1979) and many others, we use a subjective indicator to account for these monetary and financial factors, which we derive from the responses to the question *"How is your financial situation compared to last year?"* There are three possible answers respondents can choose from: the financial situation is *better*, *same as*, and *worse* compared to last year. Taking these answers, we construct the dichotomous variables $BetterFin$ and $WorseFin$, taking values of one if when respondents believe that their financial situation is respectively better and worse than last year and zero otherwise.

We also compute respondents' family income in logarithmic term[4] to account for an *objective* measure of financial situation and we include this measure in all our estimations. Finally, we include a set of controls that are usually employed in the literature of well-

[4]We follow the standard procedure of dividing the family income by the number of family members squared.

being and voting behavior: age of respondents (linear and squared), sex, marital status and income. Summary statistics for these controls are displayed in table 2.

3 The Models

The empirical strategy is based on testing the main assumptions of retrospective voting models using well-being measures rather than monetary and financial ones. This class of models assumes that voting decisions are based on utility comparison between different periods. Previous research testing retrospective voting models has used exclusively monetary and financial indicators to proxy for utility.

Our hypothesis is that well-being indicators constitute a more comprehensive (and possibly better) proxy for utility, which takes into account all those factors that are not measurable in monetary terms. There is growing consensus that indexes of SWB constitute a reasonably good proxy for utility, (e.g., Kahneman and Thaler, 1991; Benjamin et al., 2012). So our first goal is to test the validity of retrospective voting models, replacing/adding to financial and monetary indicators our life satisfaction measures to proxy for utility.

We proceed as follows. We first start by replicating the main estimations employed in previous research, to investigate whether voting decisions depend on evaluation of financial situation. In particular, following Fiorina (1979), which uses subjective questionnaire responses to show that voters are more (less) likely to cast their votes for the incumbent if they believe that their financial situation has improved (got worse) compared to the past, we first estimate our *traditional* model (Model 1):

$$SupportInc_{it} = \beta_1 BetterFin_{it} + \beta_2 WorseFin_{it} + \gamma X_{it} + \eta_t + a_i + \varepsilon_{it} > 0, \qquad (1)$$

where $SupportInc_{it}$ report the voting intention described in the previous section; $BetterFin_{it}$ and $WorseFin_{it}$ are two dummy variables taking values of 1 if the respondent has replied that her financial situation is respectively better or worse than in the past, aiming to capture variations in utility due to monetary/financial components; X_{it} is a vector of individuals' personal characteristics (age, sex, income, marital status, region of residence), note that family income is included to account for an objective measure of family finances ; η_t denotes year effects; a_i is an individual effect (either random or fixed); and ε_{it} is the error term. The coefficients of interests are β_1 and β_2. Trivially, β_1 is expected to be positive, and β_2, negative.

Next, we replace $BetterFin_{it}$ and $WorseFin_{it}$ with our well-being measures to account for the nonfinancial component of individuals' utility. So we estimate the *well-being* model (Model 2):

$$SupportInc_{it} = \delta Wellbeing_{it} + \gamma' X_{it} + \eta_t + a_i + \varepsilon_{it} > 0, \qquad (2)$$

where $WellBeing$ is constructed from respondents' answers on life satisfaction. The coefficient of interest is now δ, which is expected to be positive. Finally, we combine equations (1) and (2) to estimate a *full* model (Model 3) where both well-being and financial indicators are included as regressors:

$$SupportInc_{it} = \delta' Wellbeing_{it} + \beta_1' BetterFin_{it} + \beta_2' WorseFin_{it} + \gamma'' X_{it} + \eta_t + a_i + \varepsilon_{it}. \quad (3)$$

We start off by estimating equations (1), (2), and (3) as a linear probability model (LPM) with fixed effects (FE), to control for the within-variation effect of life satisfaction on voting behavior. However, since $SupportInc_{it}$ is a dichotomous variable, we also propose an alternative specification where we employ a random effect (RE) probit model for the conditional distribution of the probability that the respondent supports the incumbent party. To allow for correlation between the model's covariates and the unobserved heterogeneity, a_i, we follow Chamberlain (1980) and assume the latter follows a normal distribution with linear expectation and constant variance. So we augment our model with a series of individual specific observable characteristics.[5]

3.1 Baseline results

Results are displayed in tables 3 and 4. Both tables have the same format. In the first one, we present our results for the FE-LPM, and in the second one, those for the RE probit where, the average partial effect (APE) of the SWB variables are reported at the bottom of each regression. In the first column of both tables 3 and 4, we report the estimated coefficients for Model (1), the traditional retrospective voting model. In columns 2 and 3, we display the results for Model (2), the well-being model. The different columns use two variations of $Wellbeing_{it}$. First, we construct a dummy variable taking the value 1 if the respondent has chosen the answer #5, #6, or #7 to the question on life satisfaction and zero otherwise; this indicates that the respondent is satisfied with life. Second, we treat the answers (from #1 to #7) to the question on life satisfaction as a cardinal variable. Finally, in the last two columns, we propose the results of the *full model*, where both well-being measures and financial indicators are included, as in equation (3). All the regressions include the same set of controls, that is, marital status, sex, age, and age squared, along with the logarithm of family income, a set of region of residence dummies, and a set of wave-dummies. Standard errors are clustered at the individual level.

There are 4,882 individuals who were interviewed for the entire period and for which we have information on well-being and voting intentions. The dataset comprises nearly 50,000 observations.

[5]The vector of individual characteristics includes information such as whether the respondent regularly reads newspapers, whether she ever smoked over the years, whether her partner has ever been out of employment, and what is the average income of her household. By adding these variables, Chamberlain's RE probit essentially estimates the effect of varying the model's covariates while holding these individual's specific characteristics fixed.

Starting from the results on the *traditional model*, both the LPM (table 3) and probit model (table 4) estimates are in line with the basic hypothesis on the retrospective voting model, according to which one's financial situation matters for voting decisions. All the relevant coefficients are highly significant, at least at the 5% level. In particular, respondents who believe that their financial situation has improved compared to the previous year are more likely to support the incumbent compared to those whose financial situation has not changed; the estimated coefficients suggest that, approximately, the effect is a 1.3% increase in the likelihood of supporting the incumbent. Respondents who are instead worse off compared to the previous year appear to punish the incumbent by reducing the likelihood of granting their support by approximately 1.3%.

Moving to the *well-being model*, where measures of subjective financial performances are substituted with life satisfaction indicators, we can see that all the estimated coefficients of interest are highly significant in all our specifications, using both variations of well-being measures. The magnitude of the response is similar to those recorded for the previous model; if a respondent is satisfied with life, she will be about 1.8% more likely to support the incumbent than if not. Similarly, using life satisfaction as a cardinal variable, an increase of 1 percentage point in life satisfaction is associated with an increase of about three quarters of a percentage point in the likelihood of being pro-incumbent. Remarkably, the coefficients related to the well-being variables for the in table 3 using an OLS estimator are very similar to the average partial effect (APE) reported in the bottom line of table 4, using a probit estimator.

In the final model, we include both indicators of well-being and of subjective financial position. We find that both indicators retain the same sign and magnitude as in the previous set of regressions and they do not lose significance, which indicates that the two measures do capture different channels of support for the incumbent.

It is also interesting to compare the relative importance of financial situation measures with SWB ones. For the LPM displayed in Table 3 we compute y-standardised coefficients as proposed by Winship and Mare (1984) and Long (1997) and we can see that the probability of supporting the incumbent is 0.025 standard deviations higher for those whose financial situation has improved, and 0.24 lower for those whose financial situation has worsen off compared to those whose financial situation has not changed. For SWB instead we see that an increase of 1 unit in the reported SWB (measured on a 1-7 scale) raises the probability of supporting the incumbent by 0.13 standard deviations.

In summary, our results support the idea that citizens' well-being matters for voting decisions, and in particular, our findings suggest that measuring utility in terms of only monetary and financial indicators leaves out an important component, which has a significant impact on voting decisions.

3.2 Reverse causality? Tests on swing voters sample

In the voting literature, ideological preferences towards one party are generally assumed exogenosly distributed within the population. Hence, some citizens are assumed to have strong partisan preferences (either towards the incumbent or the challenger) while others are more ideologically neutral. In this setting, voting decisions become the outcomes stemming from two different components, the "ideological" one coming from party bias and the "policy" one coming from government's choices. Partisan citizens will cast their vote on both grounds (ideological and policy related), and the weights on each component will depend on the intensity of their party bias. Ideologically neutral voters instead will swing their vote exclusively in response to government policies.

As we said above, partisan voters may be more satisfied with their life because their party is ruling the government. This reverse causality represents a bias for the estimation of our model; our strategy to reduce this bias is to classify voters according to their political alignment and restrict the analysis to the voting behavior of the ideologically more neutral group of swing voters. Since this type of respondents have no (or very low) *ex ante* party preferences, they choose whom to vote for mainly on the basis of government's policies.

Two questions asked in the BHPS allow us to split the sample between partisan voters and ideologically neutral voters. The survey questions used to this purpose are (i) *"Do you support any political party"*? and (ii) *"Are you close to any political party?"* If respondents answer *"No"* to both, we classify their position for that year to be one of a nonpartisan voter. Almost 80% of individuals declared to be a nonpartisan at least once in the entire period. Among this group, we define the *swing* voters those individuals who gave such answers more than the half of median time during the whole survey, which corresponds to eight times.[6] This subsample is made out of 1,520 respondents, about 30% of the full sample. We employ it to reestimate equations (1), (2), and (3). The results are reported in tables 5 and 6, which have the same format as, respectively, tables 3 and 4. The same set of controls are used and standard errors are clustered at the individuals' level.

The results confirm our hypothesis. First, the coefficients on well-being measures in tables 5 and 6 are still very significant and, generally, higher in magnitude than those presented in tables 3 and 4; for example, looking at our preferred estimation, the RE probit in column [4] of table 6, the average partial effect for $Wellbeing$ is now 0.0231 compared with 0.0156 in the corresponding column of table 4.[7] Second, the positive effect of *improved financial situation* and the negative effect of *worse financial situation* become non significant in all specifications.

[6] We have experimented with several other possible definitions of swing voters with similar results. Output from these estimations is available upon request.

[7] Equivalently, looking at the y-standardised coefficients for the LPM in 3 and 5, in the full sample an increase of 1 unit in the level of reported life satisfaction raises the probability to support the incumbent by 0.013 standard deviations, for the swing voters sample this goes up to 0.022 standard deviations.

Finally, note that in table A.1 of the appendix, as a robustness check, we report the results for the estimation of Models (1), (2), and (3) for each level of life satisfaction. We observe a pattern consistent with a positive relationship between the probability of supporting the incumbent and the level of reported life satisfaction.

Overall we can say that, when taking out the ideological component from voting intentions, using well-being measures generates more consistent and significant results. We interpret this as a preliminary evidence that using well-being indicators to proxy for utility is more robust than using only monetary of financial proxies. We investigate their relationship further in the next section.

3.2.1 SWB vs financial position indicators

In the previous section we have shown that standard retrospective voting models have left out an important component (SWB indicators) in explaining voters' behavior, in this section we show how their inclusion affects previous results in the literature.

From the comparison of the coefficients on *financial situation (better and worse)* in column 1 with the correspondent ones in columns 4 and 5 for the LPM in Tables 3 and 5, we can observe that the inclusion of SWB does not affect the estimation of the coefficients on financial situation very much. This indicates that the correlation between well-being measures and financial situation dummies is not high; so, in principle, both measures should be included as regressors because they explain different components in voting behavior.

For the RE-Probit models displayed in Tables 4 and 6 a similar direct comparison of the coefficients is not possible, because the change in the coefficients on the financial situation dummies from column 1 to columns 4 and 5 cannot be directly attributed to the inclusion of the SWB indicators (the confounding variable), due to rescaling.[8] Wooldrige (2002) and Cramer (2007) show that average partial effects (APE) derived from probit models are unaffected by rescaling only if financial situation and SWB indicators are uncorrelated. But, if this is not the case the APEs are biased. Karlos, Holm and Breen (2011) propose a method to decompose the change in probit coefficients into confounding and rescaling[9], which allows to make a direct comparison of the coefficients in nested models, i.e. (1) vs (3).

Since our aim is to test how including measures of SWB affect previous standard models of retrospective voting, we follow their approach which consists on substituting

[8]This is due to the fact that the variance of the undelying latent variable is not identified and will be different between models.

[9]Karlos, Holm and Breen (2011) offer a method that gives unbiased comparisons of logit or probit coefficients of the same variable (x) across same-sample nested models successively including control variables (z). This solution decomposes the difference in the logit or probit coefficient of x between a model excluding z and a model including z, into a part attributable to confounding (i.e., the part mediated or explained by z) and a part attributable to rescaling of the coefficient of x.

the additional variable (*satisfaction with life* in this case) in (3) with the residuals from a regression of *satisfaction with life* on all the other controls included in (1).

The output from this exercise is displayed in Table 7. The table is divided into two vertical panels, the first one reports regression outputs for the full sample of respondents, and the second one for the swing voters sample. In each panel there are three columns, the first and the third ones, denoted [1] and [5b], correspond respectively to columns [1] and [5] in tables 4 and 6. The second column, denoted [5a], reports regression outputs when the method proposed by Karlos, Holm and Breen (2011) is applied. The bottom part of the table shows average partial effects.

The interpretation of the results is as follow. Looking at the full sample, an improvement in the financial situation compared to the previous year increases the probability of supporting the incumbent by 1.41 percentage points. Note that the coefficients of better financial situation in columns 1 and 5b are the same, suggesting that rescaling does not affect confounding. Controlling for satisfaction with life, this effect goes down to 1.36 percentage points, which is about a 4% decrease in the effect, due to confounding and net of rescaling. If we look instead at the effect of *satisfaction with life* on the worse financial situation dummy, we can see that there is a 14% reduction of the effect due to confounding net of rescaling.

For the sample of swing voters, the confounding effect of life satisfaction on financial situation is stronger, for example there is a reduction of the effect of better financial situation dummy of about 12% due to the inclusion of life satisfaction measures, but for worse financial situation dummy this reduction is over 62%.

So in summary, this exercise have confirmed that SWB measures and financial situation indicators affect voting decisions mainly through different channels, and therefore should be both included as regressors. Note also the SWB measures appear to be to some extent more robust than financial indicators.

4 Exogenous Shocks of (Un)Happiness

In the previous section we have shown that using well-being indicators together with financial indicators to proxy for utility is better than using only financial/economic measures. We have established that when a voter reports a higher (lower) level of well-being, she is also more (less) likely to support the incumbent.

In this section we present the results of an alternative exercise, which allows us to address two points. First, it constitutes a further robustness checks for the possible reverse causality in the relationship between voting and well-being. Second, it allows us to test the hypothesis whether voters correctly attribute to the government the responsibility of their well-being when they make their voting decisions.

Our identification strategy is: (i) to find an exogenous shock of life satisfaction independent from government policies and affecting only some respondents, our *treated group*; (ii) to select a matched sample of individuals who did not experience this shock (matched control group), but who have the same *ex ante* probability of experiencing the shock (propensity score matching); and (iii), to compare *before*-and *after*- shock changes in political support responses of affected individuals to changes in political support responses of unaffected individuals (DiD estimation).

The kind of shock that allows us to proceed (i) has to have a strong and significant impact on well-being and (ii) has to be independent from government actions. Our idea is to use the death of the husband or wife as a shock of life satisfaction. This event, which is arguably largely beyond government's control, is well known to have a deep *temporary* impact on well-being (see for example Clark and Oswald, 2002; Clark et al, 2006), and, interestingly, this effect is recognised to be stronger for women than men (Clark et al, 2006). So, widowhood fits well our purpose because it is possible to identify its exogenous component by using propensity score matching and, at the same time, it is largely beyond the government's control.

4.1 Propensity Score Matching

In order to be able to analyze the response to negative shocks of life satisfaction, such as those caused by an event like widowhood, we need to deal with two problems. First, a direct comparison between treated and untreated individuals is biased by the fact that differences across these two groups depend on selection. Second, the time of the treatment is respondent specific and cannot be imputed for the members of the nontreated group. Propensity score matching provides a solution to both problems. It involves relying on a set of observable characteristics that affect the "probability of being treated" (propensity score) in an attempt to reproduce the treatment group among the nontreated. Imputation of the time of treatment to the members of the control group is therefore made by pairing each of its individuals with a member of the treated group. Becker and Hvide (2013) use a similar approach to match firms with a deceased entrepreneur with firms where the organization never experienced a similar shock. In our setting, we use year of spouse death of treated respondents to impute the counterfactual year of spouse death of the matched control. So, in this way, we are able to define before and after spouse death for both treated respondents and matched controls.

We use nearest neighbor matching to select the group of individuals whose probability of experiencing widowhood between 1992 and 2008 (the whole length of the BHPS), conditional on characteristics observed in 1991, is the closest to that of the 363 individuals who did experience widowhood over the same period.[10] We begin computing the propen-

[10]This procedure involved omitting from the sample the individuals who had never been married, those who were always reported as widows, and those who remarried after widowhood.

sity score by estimating a probit for the likelihood of becoming a widow. Table 8 provides evidence of the good explanatory power of the chosen covariates, given the significance of their coefficients and the high $pseudo-R^2$ of 0.30.[11] The predicted probabilities estimated from this model constitute our propensity scores. Before matching, the average propensity score is 0.352 for the treated group, and only 0.073 for the nontreated group. After imposing a radius of 0.01 for the identification of the nearest neighbor to any individual belonging to the control group, we discard 134 individuals and remain with a sample of 230 respondents (153 of these are women and 77 men) who did experience widowhood and 230 matched respondents who didn't. In the matched sample, the average propensity score is reduced to 0.1963 for the treated group and 0.1952 for the control group. (Figure 2 in the appendix provides histograms for the estimated propensity score before and after matching.)

Table 9 reports statistics for the reduction in bias attained through the matching procedure: it reports the test of equality in the means of all used covariates across the treated and control groups, both before and after matching. The results from the last column suggest that, for all covariates, we fail to reject the null of mean equality after the matching procedure is concluded. (Figure 3 in the appendix provides a graphical representation of the same bias reduction)

4.2 DiD Setup

In section A.2 of the Appendix, we can observe that the shock of SWB following the spouse's death is negative and significant; it is stronger for women than for men, for whom in our sample it is nonsignificant, and it seems to be fading away with the years from the event. This is perfectly consistent with previous research (Clark et al., 2006).

Our main focus is now to understand whether the spouse death affects voting behavior such that it is decreasing with time following the event and, in general, follows a pattern similar to the shock in SWB. We are mainly interested in the differences after the event (the death), but we also look into the behavior before the death. As we will show there is no different behavior before the death which is consistent with the fact that the matching procedure has effectively worked by selecting individuals who do not have pre-treatment differences, even if the death is preceded by long period of illness.

We start by looking at the basic DiD regression, where we compare treated and matched controls to assess how voting intentions are affected by a spouse's death (treatment). We estimate the following model:

$$SupportInc_{it} = \alpha + \lambda_1 \times treated_i + \lambda_2 \times after_{it} \times treated_i + \lambda_3 \times after_{it} + \gamma \times X_{it} + \delta_t + u_{it} \quad (4)$$

[11] We also estimated this model with a larger set of variables controlling for a full set of personal, health-related, and financial characteristics. Other explanatory variables not included in this preferred specification resulted as consistently insignificant in all other robustness checks.

The coefficient of interest is λ_2, which measures the difference between treated respondents and control respondents after the treatment. The coefficient λ_1 also presents some interest because it constitutes a test for the lack of pretreatment effect. We include all the controls that have been previously included in the regressions; these are age (in linear and squared form), logarithm of family income, sex, as well as year and region dummies. Standard errors are clustered at the individual level. We estimate equation (4) using LPM.

Equation (4) is also extended in several directions to include some of the shock's characteristics that are formally reported in the appendix. First, since the shock turned out to be significant only for women, we look at the responses of men and women separately. We do it in two ways: (i) by interacting $after_{it} \times treated_i$ by sex of the respondent dummies; (ii) by running separate regressions for male and female respondents. Second, since the shock of wellbeing lasts for only two years after the death, we look if treated respondents differs from the control group only in the same period of the shock. To address this we estimate separately the effect on the year of the death, and 1 and 2 years after.

Finding that the effect on the probability of supporting the incumbent in the treated group lasts as long as the shock on life satisfaction and finding that the effect on women is stronger than in men, would allow us to attribute the effect of the treatment on voting intention to the shock of unhappiness.

4.3 DiD Results

We analyze whether individuals experiencing widowhood change their voting intention differently from individuals whose spouses do not die. Estimation results for equation (4) and its variations are displayed in tables 10, 11, 12, and 13. In most of our regressions, we consider windows of three and two years after and before the spouse death, but we also experiment with shorter and longer periods.

Columns [1], [2], and [3] of table 10 present the results for λ_2 when the data are restricted to respectively 4, 3, and 2 years after and before the treatment. We can observe that overall, there is a negative effect of widowhood on the probability of incumbent support; the effect is increasing and becomes significant in the sample of the two-year window (column [3]), from where we observe that such a shock decreases by about 8% the probability of voting for the incumbent. In columns 4 and 5, we obtain more precise estimates of the effect's duration, by estimating different coefficients for the year of the spouse death, {1,2} years after, or simply 1 and 2 years after. The effect of the shock seems to be decreasing, consistent with the effect on the life satisfaction shock. In these first five columns, we impose the restriction that men and women react in the same way to the spouse loss.

Columns [6] to [10] repeat the estimates of columns [1] to [5], after relaxing the restriction of homogeneous treatment effect by gender. We estimate different coefficients for men and women in the treated group. Consistently with the asymmetric shock of life satisfaction that hits the two sexes differently, the results show clearly that women are the ones whose voting behavior is affected by the spouse death; the λ_2 are negative and become significant when we restrict the sample to two or three years from the treatment. Again, we first start by estimating a common λ_2 for all years after the spouse death. The results suggest that women are about 7% to 9% less likely to vote for the incumbent following the death of their husband. When analyzing the duration of the effect, we obtain significant and negative coefficients for women in the year of the event (about -11%) and in the following year (about -12%) and a smaller nonsignificant effect two year after the event (about -5%). Coefficients for men are smaller and nonsignificant.

As a robustness check, we run separate regressions for men and women. The results are displayed in tables 11 and 12. From the inspection of the tables, we can clearly see that all the previous results are confirmed in terms of both magnitude and significance.

We can also observe that our matching technique has not left any pretreatment effect, in Section 4.2 we have shown that there are no differences between control and treated group at the beginning of the period. When we estimate (4) we also carry out tests that the two groups remain comparable in the periods before the treatment, to make sure that there are no pre-treatment differences between the two groups. The coefficients λ_1 presented in the first row of tables 10 to 12 show that this is indeed the case. To provide further evidence we interact the pre treatment period with pre-treatment *years before* {*1,2, 1-2*} dummies. The results displayed in the tables are again consistent with the assumption that there is no pre-treatment effect.

So we have shown that an exogenous shock of well-being affects voting intentions. This can be interpreted as a further evidence that SWB affects voting. Moreover, given that the death of the spouse is an event that is independent on government's action, we can conclude that voters blame (or reward) the government for actions/events it is not responsible for.

4.4 Heterogeneous Responses to Left- and Right-Wing Parties

One could argue that a well-being shock could affect an individual's political bias rather than simply her support for the incumbent. As shown in Oswald and Powdthavee (2010, 2014), a shock that makes the individual more (less) needy might increase (decrease) her support for a left wing party (i.e. the Labour Party in our case). Ideally, we would test whether individuals react differently to left and right governments by reestimating equations (1), (2), and (3) separately for the samples of Labour and Tories legislatures. Unfortunately, our data source provides us with well-being responses covering only one year (1996) of the Tory legislature, which opens a series of problems, particularly for the FE estimates of the LPM. As an alternative, we choose to reestimate equation (4), which employs data for the whole period 1992–2008 and, therefore, allows us to analyze the behavioral responses of respondents over the six years of Conservative against the eleven years of Labour legislature.

If Labour policies were more favourable to widows than Conservative policies and if voters were sensitive to this difference, then we should observe widows being more likely to support the incumbent during Labour legislatures than during Conservative ones. Observing no difference in the effect of widowhood on voting behavior among the two parties would instead bring evidence in favor of the "blaming" effect discussed in the previous section.

Table 13 presents our results. Columns [1] to [5] estimate the same models as the corresponding columns of table 10, with the addition of the interaction of the after treatment dummy with a temporal dummy identifying whether the government in power is led by the Labour Party. As we can see, the results seem to confirm our hypothesis that there is no significant difference between the two legislatures. The interaction of the after treatment dummy with the Labour temporal dummy is always nonsignificant. Column [5] suggests that the probability of supporting the incumbent in the first year following a spouse's death is 0.182 lower for the control than for the treatment group. This coefficient is comparable in magnitude and significance with the effect found in column [5] of table 9, our preferred specification (see also column [10] of the same table). Column [6] tests for the presence of pretreatment effect, and again finds that voting behavior changes only after the spouse's death.

4.5 Widowhood as an Instrument of SWB

The analysis presented so far relies on the underlying assumptions that experiencing widowhood directly affects subjective wellbeing. To further support the assumptions that motivated our identification strategy, we estimate a model where widowhood is explicitly used as an instrument for life satisfaction. Accounting for the fact that both the outcome variable, *SupportInc*, and the endogenous variable, $Wellbeing$, are discrete, we choose to estimate the following recursive bivariate probit model on the full sample of just above 4,800 individuals:

$$\begin{cases} SupportInc_i & = \delta_0 + \delta Wellbeing_i + \gamma_1 X_i + \epsilon_{1i} \\ Wellbeing_i & = \beta_0 + \beta Widowhood_i + \gamma_2 X_i + \epsilon_{2i} \end{cases}$$

where ϵ_{1i} and ϵ_{2i} are jointly distributed as bivariate normal with zero means, unit variances, and correlation ρ.[12] In this specification, the equation for well-being can be interpreted as the first step of an instrumental variable two-stage procedure, where widowhood plays the role of an exogenous instrument. The linear alternative to this specification (a standard IV-OLS model) provides consistent estimates of the average treatment effect,

[12]The parameters of interest can be estimated by full information maximum likelihood (FIML).

but is biased and has low small sample performance.[13]
The results from the estimation of this model are presented in table 14, where we only show the estimated relevant parameters. Model (1) is estimated on the full sample. The negative ρ reported at the bottom of the table indicates that the estimated correlation between the errors of the two equations (which is the conditional tetrachoric correlation) is negative and highly significant. The table additionally confirms that experiencing widowhood has a negative and significant effect on well-being, which, in turn, has a significant effect on the probability of supporting the incumbent. These results confirm our previous findings and validate our DiD approach.

5 Conclusion

Motivated by recent initiatives taken by governments and international organizations to come up with measures of SWB to yield informed policies that integrate standard monetary and financial measures, we test if well-being data can be used to predict voting behavior.

Our aim was to contribute to the empirical literature on retrospective voting by augmenting standard models of voting behavior with measure of well-being to proxy for utility. Preliminary results suggest voters change their voting intentions in response to changes in their level of life satisfaction.

There are two main sources of concern that we address in the paper. The first one is the possible reverse causality between voting and well-being when political ideology enters into the equation, as has been noted elsewhere (Di Tella and MacCulloch, 2005). For example, a strong Conservative supporter may be happy when the Tories are in power and not because of specific policy choices implemented by the party. We address this issue in two ways:*(i)* we split the sample between swing and partisan and we show that the swing react more to a SWB shock—the opposite behavior would have been true if our result was due to reverse causality; and*(ii)* we use widowhood as an instrument to better identify the model.

Once established that SWB measures are good indicators for predicting voters' behavior, we proceeded in the direction of asking whether or not voters are able to correctly reward or punish the incumbent government only for the variation in life satisfaction that is directly imputable to government actions. People's happiness may indeed depend on several factors and many of them are not directly imputable to government action. To address this, we test whether or not widowhood affects voter's preference toward the in-

[13]Chiburis, Das, and Lokshin (2011) run simulations similar to mine, and find that when there are no covariates, biprobit outperforms IV for sample sizes below 5000, and with a continuous covariate, biprobit outperforms IV in all of their simulations. They note that biprobit performs especially well when the treatment probability is close to 0 or 1, where linear methods are more likely to produce infeasible estimates.

cumbent. We use DiD estimation and propensity score matching to identify the effect that widowhood has on the probability of supporting the incumbent party. We find that a 1-point decrease in life satisfaction measured on a 7-point scale corresponds to a 12% decline in the support of the incumbent party. Consistent with our hypothesis, we find that the effect of widowhood on the SWB follows the same pattern as the shock on the support for the incumbent. We confirm the above results by estimating the effect of the shock on SWB and on the incumbent support together in a bivariate probit analysis.

We believe that our results have some important implications. First of all, they motivate the efforts taken by governments and international organizations in producing better and more comprehensive measures for well-being, since they appear to be valid indicators of what voters want, which is consistent with retrospective voting models. Second, they highlight citizens' inability to correctly blame or reward policy makers only for the actions they are responsible for. Finally, we note that this can provide an explanation for why elections are always held in May (in the UK), when the sun shines high and makes everybody happy!

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6 Tables

Table 1: Average Life Satisfaction, Conditional on Political Ideology

	Labour Partisan			"Swing"	Conservative Partisan		
	Strong	Medium	Weak		Weak	Medium	Strong
Conservative 1992	5.111	5.135	5.172	5.201	5.420	5.467	5.638
	(1.558)	(1.435)	(1.306)	(1.337)	(1.147)	(1.307)	(1.435)
Labour 1997	5.176	5.223	5.186	5.182	5.371	5.448	5.433
	(1.582)	(1.362)	(1.296)	(1.320)	(1.182)	(1.284)	(1.491)
Labour 2001	5.474	5.299	5.202	5.190	5.367	5.464	5.497
	(1.421)	(1.323)	(1.269)	(1.316)	(1.151)	(1.201)	(1.339)
Labour 2005	5.418	5.263	5.196	5.166	5.348	5.326	5.450
	(1.438)	(1.274)	(1.217)	(1.282)	(1.102)	(1.222)	(1.279)

Note: Data used for these descriptive statistics include the balanced sample of all individuals observed consecutively for all years between 1996 and 2008. Respondents dropped from the sample include those who were below the age of 16 in 1991, as well as the individuals in the top percentile of the income distribution and of the age distribution.

Table 2: Descriptive Statistics for Main Covariates

Variable	Obs.	Resp.	Mean	Std. Dev.	Min.	Max.
Support Incumbent	48432	4882	0.3749	0.4841	0	1
Life Satisfaction	48432	4882	5.2465	1.2236	1	7
Times Respondent Classifies as Nonpartisan	48432	4882	5.2037	5.3953	0	18
Widowhood	48432	4882	0.0049	0.0701	0	1
Income (ln)	48432	4882	7.3755	0.7116	-2.415	11.215
Age	48432	4882	49.6083	15.7044	18	97
Dummy (1 = female)	48432	4882	0.5541	0.4971	0	1
Dummy (1 = married)	48432	4882	0.6554	0.4752	0	1
Financial Situation Compared to Last Year = Better	48432	4882	0.2522	0.4343	0	1
Financial Situation Compared to Last Year = Worse	48432	4882	0.2388	0.4263	0	1

Note: Data used for these descriptive statistics include the balanced sample of all individuals observed consecutively for all years between 1996 and 2008. Respondents dropped from the sample include those who were below the age of 16 in 1991, as well as the individuals in the top percentile of the income distribution and of the age distribution.

Table 3: Baseline Equation, Linear Probability Models on Full Sample of Respondents

Dependent Variable: 1 If Supporting Incumbent Party		Financial Situation Only	Life Satisfaction Only		Financial Situation and Life Satisfaction	
		[1]	[2]	[3]	[4]	[5]
Financial Situation:	Better	0.0132***			0.0126***	0.0125***
		(0.0046)			(0.0046)	(0.0046)
	Worse	-0.0131***			-0.0120***	-0.0117**
		(0.0046)			(0.0046)	(0.0046)
Satisfaction with Life: Satisfied [5,6,7]			0.0185***		0.0161***	
			(0.0051)		(0.0051)	
Satisfaction with Life: [1,2,...,7]				0.0075***		0.0065***
				(0.0020)		(0.0020)
Observations		48,432	48,432	48,432	48,432	48,432
R-squared		0.0330	0.0327	0.0328	0.0332	0.0333
Number of pid		4,882	4,882	4,882	4,882	4,882

Note: Baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an FE LPM. Sample: 4,882 respondents observed since 1996. All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, age squared, and a dummy for female respondents). Region and wave dummies are also included. The variable "lfsato" from BHPS was used to define the level of life satisfaction. It is equal to seven different levels of life satisfaction, varying from completely satisfied (=7) to not at all satisfied (=1). For Model 2a and Model 3a, the variable is recoded as a dummy identifying whether the individual is satisfied (>4), whereas for Model 2b and 3b, life satisfaction is used as a continuous variable. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Baseline Equation, Probit Models on Full Sample of Respondents

Dependent Variable: 1 If Supporting Incumbent Party		Financial Situation Only [1]	Life Satisfaction Only [2]	[3]	Financial Situation and Life Satisfaction [4]	[5]
Financial Situation:	Better	0.0627***			0.0602***	0.0602***
		(0.0213)			(0.0213)	(0.0213)
	Worse	-0.0736***			-0.0682***	-0.0670***
		(0.0215)			(0.0216)	(0.0217)
Satisfaction with Life: Satisfied [5,6,7]			0.0829***		0.0699***	
			(0.0231)		(0.0233)	
Satisfaction with Life: [1,2,...,7]				0.0317***		0.0263***
				(0.0086)		(0.0087)
Log-likelihood		-22119	-22128	-22127	-22114	-22114
Observations		48,432	48,432	48,432	48,432	48,432
Number of pid		4,882	4,882	4,882	4,882	4,882
A.P.E w.r.t. Satisfaction with Life			0.0185	0.0071	0.0156	0.0059
			(0.0049)	(0.0018)	(0.0050)	(0.0018)

Note: Baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an RE probit. Sample: 4,882 respondents observed since 1996. All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, age squared, and a dummy for female respondents). Region and wave dummies are always included. The variable "lfsato" from BHPS was used to define the level of life satisfaction. It is equal to seven different levels of life satisfaction, varying from completely satisfied (=7) to not at all satisfied (=1). For Model 2a and Model 3a, the variable is recoded as a dummy identifying whether the individual is satisfied (>4), whereas for Model 2b and 3b, life satisfaction is used as a continuous variable. The Chamberlain RE probit estimates are obtained after controlling for observable respondent-specific time invariant characteristics. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Reducing endogeneity bias, Linear Probability Models on a Restricted Sample of Swing Voters

Dependent Variable: 1 If Supporting Incumbent Party		Financial Situation Only [1]	Life Satisfaction Only [2]	[3]	Financial Situation and Life Satisfaction [4]	[5]
Financial Situation:	Better	0.0121			0.0111	0.0109
		(0.0089)			(0.0090)	(0.0089)
	Worse	-0.0030			-0.0011	-0.0002
		(0.0088)			(0.0088)	(0.0088)
Satisfaction with Life: Satisfied [5,6,7]			0.0249***		0.0238***	
			(0.0087)		(0.0087)	
Satisfaction with Life: [1,2,...,7]				0.0112***		0.0108***
				(0.0034)		(0.0034)
Observations		12,926	12,926	12,926	12,926	12,926
R-squared		0.0770	0.0774	0.0776	0.0776	0.0778
Number of pid		1,520	1,520	1,520	1,520	1,520

Note: Baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an FE LPM; the coefficients reported represent the marginal effects. Sample: 1,520 respondents who are classified as "swing voters". All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, age squared, and a dummy for female respondents). Region and wave dummies are always included. The variable "lfsato" from BHPS was used to define the level of life satisfaction. It is equal to seven different levels of life satisfaction, varying from completely satisfied (=7) to not at all satisfied (=1). For Model 2a and Model 3a, the variable is recoded as a dummy identifying whether the individual is satisfied (>4), whereas for Model 2b and 3b, life satisfaction is used as a continuous variable. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Reducing endogeneity bias, Probit Models on a Restricted Sample of Swing Voters

Dependent Variable: 1 If Supporting Incumbent Party		Financial Situation Only [1]	Life Satisfaction Only [2]	[3]	Financial Situation and Life Satisfaction [4]	[5]
Financial Situation:	Better	0.0528			0.0494	0.0479
		(0.0405)			(0.0417)	(0.0406)
	Worse	-0.0291			-0.0176	-0.0139
		(0.0436)			(0.0441)	(0.0438)
Satisfaction with Life: Satisfied [5,6,7]			0.1277***		0.1218***	
			(0.0445)		(0.0448)	
Satisfaction with Life: [1,2,. . . ,7]				0.0540***		0.0518***
				(0.0163)		(0.0169)
Log-likelihood		-5,417	-5,415	-5,413	-5,414	-5,412
Observations		12,926	12,926	12,926	12,926	12,926
Number of pid		1,520	1,520	1,520	1,520	1,520
A.P.E. w.r.t. Satisfaction with Life			0.0242	0.0104	0.0231	0.0100
			(0.0085)	(0.0032)	(0.0086)	(0.0033)

Note: Baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an RE probit. Sample: 1,520 respondents who are classified as "Swing voters'. All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, age squared, and a dummy for female respondents). Region and wave dummies are always included. The variable "lfsato" from BHPS was used to define the level of life satisfaction. It is equal to seven different levels of life satisfaction, varying from completely satisfied (=7) to not at all satisfied (=1). For Model 2a and Model 3a, the variable is recoded as a dummy identifying whether the individual is satisfied (>4), whereas for Model 2b and 3b, life satisfaction is used as a continuous variable. The Chamberlain RE probit estimates are obtained after controlling for observable respondent-specific time invariant characteristics. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Baseline Equation, Average Partial Effect (APE) Comparison

		Full Sample			Swing Voters		
Dependent Variable: 1 If Supporting Incumbent Party		Financial Situation [1]	Financial Situation and Life Satisfaction [5a]	[5b]	Financial Situation [1]	Financial Situation and Life Satisfaction [5a]	[5b]
Financial Situation:	Better	0.0627***	0.0602***	0.0629***	0.0528	0.0479	0.0547
		(0.0213)	(0.0213)	(0.0083)	(0.0405)	(0.0406)	(0.0405)
	Worse	-0.0736***	-0.067***	-0.0781***	-0.0291	-0.0139	-0.0372
		(0.0215)	(0.0217)	(0.0210)	(0.0436)	(0.0438)	(0.0439)
Satisfaction with Life: [1,2,. . . ,7]			0.0263***	0.0263***		0.0518***	0.0518***
			(0.0087)	(0.0083)		(0.0169)	(0.0169)
Log-likelihood		-22,119	-22,114	-22,115	-5,417	-5,412	-5,412
Observations		48,432	48,432	48,432	12,926	12,926	12,926
Number of pid		4,882	4,882	4,882	1,520	1,520	1,520
A.P.E. w.r.t. Better Financial Situation		0.0141	0.0136	0.0141	0.0103	0.0093	0.0106
		(0.0046)	(0.0046)	(0.0046)	(0.0079)	(0.0079)	(0.0079)
A.P.E. w.r.t. Worse Financial Situation		-0.0163	-0.0148	-0.0174	-0.0056	-0.0027	-0.0072
		(0.0046)	(0.0047)	(0.0046)	(0.0084)	(0.0084)	(0.0084)
A.P.E. w.r.t. Satisfaction with Life			0.0059	0.0059		0.0100	0.0100
			(0.0018)	(0.0018)		(0.0033)	(0.0033)

Note: Baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an RE probit. All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, age squared, and a dummy for female respondents). Region and wave dummies are always included. For Models [5b], the variable "Satisfaction with Life" is replaced by the residuals from a regression of "Satisfaction with Life" on all other control variables included in Model [1]. The Chamberlain RE probit estimates are obtained after controlling for observable respondent-specific time invariant characteristics. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Propensity Score Regression - Probit Model on Individual Characteristics

Dep. Variable: Probability of Becoming Widowed between 1992 and 2008	
Age in 1991	0.0446***
	(0.00416)
Female	0.580***
	(0.0895)
In Working Age in 1991	-0.332**
	(0.138)
Dummy: 1 If Ever Smoked in Life	0.103
	(0.0788)
Dummy: 1 If Had Permanent Job in 1991	-0.113
	(0.0997)
Dummy: 1 If Employed Full Time in 1991	0.187*
	(0.101)
Dummy: 1 If Spouse/Husband Was Employed in 1991	-0.335***
	(0.0897)
ln (Household Income) in 1991	-0.116*
	(0.0649)
Dummy: 1 If in Good Health in 1991	0.0146
	(0.0866)
Dummy: 1 If Visited GP More Than Twice in 1991	-0.157*
	(0.0872)
Dummy: 1 If Ever Hospitalized in 1991	0.00542
	(0.121)
Dummy: 1 If Ever Used Alternative Medicine	0.211
	(0.155)
Dummy: 1 If Regularly Checks Blood Pressure	-0.0260
	(0.0798)
Dummy: 1 If Regularly Does Chest X-ray	0.108
	(0.104)
Dummy: 1 If Regularly Checks Cholesterol	-0.2013*
	(0.115)
Dummy: 1 If Regularly Checks Cancer	0.0134
	(0.0876)
Constant	-2.321***
	(0.674)
Observations	3,644
Log-likelihood	-825.06916
Pseudo R-squared	0.3030

Note: Probit model for the likelihood of experiencing widowhood between 1992 and 2008, conditional on characteristics observed in 1991. Sample of 3,644 respondents (obtained by excluding from the original sample of 4,882 individuals those who were not observed continuously between 1991 and 2008, those who were never married, and those who were always recorded as widow(er)s). In the sample of 3,644 individuals, there are 363 who experienced widowhood. Region and household-type dummies are included. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Propensity Score - Test on Mean Equality Before and After Matching

		Bias		Mean		Equality of Means	
	Sample	Treated	Control	%	% Red.	t-test	p>t
Age in 1991	*Unmatched*	56.436	39.116	135.2		24.890	0.000
	Matched	50.561	50.522	0.3	99.8	0.030	0.972
Female	*Unmatched*	0.732	0.545	39.6		6.880	0.000
	Matched	0.665	0.665	0	100	0.000	1.000
In Working Age in 1991	*Unmatched*	0.556	0.958	-106		-29.560	0.000
	Matched	0.822	0.809	3.4	96.8	0.360	0.719
Dummy: 1 If Ever Smoked in Life	*Unmatched*	0.260	0.275	-3.3		-0.590	0.558
	Matched	0.283	0.243	8.8	-171.3	0.950	0.342
Dummy: 1 If Had Permanent Job in 1991	*Unmatched*	0.381	0.695	-66.2		-12.270	0.000
	Matched	0.539	0.565	-5.5	91.7	-0.560	0.575
Dummy: 1 If Employed Full Time in 1991	*Unmatched*	0.288	0.580	-61.6		-10.800	0.000
	Matched	0.413	0.426	-2.8	95.5	-0.280	0.777
Dummy: 1 If Spouse/Husband Was Employed in 1991	*Unmatched*	0.318	0.629	-65.5		-11.710	0.000
	Matched	0.465	0.474	-1.8	97.2	-0.190	0.852
ln (Household Income) in 1991	*Unmatched*	9.461	9.899	-63.5		-11.970	0.000
	Matched	9.658	9.731	-10.5	83.4	-1.150	0.251
Dummy: 1 If in Good Health in 1991	*Unmatched*	0.764	0.792	-6.7		-1.230	0.219
	Matched	0.757	0.804	-11.5	-72.7	-1.240	0.216
Dummy: 1 If Visited GP More Than Twice in 1991	*Unmatched*	0.737	0.763	-6.1		-1.120	0.262
	Matched	0.704	0.709	-1	83.5	-0.100	0.919
Dummy: 1 If Ever Hospitalized in 1991	*Unmatched*	0.093	0.114	-6.8		-1.200	0.230
	Matched	0.104	0.078	8.6	-25	0.970	0.333
Dummy: 1 If Ever Used Alternative Medicine	*Unmatched*	0.055	0.040	6.8		1.320	0.187
	Matched	0.057	0.061	-2	70.1	-0.200	0.843
Dummy: 1 If Regularly Checks Blood Pressure	*Unmatched*	0.548	0.525	4.5		0.820	0.411
	Matched	0.522	0.522	0	100	0.000	1.000
Dummy: 1 If Regularly Does Chest X-ray	*Unmatched*	0.156	0.135	5.9		1.090	0.274
	Matched	0.148	0.143	1.2	79.1	0.130	0.895
Dummy: 1 If Regularly Checks Cholesterol	*Unmatched*	0.110	0.131	-6.7		-1.180	0.239
	Matched	0.139	0.117	6.7	0.3	0.700	0.487

Note: **Sample composition is 363 treated observations, 230 of which are on support, and 3,3280 control observations, 230 of which are matched.** The table reports the mean of the covariates relevant to the propensity score estimation, across the treated and control groups for both the matched and the unmatched samples. It also indicates the bias across the treated and control groups and a reduction in bias when adopting the matching procedure. Finally, it shows the results for a test of equality in the means of these covariates across the treated and control groups before and after the matching.

Table 10: DiD on Full Matched Sample, LPM

Dependent Variable: Support Incumbent	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Treated	0.0406	0.0470	0.0590	0.0590	0.0590	0.0407	0.0471	0.0590	0.0590	0.0590
	(0.0405)	(0.0422)	(0.0451)	(0.0451)	(0.0451)	(0.0406)	(0.0422)	(0.0451)	(0.0452)	(0.0452)
After*Treated	-0.0423	-0.0571	-0.0788**							
	(0.0396)	(0.0371)	(0.0354)							
After*Treated*Female						-0.0558	-0.0724*	-0.0952**		
						(0.0457)	(0.0438)	(0.0434)		
After*Treated*Male						-0.0171	-0.0291	-0.0489		
						(0.0598)	(0.0583)	(0.0577)		
Treated*Year of Spouse Death				-0.0821**	-0.0822**					
				(0.0364)	(0.0364)					
Treated*Year of Spouse Death*Female									-0.109**	-0.109**
									(0.0460)	(0.0460)
Treated*Year of Spouse Death*Male									-0.0327	-0.0328
									(0.0614)	(0.0614)
Treated*(1,2) Years After Spouse Death				-0.0769**						
				(0.0385)						
Treated*(1,2) Years After Spouse Death*Female									-0.0876*	
									(0.0467)	
Treated*(1,2) Years After Spouse Death*Male									-0.0576	
									(0.0613)	
Treated*1 Year After Spouse Death					-0.0896**					
					(0.0406)					
Treated*1 Year After Spouse Death*Female										-0.124**
										(0.0499)
Treated*1 Year After Spouse Death*Male										-0.0304
										(0.0654)
Treated*2 Years After Spouse Death					-0.0635					
					(0.0433)					
Treated*2 Years After Spouse Death*Female										-0.0496
										(0.0522)
Treated*2 Years After Spouse Death*Male										-0.0878
										(0.0694)
Observations	3,162	2,543	1,862	1,862	1,862	3,162	2,543	1,862	1,862	1,862
R-squared	0.033	0.036	0.041	0.041	0.041	0.033	0.036	0.041	0.042	0.043

Note: Sample composition is 230 treated and 230 matched control individuals; Models [1] and [2] further restrict, respectively, to four and three years before and after spouse death; Models [3] to [10] restrict to only two years before and after spouse death. OLS estimates are based on the regression showed in equation 4 ($SupportInc_{it} = \alpha + \lambda_1 xTreated_i + \lambda_2 xafter_{it} xtreated_i + \lambda_3 xafter_{it} + X'_{it}\gamma + \delta_t + u_{it}$), where $after_{it}$ is set to 1 in the years after spouse death. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared). Region and wave dummies are also always used. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 11: DiD on Matched Sample of Female Respondents, LPM

Dependent Variable: Support Incumbent	[1]	[2]	[3]	[4]	[5]	[6]
Treated	0.0344	0.0401	0.0504	0.0504	0.0503	0.0507
	(0.0507)	(0.0527)	(0.0553)	(0.0553)	(0.0554)	(0.0582)
After*Treated	-0.0660	-0.0796*	-0.101**			
	(0.0486)	(0.0457)	(0.0442)			
Treated*1 Year Before Spouse Death						-0.0007
						(0.0322)
Treated*Year of Spouse Death				-0.120***	-0.120***	-0.121**
				(0.0462)	(0.0462)	(0.0488)
Treated*(1,2) Years After Spouse Death				-0.0908*		
				(0.0480)		
Treated*1 Year After Spouse Death					-0.128**	-0.128**
					(0.0510)	(0.0543)
Treated*2 Years After Spouse Death					-0.0524	-0.0528
					(0.0536)	(0.0573)
Constant	0.634	0.852**	0.985**	0.995**	0.996**	0.996**
	(0.395)	(0.424)	(0.447)	(0.449)	(0.449)	(0.449)
Observations	2,079	1,669	1,218	1,218	1,218	1,218
R-squared	0.025	0.034	0.046	0.046	0.047	0.047

Note: Sample composition is 230 treated and 230 matched control individuals; Models [1] and [2] further restrict, respectively, to four and three years before and after spouse death; Models [3] to [6] restrict to only two years before and after spouse death. OLS estimates are based on the regression showed in equation 4 ($SupportInc_{it} = \alpha + \lambda_1 xTreated_i + \lambda_2 xafter_{it} xtreated_i + \lambda_3 xafter_{it} + X'_{it}\gamma + \delta_t + u_{it}$), where $after_{it}$ is set to 1 in the years after spouse death. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared). Region and wave dummies are also always used. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 12: DiD on Matched Sample of Male Respondents, LPM

Dependent Variable: Support Incumbent	[1]	[2]	[3]	[4]	[5]	[6]
Treated	0.0467	0.0559	0.0648	0.0648	0.0646	0.0697
	(0.0686)	(0.0714)	(0.0778)	(0.0779)	(0.0780)	(0.0834)
After*Treated	0.00829	-0.0119	-0.0257			
	(0.0698)	(0.0654)	(0.0602)			
Treated*1 Year Before Spouse Death						-0.00973
						(0.0556)
Treated*Year of Spouse Death				-0.00453	-0.00443	-0.00949
				(0.0597)	(0.0598)	(0.0663)
Treated*(1,2) Years After Spouse Death				-0.0370		
				(0.0660)		
Treated*1 Year After Spouse Death					-0.00372	-0.00877
					(0.0695)	(0.0804)
Treated*2 Years After Spouse Death					-0.0741	-0.0792
					(0.0747)	(0.0844)
Constant	0.976	1.112	1.465	1.464	1.464	1.465
	(0.779)	(0.834)	(0.950)	(0.951)	(0.950)	(0.951)
Observations	1,083	874	644	644	644	644
R-squared	0.057	0.057	0.060	0.060	0.061	0.061

Note: Sample composition is 230 treated and 230 matched control individuals; Models [1] and [2] further restrict, respectively, to four and three years before and after spouse death; Models [3] to [6] restrict to only two years before and after spouse death. OLS estimates are based on the regression showed in equation 4 ($SupportInc_{it} = \alpha + \lambda_1 xTreated_i + \lambda_2 xafter_{it} xtreated_i + \lambda_3 xafter_{it} + X'_{it}\gamma + \delta_t + u_{it}$), where $after_{it}$ is set to 1 in the years after spouse death. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared). Region and wave dummies are also always used. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 13: DiD on Full Matched Sample, Effect of Labour Legislatures

Dependent Variable: Support Incumbent	[1]	[2]	[3]	[4]	[5]	[6]
Treated	0.0417	0.0467	0.0592	0.0592	0.0592	0.0687
	(0.0408)	(0.0428)	(0.0459)	(0.0459)	(0.0459)	(0.0485)
After*Treated	-0.0752	-0.0803	-0.121*			
	(0.0735)	(0.0730)	(0.0693)			
After*Treated*Labour	0.0370	0.0283	0.0517			
	(0.0793)	(0.0794)	(0.0764)			
Treated*1 Year Before Spouse Death						-0.0052
						(0.0749)
Treated*1 Year Before Spouse Death* Labour						-0.0176
						(0.0920)
Treated*Year of Spouse Death				-0.0940	-0.0941	-0.101
				(0.0718)	(0.0718)	(0.0831)
Treated*Year of Spouse Death* Labour				0.0216	0.0216	0.0188
				(0.0812)	(0.0812)	(0.0922)
Treated*(1,2) Years After Spouse Death				-0.138*		
				(0.0768)		
Treated*(1,2) Years After Spouse Death*Labour				0.0712		
				(0.0827)		
Treated*1 Year After Spouse Death					-0.182**	-0.189**
					(0.0781)	(0.0877)
Treated*1 Year After Spouse Death*Labour					0.117	0.114
					(0.0847)	(0.0943)
Treated*2 Years After Spouse Death					-0.0800	-0.0872
					(0.0971)	(0.105)
Treated*2 Years After Spouse Death*Labour					0.0113	0.0084
					(0.103)	(0.111)
Labour Legislature	0.0641	0.0474	0.0375	0.0374	0.0375	0.0402
	(0.0467)	(0.0498)	(0.0547)	(0.0548)	(0.0548)	(0.0602)
Observations	3,162	2,543	1,862	1,862	1,862	1,862
R-squared	0.016	0.015	0.016	0.016	0.017	0.017

Note: Sample composition is 230 treated and 230 matched control individuals; Models [1] and [2] further restrict, respectively, to four and three years before and after spouse death; Models [3] to [6] restrict to only two years before and after spouse death. OLS estimates are based on the regression showed in equation 4 ($SupportInc_{it} = \alpha + \lambda_1 xTreated_i + \lambda_2 xafter_{it} xtreated_i + \lambda_3 xafter_{it} + X'_{it}\gamma + \delta_t + u_{it}$), where $after_{it}$ is set to 1 in the years after spouse death. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared). Region dummies are also always used. Standard errors are clustered by respondent and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 14: Bivariate Probit

	Full Sample Model (1)		Labour Legislations Only Model (2)	
Dependent Variable:	Support Incumbent	Satisfied	Support Incumbent	Satisfied
Satisfied with Life [lfsato=5,6,7]	0.6349***		0.5759***	
	(0.0799)		(0.0863)	
Widowhood		-0.2244***		-0.1937**
		(0.0814)		(0.0844)
Constant	-0.7873***	0.3219***	0.1183	0.3616***
	(0.1038)	(0.0921)	(0.1142)	(0.0992)
Observations	48,432		44,149	
Log-Likelihood	-55533.84		-50547.93	
Rho	-0.3596***		-0.3324***	
	(0.0484)		(0.0519)	
Wald Test (rho = 0)	7.3776		4.8554	
	0.0066		0.0276	

Note: Sample composition for Model (1) is all respondents observed since 1996; Model (2) restricts this sample to survey waves collected during Labour legislatures only. Respondents who never married and respondents always recorded as widow(er)s are excluded from the analysis. Models are estimated using a recursive bivariate probit, where the probability of supporting the incumbent depends on life satisfaction, which, in turn, is affected by widowhood. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared), and region and wave dummies. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

7 Figures

Figure 1: Distribution of Life Satisfaction Levels among British People

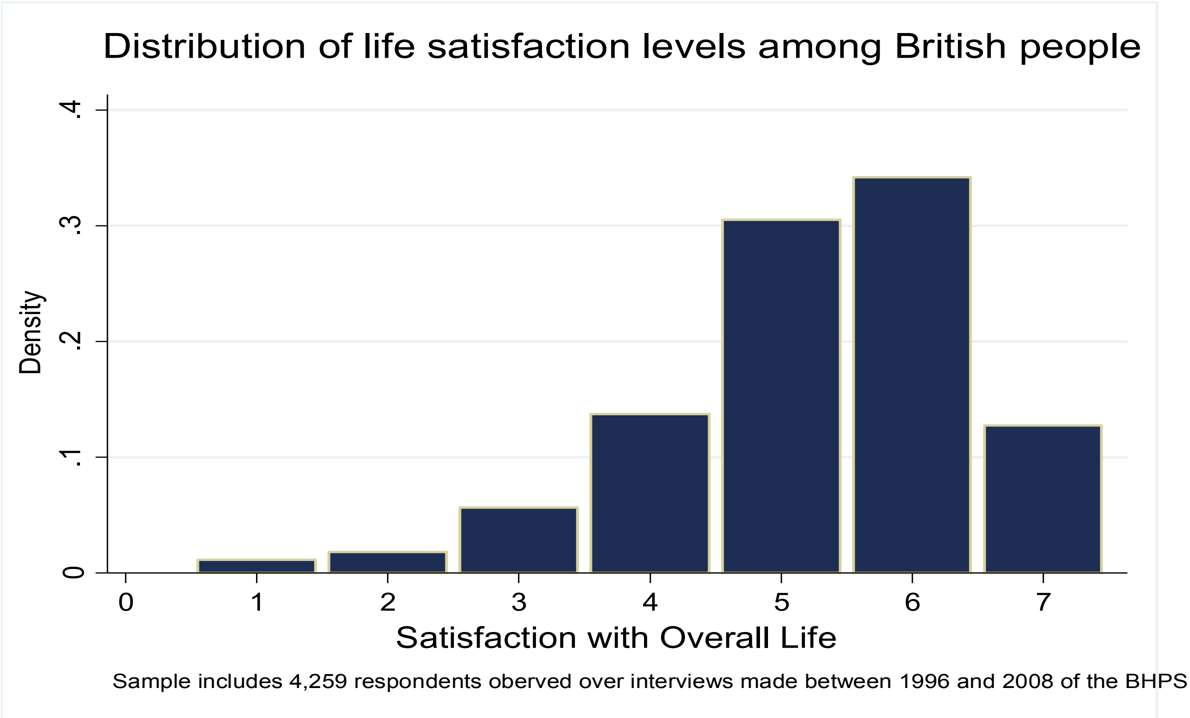


Figure 2: Histogram of Propensity Score, Conditional on Treatment Status

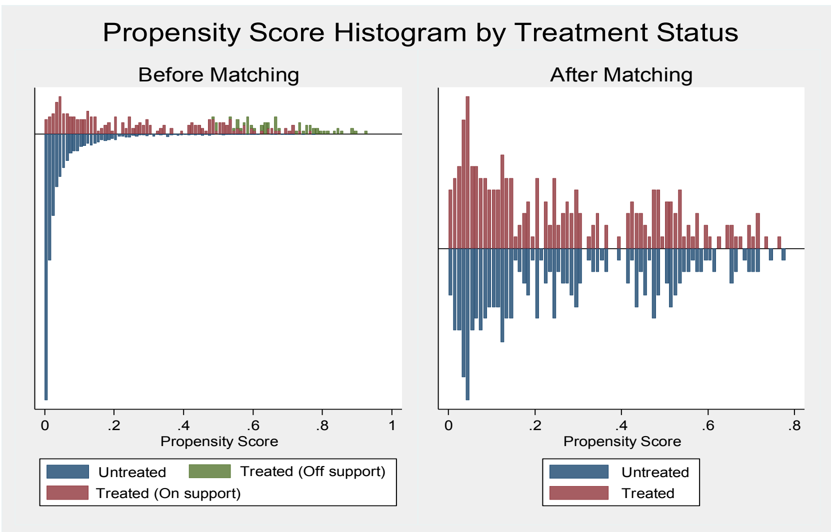
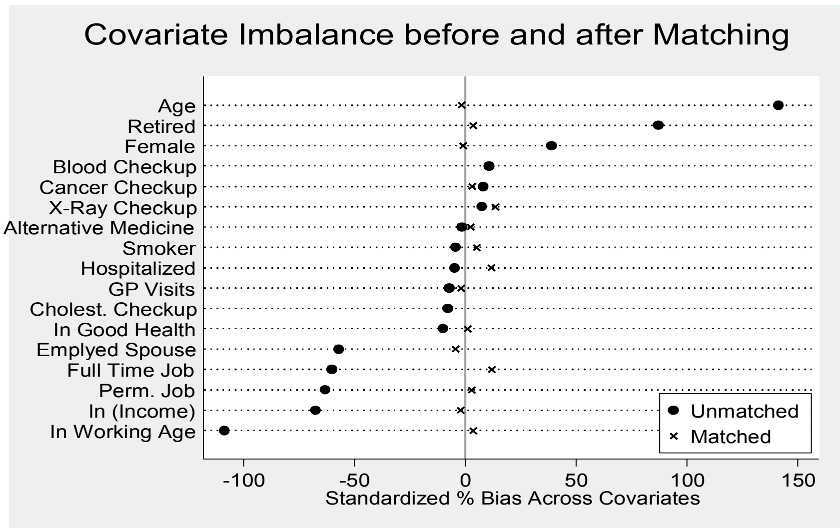


Figure 3: Covariates Imbalance Before and After Matching



A Appendix

A.1 Robustness Checks

Table A.1: Robustness Checks to Baseline Model (C-RE Probit), Each Level of Life Satisfaction

Dependent : 1 If Supporting Incumbent Party		Full Sample [1]	Swing Voters [2]
Financial Situation:	Better	0.0589***	0.0457
		(0.0213)	(0.0418)
	Worse	-0.0680***	-0.0195
		(0.0217)	(0.0443)
Satisfied with Life	[=1]	-0.0279	-0.1620
		(0.0880)	(0.1629)
	[=2]	-0.0006	0.1015
		(0.0683)	(0.1297)
	[=3]	-0.1750***	-0.2944***
		(0.0481)	(0.0921)
	[=4]	-0.0889**	-0.1571**
		(0.0385)	(0.0734)
	[=5]	-0.0493	-0.1088*
		(0.0338)	(0.0656)
	[=6]	-0.0044	0.0334
		(0.0316)	(0.0619)
Constant		-0.687*	-0.2424
		(0.398)	(0.6612)
Log-Likelihood		-22107	-5403.50
Observations		48,432	12,926
Number of Respondents		4,882	1,520
APE w.r.t. Sat.= 3		-0.0382	-0.0533
		(0.0100)	(0.0156)
APE w.r.t. Sat.= 4		-0.0195	-0.0296
		(0.0081)	(0.0135)
APE w.r.t. Sat.= 5		-0.0107	-0.0210
		(0.0070)	(0.0124)

Note: Robustness check for baseline model looking at determinants of the probability of supporting the incumbent party. Models are estimated using an RE probit model. Sample: full sample of 4,882 respondents, as in tables 3 and 4, and restricted sample of 1,520 less partisan voters, as in tables 5 and 6. All specifications include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared, and a dummy for female respondents), and time invariant characteristics used for Chamberlain specification. Region and wave dummies are always included. Life satisfaction = 7 is the baseline level. Standard errors are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

A.2 The Effect of Widowhood on SWB

To support the validity of our empirical strategy, we show in this section that widowhood actually constitutes a negative shock to life satisfaction, measured by self-reported subjective well-being. Using our matched sample, we run a difference-in-difference model to compare the effect widowhood had on the life satisfaction of the individuals who did experience such a shock to the effect such an event would have had on the counterfactual group. The respondents included in the analysis are the same used for the analysis in Section 4, but the sample is restricted to the years following 1996, as that is when we start observing SWB.

The study by Clark et al. (2008) shows that reported life satisfaction starts decreasing in the two years preceding the death of a spouse, reaches its lowest peak during the year of the spouse death, and then quickly readjusts toward the average level during the two years following the loss of the spouse. To test that our dataset also follows the same pattern, we estimate the following model:

$$Wellbeing_{it} = \alpha + \sigma_1 \times treated_i + \sigma_2 \times after_{it} \times treated_i + \sigma_3 \times after_{it} + \gamma \times X_{it} + \delta_t + u_{it}$$

The coefficient of interest is σ_2, which is the effect of widowhood on well-being for those individuals whose spouse died. We estimate several variations of this model, which include interacting $treated_i$ both with the sex of the respondents as well as with dummies indicating the number of years after the event, {*year of the death*}, {1, 2, 3, or 4 years after}.

The results for this exercise are reported in table A.2. Overall, in line with previous research, the shock of unhappiness is only significant for women, and it is reabsorbed after two years from the event. There is no evidence of a significant difference in the level of well-being between the treated and control groups three years from the event.

Table A.2: Results from Difference-in-Difference Estimates of Widowhood on Life Satisfaction

Dependent Variable: Life Satisfaction	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.210	-0.212	-0.212	-0.208	-0.210	-0.209
	(0.132)	(0.132)	(0.132)	(0.132)	(0.132)	(0.132)
After*Treated	-0.400***					
	(0.120)					
After*Treated*Female				-0.520***		
				(0.141)		
After*Treated*Male				-0.156		
				(0.157)		
Treated*Year of Spouse Death		-0.658***	-0.658***			
		(0.147)	(0.147)			
Treated*Year of Spouse Death*Female					-0.865***	-0.865***
					(0.180)	(0.181)
Treated*Year of Spouse Death*Male					-0.252	-0.252
					(0.193)	(0.193)
Treated*(1,2) Years After Spouse Death		-0.473***				
		(0.129)				
Treated*(1,2) Years After Spouse Death*Female					-0.615***	
					(0.156)	
Treated*(1,2) Years After Spouse Death*Male					-0.195	
					(0.173)	
Treated*(3,4) Years After Spouse Death		-0.192				
		(0.130)				
Treated*(3,4) Years After Spouse Death*Female					-0.258*	
					(0.149)	
Treated*(3,4) Years After Spouse Death*Male					-0.0565	
					(0.189)	
Treated*1 Year After Spouse Death			-0.514***			
			(0.139)			
Treated*1 Year After Spouse Death*Female						-0.624***
						(0.169)
Treated*1 Year After Spouse Death*Male						-0.304
						(0.192)
Treated*2 Years After Spouse Death			-0.428***			
			(0.146)			
Treated*2 Years After Spouse Death*Female						-0.606***
						(0.176)
Treated*2 Years After Spouse Death*Male						-0.0739
						(0.205)
Treated*3 Years After Spouse Death			-0.121			
			(0.143)			
Treated*3 Years After Spouse Death*Female						-0.181
						(0.167)
Treated*3 Years After Spouse Death*Male						0.0005
						(0.206)
Treated*4 Years After Spouse Death			-0.263*			
			(0.142)			
Treated*4 Years After Spouse Death*Female						-0.334**
						(0.161)
Treated*4 Years After Spouse Death*Male						-0.115
						(0.228)
Observations	2,617	2,617	2,617	2,617	2,617	2,617
R-squared	0.093	0.097	0.098	0.095	0.101	0.102

Note: The sample used is restricted to 4 years before and after the event. All specifications also include auxiliary control variables (a dummy for "married" individuals, the natural logarithm of yearly household income, age, and age squared). Region and wave dummies are also always used. Robust standard errors, clustered at the individual level, are in parentheses. *significant at * 10, ** 5, *** 1%.