Cash by Any Other Name?

Evidence on Labelling from the UK Winter Fuel Payment

Timothy Beatty	University of Minnesota and Institute for Fiscal Studies
Laura Blow	Institute for Fiscal Studies
Thomas Crossley	University of Cambridge and Institute for Fiscal Studies
Cormac O'Dea	Institute for Fiscal Studies

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Abstract: Many cash transfers and cash-equivalents are labelled (child benefit, rental vouchers, food stamps). Standard economic theory predicts that the labelling *per se* of such benefits should have no effect on spending patterns, and the empirical literature to date does not contradict this proposition. We study the case of the UK winter fuel payment (WFP), which is a cash transfer to older households. Sharp differences in the eligibility criteria provide an opportunity to assess the impact it has on fuel spending using a regression discontinuity design. We find robust evidence of a behavioural effect of the labelling. Households spend an average of approximately between 13% and 61% of the WFP on household fuel. If the payment was treated in an equivalent manner to other increases in income we would expect households to spend approximately 3% of the payment on fuel.

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1. Introduction

Government transfer payments to households and individuals are sometimes given labels indicating that they are designed to support the consumption of a particular good or service. For example, many countries provide transfers to households with children and label them a "Child Benefit". When such transfers are made in cash there is no obligation to spend all, or even any, of the payment on its ostensive purpose. Standard economic theory implies that the label of a particular transfer should have no bearing on how that transfer is ultimately spent since all income is fungible. The recipient of a transfer with a suggestive label is expected to react in exactly the same way as he would have reacted had he been given a transfer of equivalent value with a neutral label. The receipt of an in-kind transfer by infra-marginal consumers is similar. If an individual receives a voucher entitling them to quantity of a particular good or service that is less than the quantity that they would have consumed in the absence of the provision of the voucher (so that they are infra-marginal), standard economic theory suggests that they will treat the voucher as they would an unlabelled cash payment.[2] For these consumers, the fact that the voucher carries a label associated with a particular good should have no bearing on the optimal consumption of that good. Why then do governments label transfers? Of course, one possibility is that doing so makes redistribution more palatable to voting taxpayers. However, another intriguing possibility is that standard economic theory is mistaken on this particular point, and spending patterns *can* be influenced by the labelling of cash or cash equivalent transfers.

The theoretical proposition that labelling does not matter has not gone unchallenged. Thaler's (1990, 1999) framework of mental accounts is one mechanism through which the labelling of a transfer might affect its usage.[3] There is, though, very little empirical evidence to support the idea that the labelling of a transfer matters. Kooreman (2000) finds tentative evidence of a labelling effect among child benefit recipients in the Netherlands, but the effect estimated using his most credible identification strategy is not statistically different from zero.[4] Edmonds (2002) also looks at the effect of child benefit payments (this time amongst

[2] When the amount of an in-kind transfer is less than the amount that the recipient would have consumed in the absence of the transfer, it is often referred to as a *non-distortionary* in-kind transfer.

[3] Thaler's (1990, 1999) framework of mental accounts was originally developed in the context of intertemporal choice under uncertainty to address perceived failures in the standard economic model of consumption and saving over the life-cycle. The general idea is that the household has a system of mental accounts. In the life-cycle context, these would be a current income account, an asset account and a future income account. In the present context, income would be labelled according to its source, and so the Winter Fuel Payment would be allocated to a mental account for spending on heating.

[4] In particular, he finds a statistically significant effect of child benefit income on the spending patterns of couple household, but this finding cannot distinguish a labelling mechanism from an intrahousehold mechanism

families in Slovakia) and finds no evidence of a labelling effect. Moffitt (1989) and more recently Whitmore (2002) look at the effect of food stamps on consumption choices and finds that no evidence that inframarginal consumers treat food stamps differently to an equivalent cash payment.[5] In contrast, Abeler and Marklein (2010) have recently compared in-kind grants and (unlabelled) cash grants in small laboratory and field experiments and find evidence against the fungability of money in those particular contexts.[6]

In this paper we provide novel evidence on the behavioural effect of labelling based on an actual transfer received by a large population: the UK Winter Fuel Payment (WFP). The WFP is a universal annual cash transfer paid to households containing an individual aged 60 or over in the qualifying week of the relevant year (usually the third full week of September).[7] Its payment is unconditional - there is no obligation to spend any of it on household fuel. The payment is usually made in one lump sum in November or December and is currently worth £250 to households where the oldest person is aged between 60 than 80 and £400 where the oldest person is aged 80 or over.

The sharp age cut-off for receipt eligibility (the fact that all households where there is somebody aged 60 or older in the third week of September qualify for the benefit and no households where all members are younger than 60 qualify) presents an excellent opportunity to employ a regression discontinuity design to assess whether there is labelling effect associated with the WFP.

The WFP delivers additional disposable income but eligibility for the WFP, being based on age, is easily anticipated. Thus the additional disposable income may not lead to a change in spending at the onset of eligibility. To the extent that the additional disposable income that the transfer delivers *does* lead to an increase in total expenditure, we would expect this to be associated with an increase in spending on fuel (because fuel is a normal good) and a decrease in the fuel budget share (because fuel is a necessity), regardless of

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(in which the power of different household members is affected by which partner receives income). Among single-mother households, for whom these intrahousehold considerations are not relevant, he finds an effect in the direction consistent with labelling mattering, but which is not significantly different from zero.

[5] For a more recent evaluation of the effect of food stamps on consumption and labour choices, though one not directly relevant to our question here, see Hoynes (2009).

[6] First Abeler and Marklein show in a field experiment in a restaurant that beverage vouchers increase beverage consumption by more than a general voucher towards their total bill. The difference is statistically significant and larger than what might plausibly attributed to the small number of patrons for whom the transfers might be distortionary. They then show a similar effect with notional consumption of two goods in a laboratory experiment with students.

[7] Strictly speaking it is paid to households where anyone is over the female state pension age. This age was 60 for the entire period for which we have data. However, between April 2010 and April 2046 it is planned that eligibility will rise gradually to the age of 68.

whether the transfer is labelled. This variation in fuel spending and budget share with total expenditure is the "income effect" of standard demand theory.

Therefore, to test for a labelling effect, and to distinguish it from an income effect, we estimate an Engel curve for fuel expenditure allowing for flexible effects of total expenditure on the fuel budget share, and we augment this with smooth age effects on preferences and a discontinuity at age 60. This discontinuity captures the effect of payment of the WFP on share of total expenditure spent on fuel, *holding total expenditure constant*. The size of this shift is informative about the proportion of the WFP that is spent on fuel above and beyond what would be expected given the way fuel expenditure normally varies with total expenditure (as measured by the slope of the Engel curve.)

We find statistically significant and robust evidence of a substantial labelling effect. Households spend an average of approximately between 13% and 61% of the WFP on household fuel. If the payment was treated in an equivalent manner to other increases in income we would expect households to spend approximately 3% of the payment on fuel. We conduct a number of robustness and falsification tests and in particular, we carefully test – and reject – the possibility that this effect arises from non-separabilities between consumption and leisure. Thus this dramatic difference in the marginal propensity to consume fuel out of the WFP is evidence that the name of the benefit (possibly combined with the fact that it is paid in November or December) has some persuasive influence on how it is spent.

An understanding of the effect that labels have is important for public policy. If labelling cash or cash-equivalents influences how they are spent, then governments might use labels innovatively to try and increase consumption of particular goods or services that are thought to be under-consumed.[8] Of course, if the aim of a particular transfer is not to increase spending on any particular good or service but rather to carry out a straightforward redistribution of resources then an operative label might actually imply a utility cost – and care should be taken in naming benefits.

This paper proceeds as follows. Section 2 gives a brief introduction to the data that we use (the Living Costs and Food Survey). Section 3 outlines the empirical framework that we apply to identify the labelling effects, and our estimation methods. Section 4 presents our estimates of the magnitude of the labelling effect. Section 5 concludes.

2. Data

[8] Because labels are not constraints, this would be very much in the spirit of Thaler and Sunstein's (2008) "paternalistic libertarianism".

The Living Costs and Food Survey (LCF)[9] is the primary source of household-level expenditure data in the UK. It is a nationally representative annual survey with a sample size of approximately 6,000 households. Surveys are conducted throughout the year. The survey consists of an interview and an expenditure diary. Each respondent is asked to keep a diary for a two-week period in which they record every purchase that they make. In addition, an expenditure questionnaire asks them to record recent purchases of more infrequently-bought items. The combination of the diary and questionnaire allows the construction of a comprehensive measure of household expenditure. In the case of fuel spending, some information comes from the questionnaire (for example last payment of electricity on account) and some from the diary (for example slot meter payments). Total spending on fuel includes gas and electricity payments, and the purchase of coal, coke and bottled gas for central heating. Clearly some electricity and gas use may have been for cooking, lighting etc and not heating, but it is not possible to separate this out. In addition to these measures, the LCF records detailed income, demographic and socio-economic information on respondent households.

In our main analysis, we pool data from the years 2000 through 2008. The nominal value of the WFP was fairly stable over this period, with the main rate (paid at age 60) varying between £200 and £250 per year. In some analysis (to be described below) we also use a second tranche of data set covering the years 1988 through 1996. These data predate the introduction of the WFP in 1997. We do not use data from the years 1997 through 1999. In this period the WFP existed, but was much less generous than it is currently.

The sample that we use is comprised of single men and couples without children in which the male member of the couple is older. We exclude all households in which the oldest member of the household is less than 45 years old. We exclude single women and couple households in which the oldest member is a woman because for such households, eligibility for the WPF occurs at the same time as the woman becomes eligible for the state pension. Table 1 presents summary statistics for this sample divided between eligible households and households in which the oldest member is below the age cut-off

[TABLE 1 ABOUT HERE]

3. Empirical Framework and Estimation

[9] The LCF was known as the Expenditure and Food Survey (EFS) between 2001 and 2007 and previous to that was known as the Family Expenditure Survey (FES).

Households where the eldest member turns 60 before the qualifying week are eligible for the WFP and households where the eldest member turns 60 after the qualifying week are not. This sharp eligibility criterion suggests estimating the effects of the WFP using a regression discontinuity design (RDD). Take up of the WFP is very high, and so the sharp eligibility criterion can be considered a sharp receipt criterion.[10]

The intuition behind an RDD approach is straightforward: households immediately below the cut-off provide evidence on how households immediately above the cutoff would have behaved had they not received the transfer. The identifying assumption is that, in the absence of the transfer, expenditures vary continuously with age, implying that, for the sample we consider, preferences and budgets evolve smoothly with age. Any discrete change at age 60 is thus attributable to the average effect of the WFP (at age 60). Age has previously been used as the running variable in regression discontinuity designs. See for example: Edmonds et al. (2004), Card et al. (2008), Carpenter and Dobkin (2009) and Lee and McCrary (2009).

We investigate two different effects: on the level of expenditure, and on allocation of expenditure to different goods (that is, budget shares), given the overall level of expenditure.

Analysis of Spending Levels

The onset of WFP eligibility is easily anticipated, and so it is not obvious that the additional disposable income will lead to any change in spending. Thus, as a first step, we investigate whether households spend more on fuel, and on other goods, as a result of receiving the WFP. We model expenditure on good k by household i, x_{ki} as a function of program eligibility, D_i, and the forcing variable, age of the oldest household member, A_i. Eligibility is related to age by $D_i = 1[A_i \geq 60]$ where $1[.]$ is the indicator function.[11] As per Lee and Lemieux (2010), we interact $(A_i - 60)$ and $(A_i - 60)^2$ with program eligibility to allow the effect the slope and curvature of the regression line to differ on either side of the eligibility cut-off. Finally, we include a number of covariates, Z_i, to increase the precision of the regression discontinuity estimator. In all specifications, these include household size,

[10] The rate of take-up was above 90% in each year since 2003 - the first year our data allows us to estimate it.

[11] Note that in recent years the eligibility reference week has been in September. Because the LCF collects information on age at the time of interview, there is some risk of misclassifying households interviewed in October through December as being eligible, when they were not. To this end, we follow Lee and Card (2010) and adjust the discontinuity to reflect the probability that that the oldest member of the household was 60 in the previous September and were thus eligible to receive the winter fuel payment. In practice, households in which the oldest member is 60 and are observed in October receive a weight of 11/12, if they are observed in November they are assigned a weight of 10/12, and so on. Every household with a person aged 61 and above simply has a weight of 1.

month, area, year and area/year interactions. In several specifications we also include employment, housing tenure, number of rooms and education controls. This yields the following empirical specification in levels of expenditure on good *k*:

$$x_{ki} = \alpha + \tau D_i + \beta_1 (A_i - 60) + \beta_2 (A_i - 60)^2 + D_i \cdot \beta_3 (A_i - 60) + D_i \cdot \beta_4 (A_i - 60)^2 + \gamma^T Z_i + e_i \quad (1)$$

where *e* is an independent and heteroskedastic disturbance term. The coefficient on D_i, $\tau = \lim_{A \downarrow 60} E[y_k \mid A = 60, Z = z] - \lim_{A \uparrow 60} E[y_k \mid A = 60, Z = z]$, provides a local estimate of the effect of the WFP on expenditures at age 60. We estimate this model (and all subsequent models unless otherwise stated) using least squares and report robust standard errors.

Testing For Labelling Effects in an Engel Curve Framework

Finding that the receipt of WFP is associated with an increase in fuel spending is not unambiguous evidence of a labelling effect; we need to distinguish a labelling effect from a standard income effect and to assess whether the WFP is allocated *differently* to how an unlabelled transfer would be allocated. Therefore, in our main analysis we embed our regression discontinuity design within an Engel curve framework. If households on either side of the eligibility criteria spend significantly different shares of expenditure on fuel, *holding total expenditure constant*, this would be direct evidence of a labelling effect.

In standard demand analysis, Engel curves measure the relationship between household spending on a good and total household expenditure as total expenditure increases. A common empirical specification of Engel curves relates budget shares to the logarithm of total expenditure. Fuel is a normal good so as the level total expenditure rises we would expect fuel expenditure to rise. Because fuel is also a necessity, we would expect it to rise less quickly than total expenditure, and so the budget share should fall. These are standard income effects.

An implication of this is that finding that the receipt of WFP is associated with an increase in fuel spending is not unambiguous evidence of a labelling effect. An increase in fuel spending, or a decrease in the fuel budget share, with receipt of the WFP might simply represent a move *along* the Engel curve. This income effect is illustrated by the move from point A to point B in Figure 1, where the Engel curve is presented in share form.

In contrast, if there is a labelling effect, when a household receives a labelled transfer, they will shift off this Engel curve, as illustrated in Figure 1 by the move from point B to point C.

[FIGURE 1 ABOUT HERE]

To test for such a labelling effect, while allowing for standard income effects, we nest a regression discontinuity design within a standard Engel Curve framework. In particular, we estimate Engel curves which relate budget shares to a function of total expenditure, time and region dummies (to capture variation in relative prices) and demographic variables. We allow preferences to evolve continuously with age by including polynomials in age among the demographic variables. Finally, we augment this empirical specification with a dummy for for WFP eligibilty. This last variable captures any discontinuity in the way that budget shares vary with age, *conditional on total expenditure* (and other covariates). We attribute any such discontinuity to the effect of labelling the transfer.

Hence, in complete form, our regression discontinuity Engel curve specification, again using quadratic terms in age, can be written:

$$w_{ki} = \alpha + \tau D_i + \beta_1 (A_i - 60) + \beta_2 (A_i - 60)^2 + D_i \cdot \beta_3 (A_i - 60) + D_i \cdot \beta_4 (A_i - 60)^2 + \delta^T \cdot f(X_i) + \gamma^T Z_i + e_i \tag{2}$$

where, as before, *e* is an independent and heteroskedastic disturbance term and, the dependent variable is the budget share of good *k*, and $\tau = \lim_{A \downarrow 60} E[y_k \mid A = 60, Z = z, X = x] - \lim_{A \uparrow 60} E[y_k \mid A = 60, Z = z, X = x]$ provides a local estimate of the effect of the WFP on budget shares at age 60, holding total expenditure constant.[12] In results

[12] This specification imposes that the labelling effect, if any, measured in share form, is unrelated to the level of total expenditure. We will test this specification below. A more general formulation which nests equation (2) above as follows. Ignoring other covariates for the moment, write the budget share of good *k*, w_k, as

$$w_{ki} = f\left(X_i + g(A_i, X_i)\varpi_i\right) + h(A_i)$$

where ϖ_i is the WFP measured in pounds and $g(A_i, X_i)$ is some function of age and total expenditure. The null hypothesis of no labelling effect corresponds to $g(A_i, X_i) = 0$. Taking a (first order) Taylor approximation of $f\left(X_i + g(A_i, X_i)\varpi_i\right)$ around $\varpi_i = 0$ we obtain

$$f\left(X_i + g(A_i, X_i)\varpi_i\right) \simeq f(X_i) + \frac{\partial f(X_i)}{\partial X_i} g(A_i, X_i)\varpi_i$$
$$\equiv f(X_i) + \gamma(A_i, X_i)\varpi_i$$

Noting that we can always write $h(A_i) = (1 - D_i)h_1(A_i) + D_i h_2(A_i)$ then we can approximate the more general model above by:

$$w_{ki} = f(X_i) + D_i\left[\gamma(A_i, X_i)\varpi_i + h_2(A_i) - h_1(A_i)\right] + h_1(A_i)$$

We do not have sufficient data to estimate properly how $\gamma(A_i, X_i)$ might vary with X_i and so, as in addition there is very little variation in ϖ_i, we estimate an average effect, replacing $\gamma(A_i, X_i)\varpi_i$ with $\gamma(A_i)\lambda$ where λ is some constant. The only general thing we are prepared to assume about $h(A_i)$ is that $h_1(60) = h_2(60)$ and hence the only age at which we can separately identify $\gamma(A_i)\lambda$ from

presented below, we specify $f(X)$ to be a quadratic function of the natural logarithm of total expenditure, but results are robust to more flexible specifications.[13] Note that the total expenditure variables are also interacted with year dummies; within the constraints imposed by theory, we want to allow the form of the Engel curves we estimate to be quite general and so we allow the slope (as well as the intercept) of the Engel curve to change as relative prices change. This is important to ensure that the discontinuity effect we estimate is not picking up changes in the shape of the Engel curve over time that we have not allowed for.

We now turn to possible threats to the validity of this research design and how we deal with them.

Measurement error

One possible concern is that measurement error in household expenditure could bias our estimate of the effect of WFP. In general, measurement error in one variable can potentially bias the estimate of all regression coefficients. In a simple example with classical measurement error where the only regressors are log expenditure and WFP receipt, the bias on the WFP coefficient would have the same sign as the relationship between log expenditure and the fuel share, which is negative, and so the bias would actually be downwards (against finding a labelling effect). However, as we cannot be sure that this would be the case in our more complicated specification. Therefore, as a check, we follow standard practice in demand analysis and instrument total expenditure with household income.

Employment Effects

From 1988 onwards individuals aged 60 or over have been entitled to a benefit, the name and exact details of which have changed, but which is essentially a pensioner (i.e. without obligation to seek work) minimum income guarantee. From 1988 to 1999 this was called Pensioner Income Support, from 1999 to 2003 it was known as the Minimum Income Guarantee, and in 2003 this was replaced with Pension Credit. For the rest of this paper we will refer to this benefit as the Minimum Income Guarantee (MIG). Therefore, note that we do not have a period where age 60 brings only eligibility for WFP; from 1988-1996 we have the MIG alone and from 1997-2008 we have the MIG plus WFP.

Whilst we would not expect the MIG to have a labelling effect, it might have a labour market participation effect, and, if consumption is not separable from leisure, this in turn will

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$h_2(A_i) - h_1(A_i)$ is at age 60 where $h_2(A_i) - h_1(A_i) = 0$ (this is basically a restatement of the assumptions underlying the regression discontinuity design as applied to our particular case.)

[13] Engel curves relating budget shares to a quadratic function of the natural logarithm of total expenditure are the basis of the well known Quadratic Almost ideal Demand System (QuAIDS) of Banks, Blundell and Lewbel (1997).

have an effect on spending patterns. Specifically, when a working individual turns 60, they become entitled to the MIG and they might prefer stopping work and receiving the MIG to carrying on in employment. But dropping out of the labour market might influence spending patterns; someone who is now at home for more of the day might heat their home more and therefore have higher fuel spending.

It might be that controlling for observable labour market status is enough do deal with this issue, and among our specification tests we include employment and self-employment dummies and hours of work for both the head of household and (where there is one) the spouse. However, using 1988-1996 as a placebo period allows an additional check on whether our results are contaminated by the labour market effect of the MIG. Estimating an RDD on a pre-program period as a falsification test is normally good practice (see, for example Lemieux and Milligan (2008)), but here it is particularly important because the potential confounding of the WFP effect by the MIG.

We proceed by pooling data from the period when only the MIG was paid (1988-1996, denoted T_1) with the period in which both the MIG and the WFP were paid (2000-2008, denoted T_2). Denoting eligibility for the MIG by *M,* our Engel curve specification becomes:

$$w_k = \alpha + \tau D + \lambda M + \beta_1 \left(Age - 60 \right) + \beta_2 \left(Age - 60 \right)^2 + D \cdot \beta_3 \left(Age - 60 \right) + D \cdot \beta_4 \left(Age - 60 \right)^2$$
$$+ M \cdot \beta_5 \left(Age - 60 \right) + M \cdot \beta_6 \left(Age - 60 \right)^2 + \delta^T \cdot f\left(X \right) + \gamma^T Z + e$$

Note that here the MIG eligibility dummy *M* is one if the oldest member of the household was over 60 in the reference week. while the WFP eligibility dummy is now equal to one only if the oldest member of the household was over 60 in the reference week *and* the observation is drawn from period T_2 (that is, it is an interaction between age and period).

The coefficient on the MIG eligibility dummy measures any discontinuity in the way expenditure patterns vary with age in the period prior to the introduction of the WFP. Thus a significant effect would falsify the assumption that preferences evolve continously with age. The coefficient on the WFP eligibility dummy,

$$\tau = \left\{ \lim_{age \downarrow 60} E[w_k \mid age = 60, X = x, Z = z] - \lim_{age \uparrow 60} E[w_k \mid age = 60, X = x, Z = z] \right\}_{T_2}$$
$$- \left\{ \lim_{age \downarrow 60} E[w_k \mid age = 60, X = x, Z = z] - \lim_{age \uparrow 60} E[w_k \mid age = 60, X = x, Z = z] \right\}_{T_1}$$

is our "differenced-RDD" estimate of the average effect of the WFP on budget shares at age 60, *net* of any labour market effect at age 60.

Additional Robustness Checks

Regression discontinuity designs can be sensitive to the choice of the range of the forcing variable included in the regression, here the age of the oldest household member. In principle, one would like to compare households located immediately on either side of the potential discontinuity, but in practise sample size considerations prevent this. Our basic specification uses a window of fifteen years on either side of the discontinuity (45-75). As a robustness check we re-estimate with a window of ten years on either side of the discontinuity (50-70).

Finally, we conduct a further falsification test. We rerun our main analysis but with cut-offs at 55 and 66[14] rather than 60. Under the maintained assumptions of the regression discontinuity design we should not find discontinuities (in levels or shares) at these age cut-offs.

4. Results

Effects on spending levels

We begin by looking at the effect of receipt of the WFP on the level of expenditure on different goods. Column 1 of Table 2 shows that the only effect that is estimated with any precision is a small increase in spending on fuel. This finding is robust to additional control variables for education, employment and housing tenure (column 2) but is not statistically significant at any conventional level when we vary the age window used in estimation (column 3).

[TABLE 2 ABOUT HERE]

As discussed above, an effect of the WFP on fuel, on its own, is not unambiguous evidence of a labelling effect, though it is perhaps suggestive that it is only for fuel that we find any statistically significant effect (albeit only at the 10% level). Standard income effects, if present, should be evident in all goods, and be larger for luxuries like clothing and leisure goods than for necessities like food and fuel (the former have larger income elasticities). We now proceed to the results of estimating the labelling effect in an Engel curve framework.

Testing For Labelling Effects in an Engel Curve Framework

[14] Note we use 66 rather than 65 as 65 was the state pension age for men during the period for which we have data.

Table 3 shows the results of our Engel curve estimation. The first column of the Table, specification 1, gives our baseline results. We find a positive, statistically significant discontinuity effect for the fuel share and no significant effect for any other good. We interpret this effect on the fuel share, holding total expenditure constant, as a labelling effect.

The point estimates for food and clothing suggest a negative effect; the budget constraint of course implies that the positive effect on fuel spending must be offset by reductions elsewhere.

In column (2) we add additional control variables for education, employment and housing tenure variables and in column (3) we vary the age window used in estimation. The positive effect on the fuel share is robust across these specifications. The negative effect on the food share becomes statistically significant at the 10% level when we narrow the age window.

[TABLE 3 ABOUT HERE]

In Table 4 we report the results of additional specification checks for the fuel share. This has almost no impact on the estimated labelling effect. In column (1) we additionally control for the number of rooms in the home. In columns (2) and (3) we instrument for total expenditure with household income to account for the possibility of measurement error in total expenditure. This, again, has almost no impact on the estimated labelling effect.

In column (4) we report the results of estimating our "differenced-RDD" specification on pooled data from 1988-1996 and 2000-2008. This is therefore the average effect of the WFP on budget shares at age 60, conditional on total expenditure *net* of any employment effect at age 60. Note that the estimate here is larger than our baseline estimate, and though less precisely estimated, is still significant at the 5% level. This suggests that the labelling effect that we find in the 2000-2008 period is not an employment effect.

Our basic specification imposes that the labelling effect on budget shares, if present, is unrelated to the level of total expenditure. In column (5) we report the results of relaxing this assumption and allowing the effect to vary by quartile of total expenditure. The point estimates suggest that the effect is larger for poorer households, but the coefficients are not precisely estimated, and we cannot reject the null that the effect is unrelated to the level of total expenditure.

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[TABLE 4 ABOUT HERE]

Table 5 presents the results of our falsification tests. Columns (1) and (2) of Table 5 report tests for discontinuities in the relationship between age and fuel budget share at ages 55 and 66. Column (3) is the complement to column (4) of Table 4. Here we report estimates of a discontinuity at age 60 in the period before the WFP was introduced (1988-1996). In all three cases, we find no effect.

[TABLE 5 ABOUT HERE]

To summarize, we find a positive effect of WFP eligibility on the budget share of food, conditional on total expenditure and allowing preferences to evolve with age in a continuous fashion. The effect is strongly statistically significant and robust across alternative specifications. Because of the very high take-up of this transfer among eligible households, the effect of eligibility is for all intents and purposes also the effect of receipt. We attribute this effect to the labelling of this transfer. A series of falsification tests failed to contradict our identifying assumptions, and in particular, we find no evidence of a confounding of the labelling effect with employment effects around age 60.

5. Discussion

We can translate the magnitudes in the table into spending changes as follows. Ignoring other covariates for simplicity, if

$$w_k = \frac{x_k}{X} = f(x)$$

then

$$\frac{\partial x_k}{\partial X} = \frac{\partial w_k}{\partial X} X + w_k$$

so if households receive a transfer of wfp then the slide along the Engel curve starting from total budget X (the move from A to B in Figure 1) is approximately

$$\left(\frac{\partial w_k}{\partial X} x + w_k\right) wfp$$

and if our estimate of the movement off the Engel curve is τ, then the estimate of the labelling effect (the move from B to C in figure 1) is approximately

$$\tau(X + wfp)$$

With the results from, say, specification 2 in Table 3 our estimate of the slide along the Engel curve for someone with the average fuel share in 2008 of 0.0603 and total budget of around £316 per week receiving a transfer of £250 a year (so just under £5 a week) is £0.126 with a standard error of 0.009 and a 95% confidence interval around this point estimate of £0.108 to £0.145. Our estimate of the labelling effect is £1.796 with a standard error of 0.587 and 95% confidence interval of £0.646 to £2.946. In other words, if there was no labelling effect an average household would spend around 3% of a small transfer on fuel. We estimate an additional labelling effect of 37% with a confidence interval of 13% to 61%.

Conclusion

This paper asks whether labelling an unconditional cash transfer has any effect on the way in which recipients spend it. In other words, does calling the £250 that most elderly households receive in November / December a "Winter Fuel" payment make any difference? Sharp differences in the eligibility requirements allow us to use regression discontinuity design to examine how fuel expenditure changes on receipt of the benefit. We find a substantial and robust labelling effect. Recipient households, on average, exhibit a marginal propensity to consume household fuel of the WFP of between 13% and 61%. This compares to estimates of the (average) marginal propensity to consume household fuel of approximately 3%. The interpretation of this is straightforward: if households are given an unconditional and neutrally-named cash transfer of £100 they would be expected to spend approximately £3 on household fuel. If they are given an unconditional cash transfer called the Winter Fuel Payment in the middle of winter we estimate that they will spend between £13 and £61 on fuel. Overall, our evidence implies that the label of this particular transfer has a critical impact on the behavioural response displayed by those who receive it.

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Figures and Tables

Figure 1: Engel Curve with

Income Effect and Labelling Effect

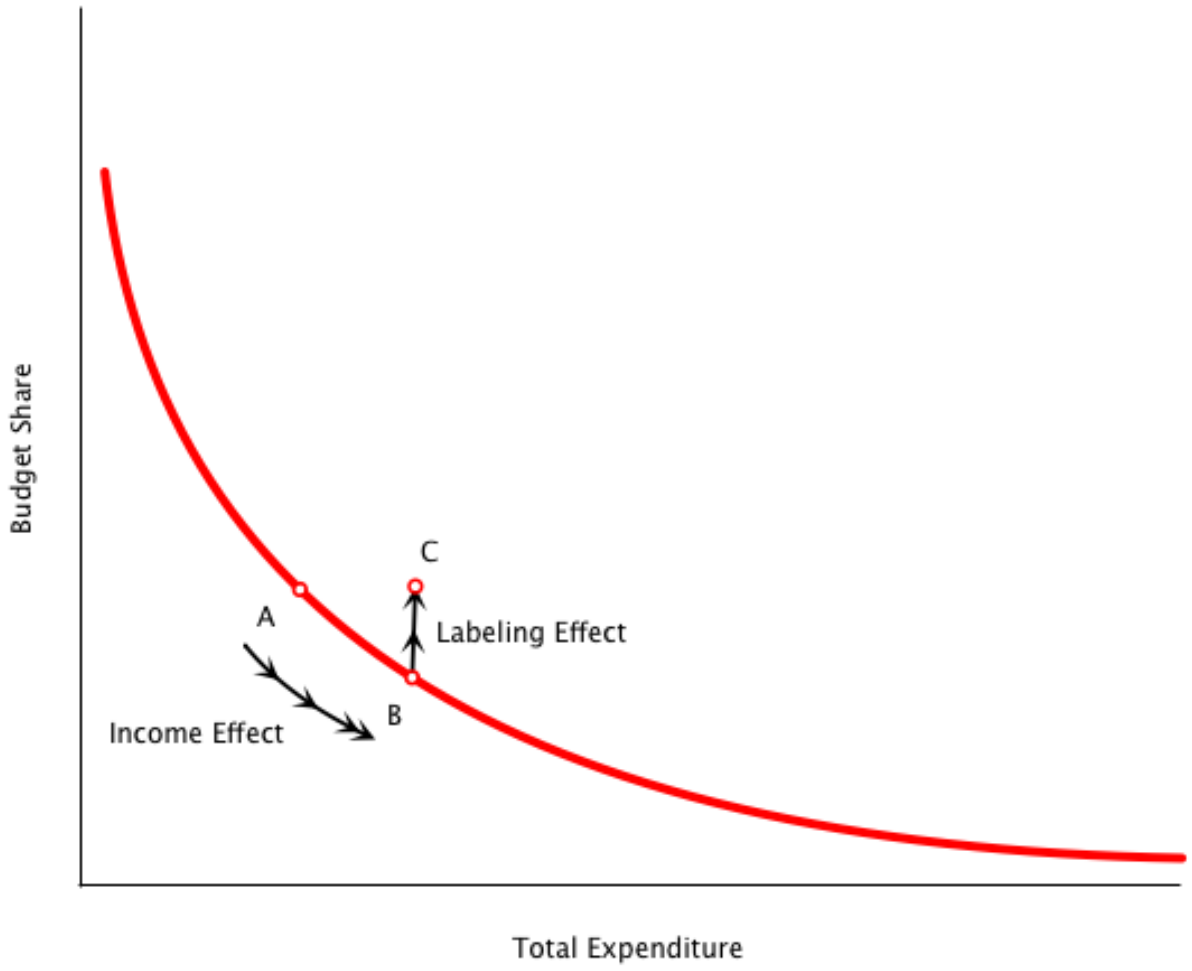

Table 1. Descriptive Statistics – weekly means (£ and shares)

	Ages 45-60		WFP Eligible	
	Everyone	Poor	Everyone	Poor
Income	542.64	233.79	410.45	261.37
Total expenditure	443.89	143.37	366.69	166.10
Fuel	18.70	12.45	18.92	14.58
Food	45.66	27.32	48.17	36.50
Clothing	13.86	2.66	12.29	3.77
Leisure Goods	14.28	4.37	13.21	5.74
Fuel Share	0.045	0.078	0.054	0.077
Food Share	0.129	0.201	0.163	0.227
Clothing Share	0.034	0.020	0.037	0.026
Leisure Goods Share	0.039	0.038	0.044	0.042
Sample Size	4972	992	6933	2274

Data: Living Costs and Food Survey (LCF), 2000-2008. Single men and couples without children in which the male is older. The LCF was known as the Expenditure and Food Survey (EFS) between 2001 and 2007 and previous to that was known as the Family Expenditure Survey (FES).

Table 2. RDD estimates. £/month effects in levels

	(1) (OLS)	(2) (OLS)	(3) (OLS)
Income	12.30	-0.03	-12.84
Expenditure			
Total	10.32	8.74	-10.79
Fuel	1.33†	1.32†	0.76
Food	0.81	-1.00	-2.47
Clothing	0.01	-0.20	-2.50
Leisure Goods	2.00	1.89	2.24
Age Window	45-75	45-75	50-70
Additional Controls:			
Employment variables		Y	Y
Education variables		Y	Y
Housing Tenure		Y	Y
Number of Rooms			

Notes:

1. The base specification includes the following controls: year dummies, region dummies and their interactions; month dummies; and (the natural logarithm of) household size.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors.
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level

Table 3. RDD estimates.
Effects of WFP on budget Shares
(conditional on total expenditure)

Shares	(1) OLS	(2) OLS	(3) OLS
Fuel	0.0056**	0.0056**	0.0059**
Food	-0.0025	-0.0025	-0.0086†
Clothing	-0.0015	-0.0017	-0.0049
Leisure Goods	0.0022	0.0022	0.0035
Age Window	45-75	45-75	50-70
Data Period	2000-2008	2000-2008	2000-2008
Additional Controls:			
Employment variables		Y	Y
Education variables		Y	Y
Housing Tenure		Y	Y

Notes:

1. The base specification includes the following controls: (the natural logarithm of) total expenditure and its square; year dummies, region dummies and their interactions; interactions between the year dummies and the total expenditure variables; month dummies; and (the natural logarithm of) household size.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors.
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level

Table 4: Further Specification Checks
Effects of WFP on Fuel Budget Share
(Conditional on Total Expenditure)

	(1) OLS	(2) IV	(3) IV	(4) OLS	(5) OLS
Expenditure Quartile:					
All	0.0057**	0.0054**	0.0054**	0.0068*	
1st					0.0115
2nd					0.0056†
3rd					0.0032
4th					0.0024
Test of Equality					
F(3,11289)					0.63
(p-value)					(0.59)
Age Window	45-75	45-75	45-75	45-75	45-75
Data Period	2000-2008	2000-2008	2000-2008	2000-2008 And 1988-1996	2000-2008
Quartiles of Total Expenditure	All	All	All	ALL	Poorest
Additional Controls:					
Employment variables	Y	Y	Y	Y	Y
Education variables	Y	Y	Y	Y	Y
Housing Tenure	Y	Y	Y	Y	Y
Number of Rooms	Y		Y		
Minimum Income Guarantee				Y	

Notes:

1. The base specification includes the following controls: (the natural logarithm of) total expenditure and its square; year dummies, region dummies and their interactions; interactions between the year dummies and the total expenditure variables; month dummies; and (the natural logarithm of) household size.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors.
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level

Table 5. Falsification Tests.
Effects on Fuel Budget Share
(Conditional on Total Expenditure)

Shares	(1) OLS	(2) OLS	(3) OLS
	Discontinuity at 55	Discontinuity at 66[5]	Prior to Policy Introduction
Fuel	0.0017	-0.0004	-0.0009
Age Window	45-75	45-75	45-75
Data Period	2000-2008	2000-2008	1988-1996
Additional Controls:			
Employment variables	Y	Y	Y
Education variables	Y	Y	Y
Housing Tenure	Y	Y	Y

Notes:

1. The base specification includes the following controls: (the natural logarithm of) total expenditure and its square; year dummies, region dummies and their interactions; interactions between the year dummies and the total expenditure variables; month dummies; and (the natural logarithm of) household size.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors.
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level
5. To avoid issues around the male retirement age of 65 we chose 66, although results for age 65 are similar