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# Unknown Eligibility whilst Weighting for Non-response: The Puzzle of who has Died and who is still Alive?

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## Non-technical summary

Most panel surveys are designed to study a living population in a country. Sample members of panel studies are interviewed repeatedly over time. The conclusions arrived at through the sample are generalised to the whole population. Thus, such samples should be representative of the relevant populations. Otherwise, the results generated from the sample cannot be said to be correct with respect to the whole population.

Survey organisations usually prepare numerical quantities to be included in the data to compensate for sample members who do not participate. These quantities, called ‘weights’, increase the influence of participants who appear to be similar to those who did not participate. It is therefore important to be able to distinguish between non-participants and sample members who have died and are therefore no longer eligible to participate. One particular challenge that faces survey researchers in panel studies is that when a sample member stops participating at some point during the course of the survey it is not easy to be sure whether or not that person is still alive at any particular point in the future. Some of these non-participants might be dead in which case they must be excluded from the calculation of the weights. Otherwise, the characteristics of dead persons will mistakenly be used to increase the influence of those who participate and have similar characteristics. Hence the survey results may become misleading.

This research evaluates a method of estimating percentages of those who died in the sample amongst those who cease participation. The estimation is based upon using information on death percentages from the population. The estimated percentages are used to reduce the effect of those who died in the context of compensating for those who are still alive and did not participate. I used the British Household Panel Survey data, and I find that some of those who stop participating are dead. Most of the dead persons amongst those who stop participating are in the oldest age group in the sample. When this information is used in the compensation for non-participation, some results change considerably.

**Unknown Eligibility whilst Weighting for Non-response: The Puzzle of who has Died and who is still Alive?**

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## Abstract

A challenge for longitudinal surveys is to update eligibility status over time for sample units, including those who cease participating. Ineligible units should be excluded from the base for weighting, as weighting would otherwise increase the influence of responding sample units that are similar to the ineligible ones. This paper estimates likelihood of eligibility of the British Household Panel Survey (BHPS) sample members by gender and age using population mortality data. This likelihood is then used to adjust non-response weights. Analysis is conducted using adjusted and unadjusted weights. Results show that the adjustment affects the significance level of some variables.

## Keywords

Survey eligibility; population of interest; representative sample; non-response; weighting.

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## 1. Introduction

Longitudinal surveys follow respondents over time and continue gathering data from them, which not only enables the collection of up-to-date answers to the survey questions, but also allows the possibility of adding new questions. These complicated features of longitudinal surveys leads to a thorough understanding of dynamic populations, which cannot be achieved by conducting cross-sectional surveys (Lynn, 2009). However, to achieve this reward, a well designed representative sample is required. The sample selected for a longitudinal survey is usually designed to represent the population of interest at the start of the survey (Lynn, 2011). Surely, the population of interest changes overtime as people are born, immigrate, die and/or emigrate (Lynn, 2011). Thus, the sample is modified to maintain representativeness of the population of interest during the course of the study. New eligible members join the sample through a specific mechanism that is often established by the sample design[1]. Accordingly, the survey researcher has control over the system by which new members join the sample, and hence new eligible sample members can be known. In turn, some sample members may die or move out of the scope of the survey, in which case they become ineligible and are no longer part of the population of interest. These ineligible participants should be identified and excluded from any analysis that aims to provide estimates of the population of interest. Otherwise, the sample used for estimation will not represent the study population correctly and will, consequently, provide biased estimates.

However, identifying eligibility status of survey participants over time can be a challenging task. For example, during the course of the survey, the survey organisation may lose track of some of the respondents, because, for instance, they have moved house without informing the survey conductors. Also, sometimes, even if the interviewer is successful in contacting a household, respondents may not want to take part in the survey. All of these cases, and any others where an interview cannot be conducted, result in non-

[1] The process by which new eligible members join the sample varies across surveys. For example, in Understanding Society in the UK, new born children of any eligible female sample member are added to the sample as eligible sample members (Lynn, 2011).

response (Sadig, 2011). Non-response obstructs the identification of eligibility status of survey participants, since little information about them is available. This is particularly a dilemma at waves subsequent to the initial non-response incident as information about non-respondents may not be available at all. For example, consider a survey that does not attempt to re contact sample members who refuse to participate in the survey when they are first contacted. At the first contact attempt (say the first wave), the survey researcher maybe able to identify which cases amongst those who refused to participate are actually eligible since the interviewer managed to contact them. However, it is almost impossible to classify the same cases as either eligible or ineligible at the, say, fifth wave when the last contact was made with them four waves ago.

As a result, eligibility status for a considerable number of non-respondents will be unknown. Therefore, estimating the eligibility status for respondents whose eligibility is unknown has puzzled survey researchers for the past few decades.

Unknown eligibility raises a number of practical concerns in longitudinal surveys. The major concerns are:

(a) It disturbs the calculation of the survey quality measures such as response rate, contact rate and co-operation rate.
(b) More importantly, it may spoil the reward of any weighting process endeavours to correct for non-response.

The existing methods (described in section 6) of estimating the proportion of eligible respondents among respondents of unknown eligibility are inadequate (Smith, 2003). Although these methods can be used to estimate the proportion of eligible respondents, which may help in calculating more accurate quality measures, they fail to assist in accurately calculating the weights. This is because estimating eligibility status is not done at the case level, and as a result, some ineligibles may be included in the creation of the weights.

This paper investigates an alternative approach to estimate eligibility status using population information from an external source. This approach allows for estimating proportions of eligible respondents by gender and age. The point of interest is to compare the estimated proportions of eligibility in the population with those in the sample so as to work out an adjustment coefficient that represents eligibility likelihood for sample members of uncertain eligibility for each category of gender and age group. This coefficient can then be used to modify non-response weights before assigning them to cases in the sample. As a result, the effect of weighting will be controlled by likelihood of eligibility of the respondent. This will reduce any bias that might potentially occur due to including ineligible units in the calculation of the weights.

Aside from survey-specific characteristics of ineligible respondents, the most common characteristics of being ineligible for a survey are: death, moving out of the geographical area that is covered by the survey and being institutionalised –such as going to prison- where contact cannot be made. In all these cases, conducting an interview is unfeasible. However, in this paper, the discussion is limited to death. Death is a special case of ineligibility since it is an absorbing state (i.e. once a respondent become ineligible through death, they can never be eligible again). Thus, the terms 'eligible' and 'ineligible' are used in this paper to refer to survival and death respectively.

The paper uses data from The British Household Panel Survey (BHPS) and mortality rates from the Office for National Statistics (ONS) to estimate eligibility rates.

The paper reviews the concept of eligibility and the effect of uncertain eligibility on the calculation of survey quality measures, but it mainly focuses on dealing with unknown eligibility in the weighting context.

## 2. Eligibility

The objective of sample surveys is to make inference about a population based on information obtained from the sample. Usually, the population of interest is defined precisely according to specific characteristics. Sample units whose characteristics match the characteristics of the population of interest are referred to as eligible sample units. Defining eligibility is a crucial step in every survey. Conditions for being eligible vary between surveys, depending on the aim and objectives of the survey. In many surveys, the definition of eligibility is linked to a certain period or point in time. For example, in a survey of smokers, if being eligible is defined as being a smoker, the survey organisation should link the definition of eligibility to a specific time period, as individuals may start or stop smoking during the data collection period.

Since the only usable data for analysis are collected from eligible sample units, ineligible cases are dropped from the sample, and as a result, the sample size is then reduced. Thus, especially in cross-sectional surveys, it is advantageous to increase the eligibility rate, by, for example, pre-screening the sample units before selecting the sample. This is because, at the sampling stage, sometimes it is difficult for the survey researcher to spot some of the undesirable or ineligible cases (cases that are not part of the population of interest) in the sample frame. For instance, in random dialling digit surveys the sample frame may contain non-working numbers; however, it might be impossible to know this unless a contact attempt is made (Groves *et al*, 2004).

In contrast, in longitudinal surveys, pre-screening the sample may not be of great benefit in the long term. This is because individuals' characteristics that match the characteristics of the population of interest can change overtime, allowing respondents to be part of the population in the earlier waves but not in the later waves. For example, if being eligible in a survey is defined by living in the country where the survey is conducted, some participants may leave the country after participating in a number of waves, and, as a result of this, they become out of the scope of the population of interest. This complexity demonstrates that dealing with eligibility in longitudinal surveys is more problematic.

Although it is cost effective if ineligible participants are identified before they are issued an interviewer, usually ineligible units cannot be identified until the data collection is completed and the interview is conducted.

**3. Unknown eligibility**

The term 'unknown eligibility' is used to refer to the status where there is not sufficient information about a respondent to allow them to be identified as either an eligible or ineligible participant after the data collection stage is completed.

The most common outcomes of any contact attempt are completed interview, refusal or non-contact. In a successful interview, eligibility is usually known, since the interviewer is able to receive responses to at least a large part of the questionnaire. However, with non-response, which occurs through refusal or non-contact, information about non-respondents is very limited and sometimes not available. Therefore, the survey researcher may be able to identify eligibility for some of the non-respondents, but for a substantial proportion, eligibility will remain unknown.

Unknown eligibility can be resolved in the case of wave non-response, where respondents are not present for at least one wave, but they resume participation at some point during the course of the survey. In this case, information related to eligibility status during the period of absence can be collected in the current interview. In turn, in panel studies, a special case of unknown eligibility occurs through attrition. Attrition is identified as the permanent dropout from a longitudinal survey after having participated at previous points of data collection (Chang, 2010). In this case, the survey researcher is unable to identify the eligibility status of sample persons even though they were eligible when they gave their last interview. Thus, it is desirable to reduce attrition by keeping track of respondents between waves (McGonagle *et al*, 2011; Laurie *et al*, 1999).

Nonetheless, despite the use of different strategies to minimize attrition in many studies (Laurie and Lynn, 2009; Laurie *et al*, 1999), in some cases it is impossible to retain survey participation. Death is one example of this and may be of particular concern in longitudinal surveys. If death is not reported to the survey organisation, dead respondents will be classified as respondents whose eligibility is unknown.

## 4. Unknown eligibility and response rate, contact rate and co-operation rate

When the data collection stage is completed, survey organizations usually publish some statistics such as the response rate, contact rate and co-operation rate, to reflect the main features of the data and inform data users about the quality of the data that the survey has gathered. However, each of these rates is defined as a ratio that contains the proportion of eligible sampled units in the denominator. Thus, an incorrect estimate of the proportion of eligible units amongst units of unknown eligibility will result in under or over estimating response, contact and co-operation rates.

### 4.1 Response rate

The response rate measures the percentage of the completed interviews[2] out of all the eligible units. For many surveys, response rates are calculated to examine the quality of the survey and the effort put forward to achieve the interviews. Additionally, the response rate may draw attention to investigating potential bias in the estimates. Usually, higher response rates are preferred, as they might reduce the potential bias introduced by non-response. However, the response rate on its own does not provide information about non-response error, but calculating the rate is a crucial stage in examining the presence of non-response error. The definition of the response rate implies that ineligible sample units should not be included in the calculation if the rate is to be computed accurately.

According to the Survey Research Centre (SRC), University of Waterloo (2005), the response rate (RR) is defined as

$$RR = \frac{\text{Number of eligible sample units with completed interviews}}{\text{Number of eligible sample units}} \qquad (1)$$

However, it is almost impossible to calculate the denominator precisely, since the number of eligible cases among non-respondents cannot be identified exactly. In almost every survey, as long as there is an incidence of non-response, there will be a number of cases whose eligibility remains unknown. Therefore, the total number of sample units (TNSU) can be broken down into two components

TNSU= Number of units with known eligibility + Number of units with unknown eligibility

Consequently, in order to calculate the response rate, the survey researcher has to estimate the number of eligible units among the units of unknown eligibility (AAPOR, 2011). The number of estimated eligible sample units (NEESU) is a sub-group of the number of units

[2] The literature on response rate usually distinguishes between fully completed questionnaires and partially completed questionnaires. For the purpose of this research both types are considered as one category.

with unknown eligibility. Accordingly, the definition of the response rate in equation (1) can be rewritten as

$$RR = \frac{\text{Number of eligible sample units with completed interviews}}{\text{Number of eligible units of known eligibility} + \text{NEESU}} \tag{2}$$

Overestimating NEESU leads to underestimating the response rate, while underestimating NEESU results in overestimating the response rate. Therefore, regardless of the method used to estimate NEESU, it is advisable to utilize a value of NEESU that does not inflate the response rate and hence give a false sense of valuing the quality of the data.

### 4.2 Contact rate

The contact rate indicates the proportion of persons who were contacted by the interviewer, even if they refused to participate in the survey or were unable to provide any type of information (Gasteiz, 2007; SRC, 2005).

The contact rate (CR) is defined as

$$CR = \frac{\text{Number of eligible sample units in which contact was made}}{\text{Number of eligible sample units}} \tag{3}$$

Similar to the response rate, the denominator cannot be known precisely with the presence of non-response. Thus, to calculate CR, it is vital to estimate the number of eligible units amongst those of unknown eligibility. Yet again, this can be represented by NEESU. Thus, equation 3 can be rewritten as follows:

$$CR = \frac{\text{Number of eligible sample units in which contact was made}}{\text{Number of eligible units of known eligibility} + \text{NEESU}} \tag{4}$$

### 4.3 Co-operation rate

The co-operation rate measures the proportion of achieved interviews among the cases in which contact was made (Gasteiz, 2007; SRC, 2005).

The co-operation rate (CoR) is defined as

$$CoR = \frac{\text{Number of sample units in which interview was conducted}}{\text{Number of eligible sample units in which contact was made}} \tag{5}$$

In many social science surveys (such as the BHPS), a sample member is eligible if they are alive and living in the geographical area covered by the survey. In this case, the survey researcher does not need to calculate NEESU in order to calculate the co-operation rate. This is because the denominator in the co-operation rate only consists of sample members who are successfully contacted and hence eligible (i.e. alive and living in the geographical area covered by the survey).

However, using the contact rate and the co-operation rate, the response rate can be redefined as

Response rate (RR) = Contact rate (CR) * Cooperation rate (CoP) (6)

Nevertheless, calculating the response and/or contact rates precisely requires the availability of the number of eligible sample units among non-respondents (NESU). Since it is not possible to count the NESU, it can be replaced with the number of estimated eligible sample units (NEESU). NEESU can be estimated using a number of practical methods. These methods are reviewed in section (6).

Thus, regarding the calculation of the survey quality measures, unknown eligibility can – to an extent- disturb the calculation of some of these measures. However, with a good estimation of NEESU, one can still calculate the response rate and the contact rate to the best possible approximation.

## 5. Unknown eligibility and weighting

In longitudinal surveys, apart from calculating precise quality measures, identifying the eligibility status of respondents will benefit the weighting process. Weighting is a process by which a higher value is assigned to some of the eligible respondents in the survey, in order to modify them to represent eligible individuals who are missing due to non-response or an incomplete frame (Sadig, 2011; Biemer and Christ, 2008; Lynn, 2005). Thus, in order for the weights to modify the sample correctly, they should be calculated using eligible sample members only. Including ineligible sample members in the non-response model that is used to calculate the weights will lead to inaccurate sizes of the weights. For example, for a given survey where eligibility is defined as being alive, suppose that

$n_{ijk}$ indicates the number of sample units (where *i*, *j* and *k* denote eligibility status, knowledge of eligibility, and survey response status respectively; and that

*i* = 1 if eligible; 2 if ineligible (actual status, regardless of whether this is known);

*j* = 1 if eligibility status is known; 2 if it is not known;

*k* = 1 if survey respondent; 2 if non-respondent.

Thus,

$\sum_{i=1}^{2}\sum_{j=1}^{2}\sum_{k=1}^{2} n_{ijk} = n_{\bullet\bullet\bullet}$ is the total sample size.

We can assume that $n_{121} = n_{211} = n_{221} = 0$ (i.e. that all respondents are eligible and known to be eligible)

Thus,

$n_{111}$ is the number of respondents;

$n_{112}$ is the number of non-respondents known to be eligible;

$n_{212}$ is the number of non-respondents known to be ineligible; and

$n_{122}+n_{222}$ is the number of non-respondents of unknown eligibility

Now, for a given weighting class 'c', the response probability ($Þ_c$) is calculated by dividing the number of responding units by the total number of respondents in the class. Accordingly, the relevant non-response adjustment weight ($w_c$) is calculated by the inverse of the response probability in the class.

Thus, in the presence of unknown eligibility:

$$Þ_{1c} = \frac{n_{111}}{n_{11.} + n_{222}} \quad \text{leading to} \quad w_{1c} = \frac{n_{11.} + n_{222}}{n_{111}}$$

But, with perfect information about eligibility, this should be:

$$Þ_{2c} = \frac{n_{111}}{n_{11.}} \quad \text{leading to} \quad w_{2c} = \frac{n_{11.}}{n_{111}}$$

Noticeably, $w_{1c} > w_{2c}$. Thus, with $w_{1c}$, cases in class 'c' will be over-weighted (i.e. if ineligible cases amongst cases of unknown eligibility are not identified as such and are excluded from weights' calculation). Moreover, the size of $w_{1c}$ incorrectly increases as more cases are added to $n_{222}$. In other words, the relevant weight in a given class will mistakenly be increased, if more non-respondents ineligible cases are not identified in that class.

Accordingly, this is particularly a problem if ineligible cases among cases of unknown eligibility are not evenly distributed across weighting classes. In this case, respondents in weighting classes with larger proportions of unidentified ineligible cases, will have larger weights. As a result, cases in these weighting classes will be over-weighted. Consequently, weighted estimates will be biased towards characteristics from classes where more ineligible cases are not identified.

This is because the weights will increase the influence of responding units in these classes and hence will mistakenly boost the sample, by representing a proportion of individuals who are not part of the population of interest. Consequently, estimates resulting from such a weighting strategy are biased. With regard to ineligibility through death, health studies have shown that death is associated with socio-demographic characteristics such as age and gender (Singh-Manoux *et al*, 2008; Dr Foster, 2004). That is to say, in most parts of the world women are expected to live longer than men, and mortality rates are higher among older age groups than among their younger counterparts. Therefore, death does not happen completely at random. Hence, results based on a weighting strategy that mistakenly include dead respondents in the weights' calculation may be biased towards the categories of respondents that have higher death rates.

Nonetheless, it is not always possible to be aware of the death of non-respondents, especially if the respondent is a single-person household. In this case, the death of the respondent may not be reported since there is no other household member to do so.

There is an increasing interest in the issue of weighting under uncertain eligibility. However, at present, we do not know of any attempt that deals with uncertain eligibility while weighting to correct for non-response. In most longitudinal surveys such as the British Household Panel Survey (BHPS), Swiss Household Panel (SHP) and the German Socio Economic Panel (GSOEP) weighting assumes that individuals whose eligibility is uncertain are eligible (Taylor *et al*, 2010; Plaza and Graf, 2008; Kroh, 2009).

However, eligible respondents can be identified in proportions by one or two categorical variables (e.g. age group and ethnicity). These proportions across the two selected variables can be estimated using information from population data. Such information could be available from an external source (e.g. census). The same proportions can be calculated in the sample. Comparing these proportions of eligibility in the sample with those in the population can disclose the degree at which eligibility rates are miscalculated in the sample. Furthermore, this comparison, since it is done across the categories of two variables can assist in identifying the likelihood that a respondent in a given category is eligible. As a result, non-response weights in a given category can be controlled by the likelihood of eligibility for respondents in that category. This strategy of weighting (explained in section 7) is the subject of this paper.

## 6. Methods of estimating the proportion of eligible cases

There are several methods which are usually used to estimate the rate of cases of unknown eligibility that are actually eligible 'e'. Most of the literature in this area assumes a random digit dialling survey (RDD) (Smith, 2003). Therefore, some of the methods are RDD specific.

*Minimum and maximum allocation*: in this method 'e' is assumed to be either 0% or 100% of the cases of unknown eligibility (Lessler and Kalsbeck, 1992; smith 2003). Accordingly, more than one response rate can be produced. Taking 'e' as 0%, gives the maximum possible response rate while substituting 'e' as 100% produces the lowest response rate. Smith (2003) indicates that this method is only useful in determining the upper and lower bounds of the response rate. However, one can obtain a range of rates by varying the values of 'e' from 0% to 100% before choosing a plausible value that does not inflate the response rate. However, if the level of unknown eligibility is high, the number of the possible response rates will become impractical.

*Proportional allocation*[3]: this method assumes that 'e', among the cases of unknown eligibility, is the same as among the cases whose eligibility is known (Frankel, 1983; Lessler and Kalsbeck, 1992; Smith, 2003; Barron, Khare and Zhao, 2008). Smith (2003) states that proportional allocation is conservative as it produces a high value of 'e', and hence does not inflate the response rate. However, he argues that it might produce a biased estimate of 'e' because it assumes that the eligibility rate among the unobserved sample is the same as among the observed sample.

*Survival analysis*: this method is the standard survival analysis method in which the number of contact attempts is used to estimate the eligible cases among the cases of unknown eligibility (Frankel *et al*, 2003; Smith, 2003). This method is considered to be a better approach to estimating 'e', since it uses more information from the sample than the other methods. However, Smith (2003) argues that one cannot be certain that the statistical assumptions of survival analysis are properly met.

*RDD specific methods*: there are a few methods used in random digit dialling surveys to estimate the eligibility rate among the unknown eligibility cases. The most commonly used of these are: allocation based on disposition codes and contacting telephone business offices. Under the disposition codes allocation approach, the outcome of the call attempt

[3] Some of the literature on the response rate refers to this method as CASRO type II as it is proposed by the American Survey Research Organisations.

is used to identify whether a case is eligible or not (Smith, 2003). For example, in a survey, a researcher might establish a rule that all of the phone numbers with answering machines are eligible, while those resulting in busy signals are not eligible. The limitation of this method is that the basis in which the disposition codes are allocated may not solely determine the eligibility. For example, a ring-no-answer alone is not enough to identify a case as being ineligible.

As for the business offices approach, survey researchers sometimes contact local telephone business offices to enquire about the status of the unknown numbers (Smith, 2003; Frankel *et al*; 2003). However, this method is considered to be both money and time consuming, in addition to the fact that business offices usually refuse to give out information about phone numbers.

Many studies have applied the above methods to estimate the eligibility rate among the cases of unknown eligibility. For example, Barron, Khare and Zhao (2008) applied the proportional allocation approach to estimate 'e', to calculate the response rate for the National Immunization Survey's Cell Telephone Pilot study (NIS-CTP). Gasteiz (2007) indicates that the minimum and maximum allocation method was used ('e' was assumed to be 100%) to estimate the eligible cases among those cases where eligibility is unknown in the Population in Relation to Activity Survey (PRA). In a list-assisted RDD telephone survey about adolescent substance abuse, the Survey and Evaluation Research Laboratory (SERL) applied the proportional allocation approach to estimate the response rate (Ellis, 2000).

However, each of the methods used has its limitations, and as Smith (2003) states "*At present none can be considered a gold standard for calculating "e"*". Furthermore, there is no evidence to show that applying any of these methods identifies the eligible cases among cases of unknown eligibility in order to drop ineligible cases and calculate non-response weights appropriately. The focus has, instead, been on calculating survey response rates. Moreover, all of these methods have mainly been implemented in cross-sectional studies. In longitudinal surveys, besides the calculation of the eligibility proportion to compute response rates, the investigation of an alternative method that takes the longitudinal aspect of eligibility into account and utilizes information from inside and outside the survey, could assist in identifying eligible cases, and may therefore result in successful weighting.

**7. Methodology**

The data used in this paper are from waves 1 to 18 of the BHPS, covering the years 1991 to 2008. The analysis was done at the individual level and was restricted to the original sample of BHPS and respondents aged 16 or older. The estimation was implemented in STATA.

The sample –if representative- is a smaller image of the population. Thus, rates of phenomena in the populations should be equal to those of the same phenomena in the sample, under expectation. One concern of this paper is to compare the proportion of eligibility in the sample with this in the population. This will be done for categories of respondents (by gender and age group). An eligibility rate for a given category in the sample will be compared with the eligibility rate in the equivalent category in the population.

Eligibility in the sample is either known or unknown. If both eligible respondents and those with unknown eligibility in the sample are considered as eligible respondents, the calculated eligibility rates in categories with larger proportions of unknown eligibility cases in the sample will be higher than the rates in the equivalent categories in the population (because some of the unknown eligibility cases in the sample may not be eligible). Accordingly, in such categories, non-response weights will not correct the sample proportions in a manner that make them represent their equivalent proportions in the population unless the weights are adjusted. Therefore, an adjustment factor is needed for the categories in the sample to modify the relevant weights so that weighted proportions in the sample represent the equivalent proportions in the population. This adjustment factor can be worked out based on the comparison between eligibility proportions in the sample and the population.

Letting $Þ_P$ and $Þ_S$ be the eligibility proportion in the population and eligibility proportion in the sample respectively. If unknown eligibility cases in the sample are considered eligible, then

$Þ_S \geq Þ_P$ (7)

if

$ad * Þ_S = Þ_P$ (8)

Then *ad* is a fraction.

*ad* is the adjustment factor that equalises the eligibility proportions in the sample and the population. From equation 8

$ad = Þ_P / Þ_S$ (9)

In other words, the adjustment factor is the ratio of the eligibility proportion in the population to the eligibility proportion in the sample. This ratio takes a value between 0 and 1 since its denominator is larger, and should be used to adjust non-response weights. The lesser differences between eligibility proportions in the sample and the population the closer the value of *ad* to 1 and hence the smaller the adjustment on weighting will be and vice versa. For example, in a given population, assuming that eligibility rate is 80% (i.e. 20% ineligible) and a representative sample from this population shows that 50% known eligible, 10% known ineligible and 40% unknown eligibility. If the weighting assumes that those with unknown eligibility are eligible, weights will increase the values of the responding sample to represent 90% eligibility in the population (50% + 40%) while it should only represent 80% (the eligibility rate in the population). Thus, the weights need to be adjusted so that they only represent the eligible respondents in the population. Based on equation 9, an adjustment of 8/9 is needed in this example. Section 11 explains in details how this adjustment factor was made for the BHPS sample.

While proportions of eligibility in the sample can be calculated from the data, proportions in the population should be estimated using information from external source. Note that eligibility in the BHPS is met if the sample member is alive and living in the United Kingdom (UK). Thus, eligibility proportions are actually the survival proportions. Therefore, this paper used information from The Office for National Statistics (ONS) on survival/mortality rates for the population in England and Wales. These were used to estimate proportions of eligibility in the population. However, the original sample of

BHPS includes respondents from England, Wales and Scotland. Nonetheless, ONS does not publish annual mortality rates for the population in England, Wales and Scotland together (to match the target population of BHPS original sample). Thus, in this paper it was assumed that mortality rates for the population in England and Wales are the same as those for the population in England, Wales and Scotland. Based on this assumption, survival/mortality rates by ONS are considered to be for the target population of the BHPS original sample.

## 8. Calculating proportions of survivals in the sample

The BHPS data provide details about the contact outcome at every wave through a variable named '*individual interview outcome*' (IVFIO). The main categories of this variable indicate whether the outcome is full interview, proxy interview, telephone interview, refusal, in institution, non-contact, other non-interview, out of scope or dead. Thus, this setting leads to three categories of respondents in terms of eligibility status:

(a) Eligible respondents (ER): these are respondents who gave a full interview, proxy interview, telephone interview, or refusal.
(b) Ineligible respondents (IR): these are respondents who are reported dead, in institution or out of scope.
(c) Respondents with unknown eligibility (RUE): these are mainly respondents who were not contacted.

Proportions of category (a) through to (c) were calculated by gender and age groups[4] and are shown in table (1). For example, 44.36% of the males aged between 16 and 19 who joined BHPS in its first wave (1991) are still eligible in 2008 while only 1% of them are known to be ineligible and the remaining 54.64% are of unknown eligibility. Of particular interest, for both males and females, none of the respondents aged 85 or over at the start of the survey (1991) are known to be eligible in 2008 (0.00%). However, only 86.76% and 84.27% males and females (respectively) aged 85 or over are actually reported as being ineligible, and the rest are of unknown eligibility. Those who started the survey at

[4] The age groups used were 16-19, 20-24, 25-34, 35-44, 45-54, 55-64, 65-74, 75-84, 85 and over. These are the same age groups used by the ONS to publish mortality rates for England and Wales. With the exception of the first age group as ONS provides mortality rates from the age group 15-19. However, the analysis here is restricted to those aged 16+.

the age of 85 or over and are of unknown eligibility 18 years later, are likely to be ineligible.

For all ages, amongst the males, 42.27% are eligible respondents, 15.79% are ineligible and 41.94% are of unknown eligibility. For the females, 48.28% are eligible respondents, 15.42% are ineligible and 36.3% are of unknown eligibility. This shows that the known eligibility rate for females is higher than for males, meaning that the unknown eligibility rate among females is lower than among males. The sample also shows interesting distributions of gender and age groups in terms of eligibility status. For instance, for both males and females, eligibility rate goes up as age increases, but reaches its peak in the age group 35-44 (51.03% M; 58.59% F) before it starts declining as age increases, to reach its nadir at the age of 85 or over (0.00% M; 0.00% F). This pattern of eligibility is reasonable, since mortality is higher amongst older age groups. Furthermore, ineligibility rate increases constantly with age. However, it increases faster for males than females and reaches its peak at age 85 or over (86.67% M; 84.27% F). This finding is consistent with the literature on mortality, which establishes that death rates are always higher amongst older age groups (Singh-Manoux *et al*, 2008) and life expectancy among females is higher than among males (Dr Foster, 2004). Additionally, for both males and females, the unknown eligibility rate decreases with increasing age, and its highest rate is in the age group 16-19 (54.64% M; 50.90% F).

**Table 1** Proportions of eligible, ineligible and unknown eligibility cases in the original sample (1991) of BHPS in 2008 by gender and age groups

| | **16-19** | **20-24** | **25-34** | **35-44** | **45-54** | **55-64** | **65-74** | **75-84** | **85+** | **All ages** |
|---|---|---|---|---|---|---|---|---|---|---|
| **Male** | | | | | | | | | | |
| Eligible | 44.36% | 41.74% | 48.63% | 51.03% | 48.64% | 43.23% | 21.09% | 3.21% | 0.00% | 42.27% |
| Ineligible | 1.00% | 1.06% | 1.55% | 4.42% | 10.00% | 26.10% | 54.88% | 79.12% | 86.67% | 15.79% |
| UE | 54.64% | 57.20% | 49.82% | 44.55% | 41.36% | 31% | 24.03% | 17.67% | 13.33% | 41.94% |
| **Female** | | | | | | | | | | |
| Eligible | 49.10% | 57.62% | 58.52% | 58.59% | 57.35% | 49.23% | 26.48% | 4.75% | 0.00% | 48.28% |
| Ineligible | 0.00% | 0.21% | 1.58% | 2.55% | 7.53% | 19.60% | 43.84% | 69.13% | 84.27% | 15.42% |
| UE | 50.90% | 42.17% | 39.90% | 38.86% | 35.12% | 31.17% | 29.68% | 26.12% | 15.73% | 36.30% |

* UE refers to unknown eligibility.

However, whilst weighting, respondents of unknown eligibility (RUE) must be decided as either alive or dead (i.e. eligible or ineligible). For the purpose of the analysis in this paper, RUE were assumed to be eligible[5]. Therefore, eligible respondents (ER) and RUE were combined to calculate the proportions of survival in the sample. These proportions were calculated by age groups and gender and are shown with their corresponding proportions in the population in table (2).

## 9. Calculating proportions of survival from population information

ONS publishes annual mortality rates (ONS, 2014) by gender and 10-year age groups (with the exception of the first two age groups: 15-19 and 19-24) for the population in England and Wales (see these rates in appendix B and D). Using this information, the survival proportions by gender and age groups were calculated as follows:

- A rate in a given year for a certain category of age group and gender gives the probability that an individual in that category will die in the following year, given that he or she is alive in the current year. Subtracting the rates from 1 gave the survival probabilities in the following year (see these rates in appendix C and E). Rates for the years 1991 to 2007 were used to correspond to the 17 waves following the first wave (1992-2008). To simplify the calculations, the rates were expanded by a single year of age from age 16 to age 112[6], by giving each age in the same age group the survival rate for that age group. Considering the original sample at wave 1 in 1991, the survival probabilities by the end of wave 18 (2008) were then calculated for each single year of age, by computing the product of the rates in the consecutive 17 years for each age. These probabilities were then regrouped into the original age groups by taking the average. These proportions are shown with their corresponding proportions from the sample in table (2).

[5] In the standard approach of weighting, RUE are usually assumed to be eligible, especially if weighting is done through a model-based method.
[6] The oldest participant in the original sample of BHPS was aged 96. By the end of the 18 waves, this person would have been aged 112 if they were still alive. Thus it was necessary to expand rates up to age 112, in order to be able to calculate the survival probabilities for all respondents.

Table 2 shows the proportions of survival in the sample and the population during the period 1991 to 2008 by gender and age group. For example, 2.73% of the males aged 85 or over in the population in 1991 are expected to still be alive by 2008 while the corresponding proportion of this in the sample is 13.33%. Overall, the survival proportions in the sample are larger than those in the population. This confirms the hypothesis that there is under reporting of death in the sample. Nevertheless, the differences between the survival proportions in the population and the sample are not worryingly large, and suggest only a small number of unreported deaths. The largest differences for both gender types are registered for the age groups 65-74, 75-84 and 85 or over. Interestingly, respondents in the oldest age group (85 or over) show smaller differences between the survival proportions in the population and the sample than the age groups 65-74 and 75-84. This may be because individuals aged 85 or over are less likely to be single-person households (at old age people usually need to be cared for either through family members or by professional carers) than those in the age groups 65-74 and 75-84 (at these ages individuals may still live independently as single-person households). Hence, death at age 85 or over has more chance of being reported than death in the two age groups, 65-74 and 75-84.

**Table 2** Calculated survival proportions in 2008 in the population and assumed proportions in the original sample of BHPS by gender and age group

| | **16-19** | **20-24** | **25-34** | **35-44** | **45-54** | **55-64** | **65-74** | **75-84** | **85+** |
|---|---|---|---|---|---|---|---|---|---|
| **Male** | | | | | | | | | |
| Population | 98.50% | 98.16% | 97.25% | 93.69% | 84.08% | 61.39% | 28.03% | 6.07% | 2.73% |
| Sample | 99.00% | 98.94% | 98.45% | 95.58% | 90.00% | 73.90% | 45.12% | 20.88% | 13.33% |
| **Female** | | | | | | | | | |
| Population | 99.36% | 99.10% | 98.35% | 95.66% | 89.74% | 73.31% | 40.58% | 10.86% | 5.43% |
| Sample | 100.00% | 99.79% | 98.42% | 97.45% | 92.47% | 80.40% | 56.16% | 30.87% | 15.73% |

*Entries are the survival proportions in 2008 for those who were alive in 1991. The sample proportions include both those who are known to be alive (eligible) and those with unknown status of survival (unknown eligibility).

Before describing how the adjustment factor was calculated for each category of gender and age group, the next section provides details of the creation of non-response weights.

## 10. Weights creation

Non-response weights were created based on respondents in the 18 waves using a model-based method (the standard approach). The response propensity was modelled using logistic regression. The dependent variable was a categorical variable with two categories indicating whether a respondent participated in all of the 18 waves or not. Although the relationship between the response propensity and weighting variables may vary among waves, one way of creating non-response weights is to ignore the effect of time-varying variables (Sadig, 2011) and use variables from wave 1. This way guarantees availability of information for both respondents and non-respondents in the 18 waves, which is essential in modelling the response propensity. In this paper, a large combination of continuous and categorical variables from wave 1 was used to estimate the model. These variables[7] were selected to represent three categories of variables: interview/interviewer condition/characteristics (e.g. interviewer's sex and length of interview), household characteristics (e.g. household size and household type) and individual characteristics (e.g. age, sex and savings). These variables are commonly used in the analysis of non-response (Urig, 2008; Nicoletti and Peracchi, 2005; Nicoletti and Buck, 2004).

$$\text{Logit}\,(R_i) = f\left(\textstyle\sum_k I_k + \sum_{jk} H_{jk} + \sum_{ijk} D_{ijk} + \varepsilon_i\right) \quad (10)$$

Where:

$R_i \equiv$ Responding Status at the 18 waves.

$$R_i = \begin{cases} 1, & if\ unit\ i\ responded\ in\ all\ of\ the\ 18\ waves \\ 0, & otherwise \end{cases} \quad (11)$$

$I_k \equiv$ Interview/Interviewer condition/characteristics.

$H_{jk} \equiv$ Household characteristics.

$D_{ijk} \equiv$ Individual characteristics.

$\varepsilon_i \equiv$ Error term.

---

[7] These variables are sex, age, ethnic group, region, health status, household size, presence of children in household, housing tenure, income, number of people age 75+ in the household, type of household, number in employment in household, education, employment status, savings, debt, type of accommodation, financial situation, socioeconomic group, number of weekly working hours, number of weekly overtime hours, work location, smoking status, car ownership, number of own children in the household, presence of others during interview, interviewer ID, interviewer sex and length of interview.

Table 3 shows the results of modelling the response propensity in the 18 waves through a logistic regression model. The table presents odds ratios. Regarding the tendency to respond in the period of 18 waves, most variables have a significant effect on response propensity. For example, females are more likely to respond than males ($\hat{b}$ = 1.393, $p <$ 0.001). In addition, respondents from a white ethnic origin tend to participate more than respondents from other ethnic origins ($\hat{b}$ = 1.652, $p < 0.01$). Bad health however, appears to be negatively correlated with the response propensity ($\hat{b}$ = 0.839, $p < 0.05$), indicating that individuals with better health have a greater tendency to respond. Also, homeowners are more likely to respond than non-homeowners ($\hat{b}$ = 1.91, $p < 0.05$), while increase in age is negatively associated with survey participation ($\hat{b}$ = 0.994, $p < 0.05$).

Additionally, the response propensity in the 18 waves is also significantly correlated (positively or negatively) with other variables. These are household type, education, employment status, savings ownership, housing tenure, sex of interviewer and region.

However, other factors, such as household size and the presence of children in a household do not show a significant effect on response propensity.

**Table 3** Logistic regression model of the response propensity in the 18 waves.

| | Model of response propensity based on variables from wave1 |
|---|---|
| **Female** | 1.393*** |
| **White** | 1.652** |
| **Bad health** | 0.839* |
| **Household size** | 0.975 |
| **Household with dependent children** | 1.084 |
| **Home owner** | 1.191* |
| **Age** | 0.994* |
| **Annual income/1000** | 1.004* |
| **Household has no member aged 75 or over** | 0.890* |
| **1 or 2 persons in employment in household** | 0.855* |
| **3 persons or more are in employment in household** | 0.866 |
| **Single person household** | 0.689** |
| **Has GCE qualification or above** | 1.375*** |
| **Employed** | 1.367*** |
| **Having a second job** | 0.772* |
| **Has no savings** | 0.818* |
| **Living in a flat** | 0.635*** |
| **Based in business premises** | 0.948 |
| **Living in a bedsit** | 0.503 |
| **Living in other housing type** | 1.376 |
| **Interviewed by a female** | 1.462** |
| **Lives in South-East** | 1.174 |
| **Lives in South-West** | 1.120 |
| **Lives in East Anglia** | 1.290 |
| **Lives in the Midlands** | 1.224 |
| **Lives in the North** | 1.325* |
| **Lives in Wales** | 1.321 |
| **Lives in Scotland** | 0.902 |
| **N** | 10248 |
| **Pseudo $R^2$** | 0.087 |

Note: The entries are odd ratios. The reference categories of the categorical independent variables in the model are male, non-white, good health, household with no children, not a home owner, there is at least one person aged 75+ in HH, no one is in employment in HH, multi-person household, does not have a GCE or higher degree, unemployed, has a second job, has savings, living in a house, interviewed by a male and lives in London respectively. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Accordingly, weights were calculated as the inverse of the predicted probabilities from the model for each case in the sample.

$$w_i = 1/r_i \tag{12}$$

Where:

$w_i$ ≡ Case i initial non-response weight.

$r_i$ ≡ Predicted probability of response for case i from the model.

The set of initial non-response weights was then multiplied by the BHPS set of design weights ($w_D$). However, in the BHPS, the design weights were combined with a set of weights that compensate for wave 1 non-response. Thus, $w_D$, which is provided by BHPS, adjusts for the difference in the selection probabilities and compensate for wave 1 non-response simultaneously.

$$w_{NRi} = w_i * w_{Di} \quad (13)$$

Where:

$w_{NRi}$ ≡ Case *i* non-response weight.

$w_i$ ≡ Case *i* initial non-response weight.

$w_{Di}$ ≡ Case *i* design weight (combination of design weight and wave 1 non-response weight).

## 11. Adjustment factor

Theoretically, survival proportions in the sample should equal survival proportions in the population from which the sample is drawn if the sample is correctly representing the study population. However, since RUE in the sample were considered eligible (alive), proportions of death in the sample were under calculated. Consequently, this led to over calculating survival proportions in the sample. Thus, survival proportions calculated from the sample are higher than survival proportions estimated using the population information. These differences are due to not being able to identify some of the deaths in the sample. Based on the differences between survival proportions in the sample and the population, an adjustment factor that varies by gender and age groups was calculated (as shown in equations 7 to 9) as follows:

- For each age group and both genders, the adjustment factor was calculated as the ratio of proportion of survived respondents in the population to the proportion of survived respondents in the sample, under the assumption that all

RUE are eligible. Since survival proportions in the sample are bigger than their correspondence proportions in the population, this ratio takes a value between zero and one. For instance, if, in any category of gender and age group in the sample, large amount of death is identified, survival proportions in the sample and the population will be approximately equal. Hence, the adjustment factor will take a value close to one and consequently lead to a small effect or no effect at all on the relevant weight. Contrary, if, large amount of death is not identified, survival proportion in the sample will take bigger value than it is in the population and accordingly the adjustment factor will take a value close to zero. Thus, the effect of the relevant weight will be reduced to represent lesser units from this category due to the low likelihood of eligibility. Hence, the adjustment factor represents the likelihood of survival for each category of gender and age group.

$$ad_{jk} = Þ_{Pjk}/Þ_{Sjk} \tag{14}$$

Where:

$ad_{jk}$ ≡ Adjustment factor for the category of age group *j* and gender *k*.

$Þ_{Pjk}$ ≡ Population proportion of survived respondents in age group *j* and gender *k* (probability that a respondent in age group *j* and with gender *k* is alive by the end of wave 18).

$Þ_{Sjk}$ ≡ Sample proportion of assumed survived respondents in age group *j* and gender *k*.

Thus, the final non-response weight for case *i* which falls in the category of the age group *j* and gender *k*, was calculated as the product of case *i* non-response weight and the adjustment factor in the category of age group *j* and gender *k*.

$$w_{FNRijk} = w_{NRijk} * ad_{jk} \tag{15}$$

Where:

$w_{FNRijk}$ ≡ Final non-response weight for case *i* in the category of age group *j* and gender *k*.

$w_{NRijk}$ ≡ Non-response weight for case *i* in the category of age group *j* and gender *k*.

$ad_{jk}$ ≡ Adjustment factor for age group *j* and gender *k*.

The adjustment factor should modify the weight of respondents in a given category according to the likelihood of survival by the end of the 18-wave period in that category. Accordingly, respondents in older age groups are more likely to receive smaller size weights than those in younger age groups, since older respondents have a lower chance of survival after 18 years. Thus, any potentially dead (ineligible) respondents who were initially included when weights were calculated will be removed from the adjusted weights depending on the survival likelihood in their category.

For example, table 2 shows that for women aged 85 or over, the survival proportions in the population and the sample are 5.43% and 15.73% respectively. Thus, the adjustment factor for respondents in this category can be calculated, based on equation 14, as follows:

$ad_{85w}$= 5.43/15.73

$ad_{85w}$ = 0.35

This means that, while the sample shows that the survival proportion of women aged 85 or over at the beginning of BHPS (1991) in 2008 is 15.73%, the adjustment factor shows that only 35% of this proportion is likely to be alive in 2008. Accordingly, this will be used to adjust the weight for women in the responding sample who fall in this category. Therefore, the final non-response weight for women in this category can be calculated based on equation 15 As follows:

$w_{FNR,85\,w}$= $w_{NR,85w}$*0.35

Similarly, for women in the age group 16-19, the table 2 indicates that their survival proportions in the population and in the sample are 99.36% and 100% respectively. Thus, the adjustment factor and final nonresponse weight for this category are

$ad_{16-19w}$= 99.36/100

$ad_{16-19w}$ = 0.99

$w_{FNR,16-19\,w} = w_{NR,16-19w}$*0.99

Consequently, the weights for responding women in the age group 85 or over will be reduced more than the weights for responding women in the age group 16-19 since for the latter the extent to which the likelihood of survival is over estimated is smaller ($ad_{16-19w} = 0.99 > ad_{85w}$= 0.35).

Table 4 shows all of the calculated adjustment factors. These are presented for each category of gender and age group. The table was calculated based on the information in table 2 and equation 14. As can be noticed from the table, for both men and women, the adjustment factor will have very small effect (mostly no effect) on weights for respondents aged between 16 and 64 ($0.83 \leq ad \leq 0.99$). However, for both men and women, the factor shows a drastic change for the ages above 64 implying more effect of the adjustment factor on the weights for respondents aged 65 or above ($0.20 \leq ad \leq 0.62$). Thus, adjusted weights are expected to have more impact on estimates related to older respondents (aged 65 or above) than on estimates based on their younger counterparts (aged between 16 and 64).

**Table 4** Calculated adjustment factors for the categories of gender and age groups.

| | **16-19** | **20-24** | **25-34** | **35-44** | **45-54** | **55-64** | **65-74** | **75-84** | **85+** |
|---|---|---|---|---|---|---|---|---|---|
| **Male** | | | | | | | | | |
| *ad* | 0.99 | 0.99 | 0.99 | 0.98 | 0.93 | 0.83 | 0.62 | 0.29 | 0.20 |
| **Female** | | | | | | | | | |
| *ad* | 0.99 | 0.99 | 0.99 | 0.98 | 0.97 | 0.91 | 0.72 | 0.35 | 0.35 |

*Entries are the adjustment factors (*ad*). These are calculated based on equation 2.7 and information in table 2.2.

## 12. Assessment of the effect of the adjustment factor

Assessing the effect of the adjustment factor was done by conducting different types of statistical analyses using the two sets of weights (adjusted and unadjusted) and comparing the results. The analyses include calculating the coefficient of variation (CV) for adjusted and unadjusted weights as well as applying other data analysis techniques. The latter includes producing descriptive statistics and estimating panel data models (multivariate analysis) using adjusted and unadjusted weights.

Since the effect of the adjustment factor is expected to be different for respondents aged 65 or older than those aged below 65, the analysis was done separately for respondents aged: between 16 and 64, 65 or older (65+) and for the whole sample.

**12.1 Coefficient of variations (CV)**

CV is a measure of dispersion. It is defined as a ratio of the standard deviation to the mean. Higher values of CV indicate larger variability in the data and vice versa. Since the calculation of CV does not directly involve the measurement unit (only uses the standard deviation and the mean), it can be used to compare the variability of two different variables. Thus, calculating and comparing CV for the adjusted and unadjusted weights will show whether the adjustment factor reduces the variance of the weights.

Table 5 shows the standard deviation, mean and CV for the unadjusted and adjusted weights. The results are presented separately for the full sample, respondents aged 16 to 64 and respondents aged 65+. As can be seen from the table, for both the whole sample and respondents aged 65+, the CVs for the unadjusted weights are higher than the CVs for adjusted weights ($CV_{un.\ sample}$= 61% > $CV_{ad.\ sample}$= 54% and $CV_{un.\ 65+}$= 71% > $CV_{ad.\ 65+}$= 62%). These results indicate that, for the whole sample and respondents aged 65+, there is less variability in the sets of adjusted weights than in the unadjusted weights. In other words, the adjustment factor reduces the sizes of the largest weights amongst the weight values of respondents aged 65+ and as a result the variance of the weights is reduced both for respondents aged 65+ and the full sample. Based on this result, one may expect the standard errors of estimates to differ if adjusted weights are used in the estimation instead of unadjusted weights.

As for weights concerning respondents aged between 16 and 64, the CV for unadjusted weights ($CV_{un.\ 16-64}$= 67%) is only 1% higher than the CV for adjusted weights ($CV_{ad.\ 16-64}$= 66%).

**Table 5** Standard deviations, means and coefficients of variation for adjusted and unadjusted weights.

| | All respondents aged 16+ | | Respondents aged 16 to 64 | | Respondents aged 65+ | |
|---|---|---|---|---|---|---|
| **SHS** | **Unadjusted weights** | **Adjusted weights** | **Unadjusted weights** | **Adjusted weights** | **Unadjusted weights** | **Adjusted weights** |
| **Std.Dev** | 1.39 | 1.03 | 1.21 | 1.07 | 3.69 | 1.13 |
| **Mean** | 2.28 | 1.92 | 1.81 | 1.61 | 5.19 | 1.81 |
| **CV** | 0.61 | 0.54 | 0.67 | 0.66 | 0.71 | 0.62 |

- Std.Dev is the standard deviation. CV is calculated as the ratio of Std.Dev to the Mean (CV=Std/Mean).

Turning to the descriptive and multivariate analyses, this was carried out to investigate the subjective health status (SHS) in the BHPS. The following two sections summarise these analyses.

## 12.2 Descriptive statistics

In the BHPS, SHS is measured by asking respondents every year to rank their own health as excellent, good, fair, poor or very poor. The proportions of respondents in each of these categories are calculated using adjusted and unadjusted weights and are displayed in table 6. The weighted proportions are presented for the whole sample, respondents aged 16 to 64 and respondents aged 65+ separately. Also, the table presents 95% Confidence Intervals (CI) for the unadjusted proportions. CI's are used here to assess whether the adjusted proportions are within the calculated CI's of the relevant unadjusted proportions. If any adjusted proportion falls out of the CI of its corresponding unadjusted proportion, this may be taken as an indication of a significant difference between the two proportions, and hence clear effect of the adjustment factor.

Focussing on respondents aged 16 to 64 first, adjusted and unadjusted weights produced similar proportions across the categories of SHS and none of the adjusted proportions seem to be out of the CI's of the unadjusted proportions. Thus, this result indicates no significant differences between adjusted and unadjusted proportions for those aged 16 to 64. In other words, there is no evidence of considerable differences between adjusted and unadjusted weights for the age group 16 to 64.

Turning to all respondents aged 16 and over, there is one significant difference relating to the category 'very poor health'. In this category, the adjusted proportion (1.12%) falls out of the CI (1.55%-2.27%) of the unadjusted one (1.91%) indicating a significant difference due to making the adjustment.

As for those aged 65+, there are two significant differences here. First, using unadjusted weights, the proportion of those who reported poor health is (2.22%) with 95% CI of (1.45%-2.99%), meanwhile using the set of adjusted weights yields a lower percentage (1.40%) that is out of the range of the calculated CI. Second, proportions of those who reported very poor health are also significantly different. With adjusted and unadjusted weights, these proportions are 0.59% and 2.25% respectively. However, the adjusted proportion is lower than the lower limit of the CI of the unadjusted proportion (1.48%-3.02%).

Based on these results, it can generally be concluded that, for the whole sample and those aged 65+, adjusted weights produce different proportions than unadjusted weights; meanwhile, for those aged 16 to 64 adjusted and unadjusted weights result in similar proportions. This maybe explained by the fact that, when adjusting the weights, the effect of the adjustment factor on the weights of respondents who aged 65+ is large and in a downwards direction (the adjusted weights are smaller), as these are the respondents in poorest health. Meanwhile, the adjustment factor does not change the weights of respondents aged between 16 and 64 considerably.

**Table 6** Weighted proportions across the categories of subjective health status.

| | **All respondents aged 16+** | | **Respondents aged 16 to 64** | | **Respondents aged 65+** | |
|---|---|---|---|---|---|---|
| **SHS** | **Using unadjusted weights** | **Using adjusted weights** | **Using unadjusted weights** | **Using adjusted weights** | **Using unadjusted weights** | **Using adjusted weights** |
| **Excellent** | 29.37% (28.16%-30.58%) | 30.28% | 29.60% (28.19%-31.01%) | 30.39% | 29.42% (27.05%-31.79%) | 30.37% |
| **Good** | 45.58% (44.26%-46.90%) | 45.95% | 46.08% (44.54%-47.62%) | 46.12% | 48.02% (45.42%-50.62%) | 49.57% |
| **Fair** | 16.67% (15.68%-17.66%) | 16.48% | 15.60% (14.48%-16.72%) | 15.32% | 18.09% (16.02%-20.16%) | 18.07% |
| **Poor** | 6.47% (5.82%-7.12%) | 6.17% | 6.93% (6.14%-7.72%) | 6.65% | 2.22% (1.45%-2.99%) | 1.40%* |
| **V. Poor** | 1.91% (1.55%-2.27%) | 1.12%* | 1.79% (1.38%-2.20%) | 1.52% | 2.25% (1.48%-3.02%) | 0.59%* |

Note: The numbers in brackets are 95% confidence intervals. * indicates that the adjusted proportion falls out of the CI of its corresponding unadjusted proportion (i.e. there is a significant difference between the adjusted and unadjusted proportions).

### 12.3 Multivariate analysis

The multivariate analysis was carried out in order to investigate factors affecting SHS. This was done by estimating three panel data models (using the whole sample, respondents aged 16 to 64 and respondents aged 65+). To detect the potential effect of the adjustment factor on estimates, each model was estimated two times by using unadjusted and adjusted weights. The comparison between models using unadjusted weights and models using adjusted weights revealed how adjusting the weights would affect the estimation.

In this analysis, the five categories of SHS (excellent, good, fair, poor and very poor) were reorganised. The first three categories were combined into one category (good health status) and the last two were combined into another category (poor health status). Accordingly, SHS became a categorical variable with two categories, indicating whether the respondent has good or poor health status. This variable was used as the dependent variable in the analysis.

$$SHS_i = \begin{cases} 1, & if\ case\ i\ has\ a\ good\ health\ status. \\ 0, & if\ case\ i\ has\ a\ poor\ health\ status. \end{cases} \qquad (16)$$

Where

$SHS_i$ $\equiv$ Subjective health status.

The independent variables are sex, ethnicity, age, financial situation, income, marital status, type of household, energy compared to average at the same age, smoking status and number of visits to GP since last year (NVGP). These variables are known for their effect on health status and were used in prior research of self-assessed health in the BHPS (for example Jones *et al*, 2004).

Using data from 18 waves of the BHPS, allowed the estimation of a random effects logistic regression model. However, as described earlier, the model was estimated separately for the whole sample, respondents aged between 16 and 64 and respondents aged 65+. Moreover, the model for each group was estimated with adjusted and unadjusted weights separately.

The analysis was done at the individual level in STATA. The data was introduced as a panel data set to allow the consideration of multiple observations per person and therefore the application of panel data modeling. BHPS sample is not a simple random sample; it was rather selected through a complex sampling design that involved clustering and stratification. However, STATA –like many statistics software– does not support the identification of clustering and stratification while estimating a panel data model. Thus, it was not possible to take clustering and stratification into account in this analysis. This may lead to under estimating the standard errors of estimates. However, although this may not lead to precise standard errors, any differences between the estimates produced using adjusted and unadjusted weights will be due to adjusting the weights since the modeling strategy is held constant. Furthermore, significance levels of regression coefficients were interpreted conservatively and only highly significant coefficients were considered.

Table 7 shows the results of the logistic regression models. The table presents odds ratios. Although the models capture significant relationships between subjective health status and most of the factors included in the analysis, the importance of these factors differs between those aged between 16 and 64 and those aged 65+. For example, in the time period 2003-2008, individuals indicate worse health status than in 1991-1996. However, this is only significant for respondents aged 65+ ($\hat{b}_{16-64,1}$= 0.714, $p > 0.05$ and $\hat{b}_{16-64,2}$= 0.689, $p > 0.05$; $\hat{b}_{65+,1}$= 0.561, $p < 0.01$and $\hat{b}_{65+,2}$= 0.523, $p < 0.01$). However, for the purpose of this paper, the focus is rather on the comparison between the results from unadjusted and adjusted weights. Turning to this, the result of the comparison can be summarised in what follows:

First, focussing on models for respondents aged between 16 and 64, no differences are found between estimates in the model using unadjusted and the model using adjusted weights. In other words, adjusting the weights for respondents aged 16 to 64 does not affect estimates. This result is expected since the adjustment factors in all the age groups under 65 have values close to 1; hence they do not change the weighting much.

Second, as regard to the models of the whole sample and those aged 65+, the significance levels of four estimates were increased when adjusted weights were used to estimate the model. For the whole sample these estimates are related to the following factors:

- '*financially struggling*' ($\hat{b}_1$= 0.477, $p < 0.05$; $\hat{b}_2$= 0.512, $p$ <0.01).
- '*single*' ($\hat{b}_1$= 0.498, $p < 0.05$; $\hat{b}_2$= 0.476, $p < 0.01$).
- '*has less energy as average at their age*' ($\hat{b}_1$= 0.901, $p < 0.05$; $\hat{b}_2$= 0.889, $p <$ 0.01).
- '*single-person household*' ($\hat{b}_1$= 0.475, $p < 0.05$; $\hat{b}_2$= 0.433, $p < 0.01$).

As for those aged 65+ the estimates are related to:

- 'age' ($\hat{b}_1$= 0.610, $p < 0.05$; $\hat{b}_2$= 0.587, $p$ <0.01).
- '*financially struggling*' ($\hat{b}_1$= 0.578, $p < 0.05$; $\hat{b}_2$= 0.522, $p$ <0.01).
- '*single*' ($\hat{b}_1$= 0.651, $p < 0.05$; $\hat{b}_2$= 0.628, $p$ <0.01).
- '*single-person household*' ($\hat{b}_1$= 0.457, $p < 0.05$; $\hat{b}_2$= 0.433, $p$ <0.01).

As expected, these results suggest no changes in the significance level in the model of those aged 16-64. Additionally, they show that the adjustment factor affects the weights of those aged 65+ mostly and as a result it changes the weights for the sample as a whole. Adjusting the weights results in reducing the values of large weight and therefore minimising the weights' variance. Consequently, the adjusted weights (with less variability) reduce the standard errors of some estimates and hence they become more significant.

Regarding the bias, this was assessed by checking if the coefficients estimated through adjusted weights, falls out of the 95% CIs of the equivalent coefficients in models with unadjusted weights. For ease of exposition, these 95% CIs are not displayed in table 2.7. However, across the three sets of respondents, all of the coefficients in the adjusted models fall within 95% CI of their corresponding coefficients in the equivalent unadjusted models. This means that we have no evidence that our adjustment have reduced the potential bias from the estimated coefficients in the models.

To sum up, in general, estimates constructed based on unadjusted weights are similar to estimates based on adjusted weights. However, some of the estimates increased in their significance level when constructed using adjusted weights. This is particularly in estimates related to respondents aged 65+ as the likelihood of survival (represented by the adjustment factor) in this age group is lower than in their counterparts ages (16 to 64). Corollary, estimates constructed using the whole sample, may also be affected if constructed using adjusted weights. In this analysis, some of the estimates produced based on the whole sample, appeared more significant when constructed using adjusted weights.

As for bias, the multivariate analysis does not provide clear evidence that adjusted weights result in bias reduction. Thus, it cannot be asserted that the adjustment has resulted in bias reduction in our regression coefficients.

Finally, based on this analysis, it can be concluded that most of the ineligible (dead) respondents amongst respondents of unknown eligibility in the BHPS sample are within the age group 65+ rather than within the ages 16 to 64. Thus, if the weighting is not controlled for those aged 65+, the estimated standard errors of some estimates might be misleading.

**Table 7** Random effects logistic regression models for the determinants of subjective health status.

| | All respondents aged 16+ | | Aged between 16 and 64 | | Aged 65+ | |
|---|---|---|---|---|---|---|
| | **Using unadjusted weights** | **Using adjusted weights** | **Using unadjusted weights** | **Using adjusted weights** | **Using unadjusted weights** | **Using adjusted weights** |
| **Year 1997 to 2002** | 0.862 | 0.791 | 0.804 | 0.812 | 0.732 | 0.758 |
| **Year 2003 to 2008** | 0.903 | 0.921 | 0.714 | 0.689 | 0.561*** | 0.523*** |
| **Female** | 1.079 | 1.038 | 0.788 | 0.721 | 1.098** | 1.087** |
| **White** | 1.121 | 1.047 | 1.098 | 1.126 | 1.079** | 1.068** |
| **NVGP** | 0.735*** | 0.699*** | 0.813*** | 0.791*** | 0.939*** | 0.951*** |
| **Age** | 0.610** | 0.587*** | 0.690** | 0.637** | 0.578*** | 0.529*** |
| **Financially okay** | 1.301 | 1.241 | 1.260 | 1.199 | 0.496 | 0.563 |
| **Financially Struggling** | 0.578** | 0.522*** | 0.715** | 0.701** | 0.477** | 0.512*** |
| **Annual income/1000** | 1.014** | 1.047** | 1.094** | 1.068** | 1.030*** | 1.011*** |
| **Single** | 0.651** | 0.628*** | 0.658** | 0.620** | 0.498** | 0.476*** |
| **Widow** | 0.459 | 0.486 | 0.708 | 0.734 | 0.405** | 0.417** |
| **Divorced or separated** | 0.794** | 0.768** | 0.532** | 0.510** | 1.031** | 1.020** |
| **Smoker** | 0.886*** | 0.852*** | 0.762*** | 0.723*** | 0.861*** | 0.823*** |
| **Has GCE qualification or above** | 1.092 | 1.076 | 1.013 | 1.116 | 0.724 | 0.715 |
| **Has same energy as average at their age** | 1.187 | 1.206 | 1.203 | 1.192 | 0.588** | 0.539** |
| **Has less energy as average at their age** | 0.784*** | 0.723*** | 0.885*** | 0.817*** | 0.901** | 0.889*** |
| **Single-person household** | 0.457** | 0.433*** | 0.340** | 0.311** | 0.475** | 0.433*** |
| **N** | 5019 | 5019 | 3758 | 3758 | 1261 | 1261 |
| **rho** | 0.41 | 0.43 | 0.36 | 0.36 | 0.30 | 0.33 |

Note: Data are from BHPS 1991-2008. Entries are odd ratios. Across the three sets of respondents, all of the odd ratios in the adjusted models fall within 95% CI of their corresponding odd ratios in the equivalent unadjusted model, indicating no significant difference between the coefficients in the two models. The reference categories of the categorical independent variables are: year 1991 to 1996, male, non-white, having good financial situation, married or living with a partner, not a smoker, does not have a GCE qualification or above, has more energy compared to average at their age and multi-person hh respectively. rho represents the percentage of variance that is due to differences across respondents, and the values in the table indicate enough variability between respondents to favour a random effects model. ** $p < 0.05$, *** $p < 0.01$.

## 13. Conclusion

This paper investigates a limitation in the standard methodology of weighting for non-response in longitudinal surveys in relation to dealing with units of unknown eligibility whilst weighting (during weighting, the standard approach assumes that all cases for which eligibility is unknown are eligible). The investigation here is based on considering the definition of eligibility in the BHPS (and in most longitudinal social science surveys today) which is ‘sample member is alive and living in the country where the survey is conducted’. Using population information on survival/mortality, the alternative method in this paper estimates likelihood of survival (adjustment factor) for cases in the sample (by comparing survival rates in the population with survival rates in the sample). The adjustment factor was then used to adjust non-response weights to correct for the potential inclusion of ineligibles in weights calculation. The findings in this paper suggest the following:

While weighting for non-response endeavours to reduce bias by correcting for the missing units and adjust the distribution of the responding sample to resemble the distribution of the selected sample, unknown eligibility may mislead this process. If some of the unknown eligibility units, that are assumed to be eligible during weighting, are actually ineligible, then weighting, as in the standard approach, may mislead the calculation of survey estimates. It will allow the eligible responding units, that are similar to the ineligible ones, to contribute to the calculation of the estimate in question more than they should. This is especially the case if the cause of ineligibility (e.g very poor health may cause death) is directly linked to what is being measured (e.g. indicators of good health). The findings in this paper appear to support this.

The introduced adjustment factor in this paper reduces the value of some of the weights from the standard approach which were otherwise too large, due to including some ineligible units in the denominator of the weighting model. Consequently, the variance of the weights is also reduced. As a result, the adjusted weights have significantly different impact on some estimates (by reducing their standard errors).

With respect to bias, although the multivariate analysis here does not show evidence that the adjusted weights reduce potential bias, the descriptive findings demonstrate that adjusted weights could result in different estimates of some descriptive statistics. This indicates that adjusted weights may remove bias components that were included because the standard weights contain influence of some ineligible units. If this approach applied in different data/analyses, with the availability of more accurate population information on survival/mortality, evidence for bias reduction are likely to be clearer both on estimates from multivariate and descriptive analyses. Thus, surveys that suffer from high rates of unknown eligibility, and where eligibility is also defined by being alive and living in the geographical area covered by the survey, the method is highly recommended.

However, when this approach is used one should pay extra attention to the mortality rates that are used to calculate the adjustment factor. For accurate calculation of adjustment factor, mortality rates should be up-to-date and reliable. For instance, the availability of a single-year mortality rates from the population (rather than 10-year age band as in the analysis in this paper) would improve the calculation of the adjustment factor as large age bands might conceal some information. Also, more importantly, one should use mortality rates for the same population covered by the survey both in terms of time period and geographical area. For example, in this research, the BHPS sample was selected only from residential addresses, meanwhile registered population mortality statistics include people at all types of addresses (e.g. nursing homes). Thus, registered mortality rates may not perfectly match the rates in the population of interest, at least for the first two or three years of the survey (eventually, those initially institutionalised people will die, and all of the new institutionalised population will have been from the residential addresses covered by the survey, so at that point the survey should become representative of the entire population, i.e. the same population to which the mortality statistics refer).

Moreover, the availability of population information on emigration and institutionalised individuals would be beneficial. Combining this information with mortality rates when calculating the adjustment factor will result in more accurate values as all forms of ineligibility are taken into account.

However, in surveys where ineligibility predominantly occurs by satisfying other characteristics (e.g. reaching a specific age or belonging to a certain social group) and maybe partially through death, the strategy of survival/death based adjustment factor may not be very useful. This is because the calculation of the adjustment factor (which is based on comparing the survival proportions in the sample and the population) in this case will have not taken into account the main forms of ineligibility. The approach of the adjustment factor will be more effective if the main ineligibility form in the sample can be found in the records of population statistics, or other reliable external data, as the case in this paper (i.e. population mortality rates).

Finally, an alternative procedure is to carry out a case-level death imputation in trying to identify dead sample members. This can be achieved by applying some kind of imputation model that can use available information about respondents of unknown eligibility (from their last interview), and relate it to similar characteristics of dead respondents. This can assist in identifying individuals among unknown eligibility cases who possess the same characteristics as dead respondents. For example, indicators of old age and poor health may be strong candidate variables for the imputation model. On the one hand, in this approach, eligibility is identified based on information from within the survey (no need for seeking and utilising information from external source such as a census etc…). Moreover, the main advantage of this method is that it helps in determining eligibility at the case level. Thus, weights' construction will easily exclude ineligible units. However, this approach may not be accurate, since death can sometimes occur randomly (i.e. regardless of age and health status). Therefore, the imputed values would contain some random variation as the imputation model is imperfect. Nonetheless, in situations where information on mortality rates in the population is not available/reliable, this approach could be recommended.

Another alternative procedure (used in the Health Survey for England and HILDA) could be contacting the death register office. In almost every country there is an office where deaths are registered. These offices collect information such as name, time and date of death, place and date of birth, the last address, occupation, reason for death and contact information of a surviving person related to the person died (usually a spouse or civil partner). If the survey organisation is able to contact the death register office and obtain this information, death can be identified by matching the records of respondents of unknown eligibility with the information held in the register office. The advantage of this approach is that it produces precise estimates based on accurate information. However, apart from the fact that this approach is time consuming (need to be done for all unknown eligibility cases at every wave), in some countries, register offices may not be willing to co-operate, for reasons of confidentiality.  In any case, for the BHPS sample, any method of identifying death in the sample should focus on respondents aged 65+ as most of the unknown death is centred in this age group.

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