Cash by any other name? Evidence on labelling from the UK Winter Fuel Payment

Timothy K.M. Beatty
Laura Blow
Thomas F. Crossley
Cormac O'Dea

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Timothy K.M. Beatty[1]   University of Minnesota[a]
and
Institute for Fiscal Studies[b]

Laura Blow   Institute for Fiscal Studies[b]

Thomas F. Crossley   Institute for Fiscal Studies[b]
University of Cambridge[c]
and
Koç University[d]

Cormac O'Dea   Institute for Fiscal Studies[b]

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[a]Department of Applied Economics
University of Minnesota
1994 Buford Ave
St Paul, MN USA
55108

[b]Institute for Fiscal Studies
7 Ridgmount Street
London
WC1E 7AE, UK

[c]Faculty of Economics
University of Cambridge
Sidgwick Avenue
Cambridge
CB3 9DD, UK

[d]Department of Economics
Koç University
Rumelifeneri Yolu
Sariyer 34450 Istanbul
Turkey

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Abstract:
Standard economic theory implies that the labelling of cash transfers or cash-equivalents (e.g. child benefits, food stamps) should have no effect on spending patterns. The empirical literature to date does not contradict this proposition. We study the UK Winter Fuel Payment (WFP), a cash transfer to older households. Exploiting sharp eligibility criteria in a regression discontinuity design, we find robust evidence of a behavioural effect of the labelling. On average households spend 41% of the WFP on fuel. If the payment was treated as cash, we would expect households to spend approximately 3% of the payment on fuel.

1. Introduction

Government transfers 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 such as food stamps is similar as long as consumers are infra-marginal – i.e. for those whom consumption of the good in question is already larger than the voucher amount. 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. In this paper we provide novel evidence on the behavioural effect of labelling from the UK Winter Fuel Payment (WFP).

The theoretical proposition that labelling is irrelevant has been challenged. For example, Thaler's (1990, 1999) framework of mental accounts is one mechanism through which the labelling of a transfer might affect its usage.[2] There is, though, very little previous empirical evidence to support the idea that the labelling of a transfer payment matters. Kooreman (2000) and Blow, Walker and Zhu (2010) find evidence that additional child benefit differs from other income in its effect on household spending patterns among child

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[2] 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.

benefit recipients in the Netherlands and the UK respectively. Kooreman finds some evidence of a labelling effect (i.e. child benefit is spent on child-related goods); in contrast, Blow, Walker and Zhu's results suggest child benefit is spent disproportionately on adult-related goods[3]. Edmonds (2002) also looks at child benefit payments (in this case amongst families in Slovakia) and finds no evidence of a labelling effect. It is important to note that plausible identification in these studies rests solely on time series variation in the real value of child benefit within household type. As we explain below, the design of the Winter Fuel Payment leads naturally to a regression discontinuity design. Moreover, it is not possible in two-adult households to separately identify a labelling effect of child benefit income from the alternative explanation that the increase in the share of total household income received by the mother (child benefit is almost always paid to the mother) leads the change in spending patterns. That is, it could be who receives the money, rather than the label, that matters. This issue of intrahousehold allocation seems particularly important in the case of spending on children. Among single-mother households, for whom these intrahousehold considerations are not relevant, Kooreman finds an effect in the direction consistent with labelling mattering, but which is not significantly different from zero at conventional levels. Similarly, Blow, Walker and Zhu find weaker results for single-parent households.

Turning to in-kind transfers, Moffitt (1989) and more recently Whitmore (2002) look at the effect of food stamps on consumption choices and find no evidence that infra-marginal consumers treat food stamps differently than an equivalent cash payment. 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 contexts.[4,5]

The WFP, which we study, is a universal annual cash transfer paid to households containing an individual aged 60 or over in the qualifying week of the relevant year.[6] Its

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[3] This does not imply parents disregard their children's welfare. The paper finds evidence that this spending effect comes from the unanticipated variation in child benefit which suggests that parents are altruistic and insulate their children from income variation.

[4] 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.

[5] There is much better evidence that labelling of transfers between levels of government has an important effect on how the transferred funds are spent. This is called the "flypaper effect". See Hines and Thaler (1995).

[6] In recent years the qualifying week has been the third full week of September. Strictly speaking the WFP 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.

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 during most of the period covered by our data was 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 (these values were reduced to £200 and £300 in the UK Budget of March 2011). The sharp age cut-off for receipt eligibility (the fact that all households where there is somebody aged 60 or older at the cut-off date 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. Relative to small laboratory or field experiments, studying the WFP has the advantage that the WFP is an actual transfer received by a large population. Relative to studies of the child benefit, the WFP offers clean identification of a labelling effect.

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 whether the transfer is labelled. This variation in fuel spending and budget share with total expenditure is the "income effect" of standard demand theory. Thus, to provide unambiguous evidence of a labelling effect, we need to be able to distinguish a labelling effect from a standard income effect. Therefore, in our analysis we embed our regression discontinuity design within an Engel curve framework. 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 from the usual "income effect" (as measured by the slope of the Engel curve.)

We find statistically significant and robust evidence of a substantial labelling effect. We estimate that households spend an average of 41% 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 only about 3% of the payment on fuel. We conduct a number of robustness and falsification tests. We carefully test – and reject – the possibility that this

effect arises from non-separabilities between consumption and leisure: the effect we observe cannot be explained by retirements around age 60 altering the demand for heating fuel. We also find a statistically significant effect for both singles and couples, confirming that this is not an intrahousehold allocation effect. 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.

Understanding 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.[7] 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

The Living Costs and Food Survey (LCFS)[8] 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

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

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 LCFS 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 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]

For both eligible and ineligible households, we present summary statistics for the entire subsample, and for the poorest quartile of households (as determined by household total expenditure). Note that relative to the average, poorer households spend less on fuel absolutely, but spend a larger share of their budget on fuel. Fuel is a normal good, and a necessity. These facts are well known, but they play an important role in our empirical design, which we turn to next.

3. Empirical Framework and Estimation

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.[9]

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

The intuition behind an RDD approach is straightforward: households immediately below the cut-off provide evidence on how households immediately above the cut-off would have behaved had they not received the transfer. The identifying assumption is that, in the absence of the transfer, expenditures vary continuously with the forcing variable, 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).[10] Age has previously been used as the exogenous forcing variable in regression discontinuity designs. See for example: Edmonds et al. (2005), Card et al. (2008), Carpenter and Dobkin (2009) and Lee and McCrary (2009).

Testing For Labelling Effects in an Engel Curve Framework

Receipt of WFP might lead to an increase in fuel spending simply because of a standard income effect. In our analysis we need to distinguish a labelling effect from an income effect and to assess whether the WFP is allocated *differently* to how an unlabelled transfer would be allocated. Therefore, we embed a 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 of 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. Thus, an increase in fuel spending, or a decrease in the fuel budget share, with receipt of the WFP might simply represent a standard move *along* the Engel curve – i.e. an income effect; this 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.

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[10] In principle we could also search for an effect at age 80, at which point the WFP becomes more generous. However, in the LCF age has been topcoded at 80 since 2002 which means that we are unable to implement the RDD around age 80.

[FIGURE 1 ABOUT HERE]

To test for a labelling effect, while allowing for standard income effects, we estimate Engel curves which relate budget shares to a function of total expenditure. We allow preferences to evolve continuously with the forcing variable, age of the oldest household member, A_i, by including polynomials in age. We augment this empirical specification with a dummy, D_i, for WFP eligibility. This 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. 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 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 and to capture variation in relative prices. In all specifications, these include household size, month, area, year and area/year interactions. In several specifications we also include employment (of head and, where relevant, spouse), housing tenure, number of rooms and education controls.

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

$$w_{ki} = \alpha + \tau D_i + \beta_1 (A_i - 60) + \beta_2 (A_i - 60)^2 + D_i \cdot \beta_3 (A_i - 60) + D_i \cdot \beta_4 (A_i - 60)^2$$
$$+ \delta^T \cdot f(X_i) + \gamma^T Z_i + e_i \tag{1}$$

where e is an independent (and possibly heteroskedastic) disturbance term and, the dependent variable is the budget share of good k, and $\tau = \lim_{A \downarrow 60} E[w_k \mid A = 60, Z = z, X = x] - \lim_{A \uparrow 60} E[w_k \mid A = 60, Z = z, X = x]$ provides a local estimate of the effect of the WFP on budget shares at age 60, holding total expenditure constant. We estimate this model (and all subsequent models unless otherwise stated) using least squares and report robust standard errors.

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[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.

This specification imposes that the labelling effect on the budget share, if any, is independent of the level of total expenditure.[12] We will test this specification below, and in the appendix, we lay out a more general specification which nests equation (1).

In results 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, 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 minimum income guarantee (i.e. a minimum income guarantee without obligation to seek work). 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).

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[12] Of course, this specification implies that the effect, in any, on pounds of fuel expenditure varies with the level of total expenditure.

[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).

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 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 to 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.

Analysis by sub-group

The discontinuity captured by τ in equation (2) measures the average effect of the WFP at age 60; that is, our base specification does not allow the effect to vary by any household characteristics. Rather than imposing any additional structure, we investigate this further by splitting our sample according to some characteristics and testing for equality of the WFP effect across groups. The variables on which we split our sample are income quartile, season and household structure (within our sample the latter means between single men and couple households).

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 practice 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

Testing For Labelling Effects in an Engel Curve Framework

Table 2 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.

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[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.

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 and number of rooms in the home, 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 and clothing shares become statistically significant at the 10% and 5% level, respectively, when we narrow the age window.

[TABLE 2 ABOUT HERE]

In Table 3 we report the results of additional specification checks for the fuel share. In column (1) we instrument for total expenditure with household income to account for the possibility of measurement error in total expenditure. This has almost no impact on the estimated labelling effect.

In column (2) 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.

[TABLE 3 ABOUT HERE]

Our basic specification imposes that the labelling effect on budget shares, if present, is unrelated to the level of total expenditure and to any other variable. In Table 4 we report the results of relaxing this assumption and allowing the effect to vary by quartile of total expenditure, by season, and by household type. Mostly the coefficients are not precisely estimated, which is to be expected given the now much smaller sample sizes. In none of the three divisions can we reject the null that the coefficients are the same across the groups.

Features to note are that the point estimates in column (1) suggest that the effect *on shares* is larger for poorer households. This does not mean, though, that the absolute labelling effect (on pounds of expenditure) varies this much; a larger share shift at lower total

expenditure could translate into a similar spending effect as a smaller shift at higher total expenditure. We will elaborate on this in the discussion below.

The WFP differs from child benefit in that there is no compelling reason to believe that its effect on spending patterns works through the intra-household distribution of income receipt. First, as noted above, there is reason to think that the intra-household distribution of income receipt is particularly important in the case of spending on children. Second, in the sample of couples we study the male member is always older. Thus at the eligibility threshold for WFP, only the male is eligible and when only one member of a household is eligible for WFP, the transfer is paid to that member.[15] This means that, when implemented on our sample of couples in which the husband is older, our regression discontinuity design studies the effect of a labelled transfer to *husbands*. In the birth cohorts we study husbands were the primary earners and it is implausible that this £250 transfer had a significant effect on the influence those husbands had over household spending patterns. Despite these considerations, it is reassuring to see, in column (3), that the labelling effect is still significant when we split our sample into single men and couples, and indeed marginally more so for single men despite the much smaller size of this group relative to couples. This confirms that the effect we find is indeed a labelling effect and not, instead, an intra-household effect. The point estimate for single men is larger than for couples (although, as stated, not significantly different from each other) but, again, the average total expenditure of this sample of single men is much lower than the couples sample.

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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 (2) of Table 3. 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]

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[15] Where both members of a couple are eligible for the WFP half of the amount is paid to each member. However, for our sample, this is not relevant in at the eligibility threshold, because it is the husband that qualifies initially.

To summarise, we find a positive effect of WFP eligibility on the budget share of fuel, 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.

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 measured in percentage points of budget share (the move from B to C in Figure 1) is τ, then the estimate of the labelling effect measured in pounds of expenditure is approximately.

$$\tau(X + wfp) \qquad\qquad (2)$$

With the results from, say, specification 2 in Table 2 our estimate of the slide along the Engel curve for someone with the average fuel share in 2008 of 0.0613 and total budget of around £308 per week receiving a transfer of £250 a year (so just under £5 a week) is £0.128 with a standard error of 0.010 and a 95% confidence interval around this point estimate of £0.108 to £0.148. Our estimate of the labelling effect is £1.818 with a standard error of 0.623 and 95% confidence interval of £0.600 to £3.037. 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 38% (with a confidence interval of 12% to 63%) so that the overall marginal propensity to spend on fuel associated with the WFP is around 41%.

Equation (2) shows that the absolute labelling effect depends on the estimated size of the discontinuity and on total household expenditure. Therefore, the different shifts estimated by expenditure quartile translate into relatively similar point estimates of additional labelling effects of £1.857, £1.410, £1.475 and £1.446 respectively (although we state again that a test of equality of the WFP coefficient or of the absolute labelling effect is not rejected).

5. 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 in the UK receive in November / December a "Winter Fuel" payment make any difference? Sharp differences in the eligibility requirements allow us to use a regression discontinuity design to examine how fuel expenditure changes on receipt of the benefit. We find a substantial and robust labelling effect. Our estimate of the (average) marginal propensity to spend on household fuel out of unlabelled income is approximately 3%. On average, we find recipient households exhibit an additional marginal propensity to spend on household fuel out of the WFP of between about 12% and 63%, and so the combined effect is between 15% and 66%. 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 £15 and £66 on fuel (our point estimate is £41). 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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Appendix

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

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

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

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

$$\equiv f\left(X_i\right) + \gamma\left(A_i, X_i\right)\varpi_i$$

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

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

We do not have sufficient data to estimate properly how $\gamma\left(A_i, X_i\right)$ might vary with X_i and so, as in addition there is very little variation in ϖ_i, we estimate an average effect, replacing $\gamma\left(A_i, X_i\right)\varpi_i$ with $\gamma\left(A_i\right)\lambda$ where λ is some constant. The only general thing we are prepared to assume about $h\left(A_i\right)$ is that $h_1\left(60\right) = h_2\left(60\right)$ and hence the only age at which we can separately identify $\gamma\left(A_i\right)\lambda$ from $h_2\left(A_i\right) - h_1\left(A_i\right)$ is at age 60 where $h_2\left(A_i\right) - h_1\left(A_i\right) = 0$ (this is basically a restatement of the assumptions underlying the regression discontinuity design as applied to our particular case.)

Figures and Tables

Figure 1: Engel Curve with
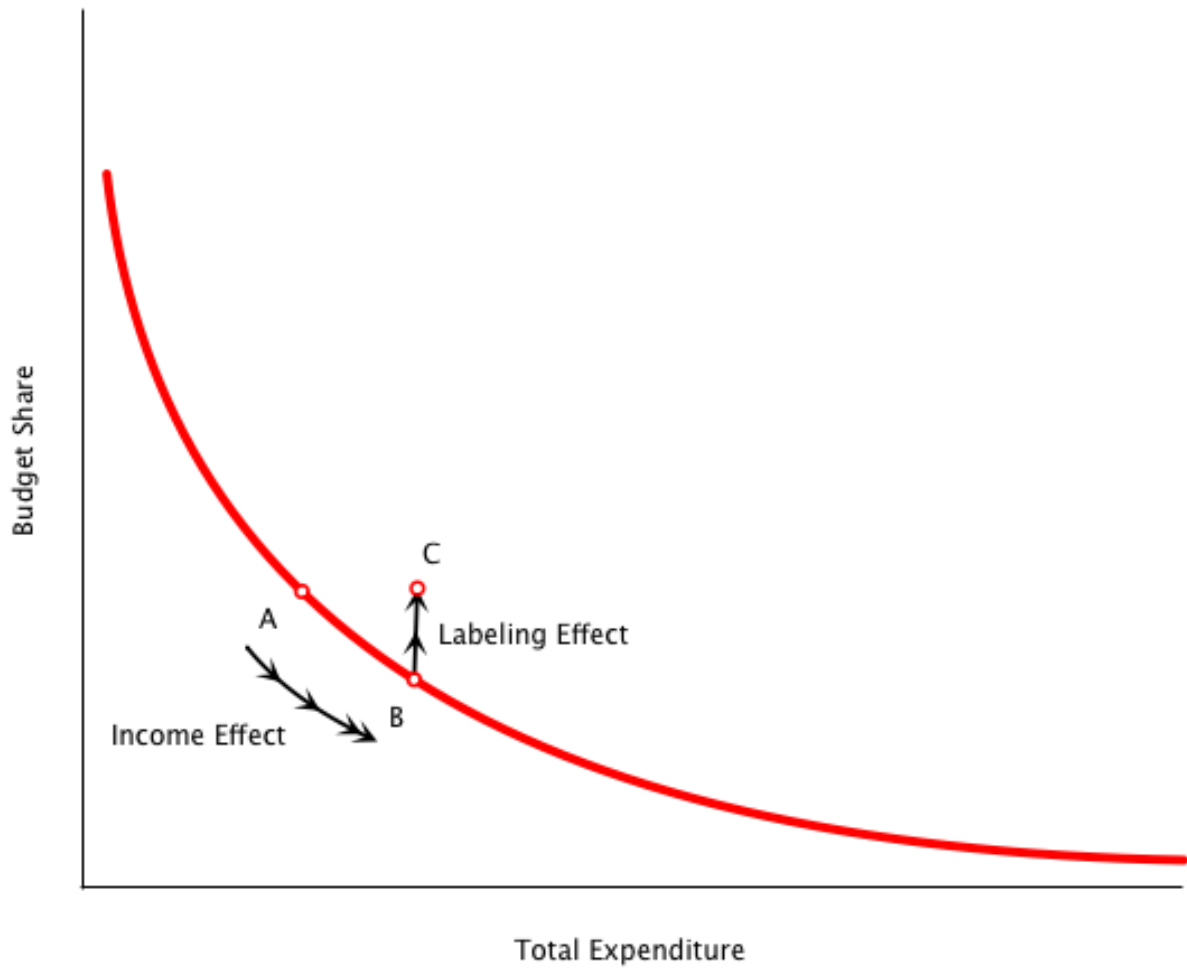
Income Effect and Labelling Effect

Total Expenditure

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

	Ages 45-60		WFP Eligible	
	All	Poorest Quartile	All	Poorest Quartile
Income	531.35	199.63	405.25	244.95
Total expenditure	434.59	124.47	362.42	151.62
Fuel	18.37	11.96	18.79	13.96
Food	44.14	24.74	47.23	34.32
Clothing	13.37	2.01	11.95	3.33
Leisure Goods	14.04	3.67	13.09	5.32
Fuel Share	0.046	0.084	0.055	0.081
Food Share	0.128	0.210	0.162	0.232
Clothing Share	0.033	0.018	0.036	0.025
Leisure Goods Share	0.039	0.037	0.044	0.042
Sample Size	4423	760	6326	1746

Data: Living Costs and Food Survey (LCFS), 2000-2008. Single men and couples without children in which the male is older. The LCFS 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).The poorest quartile is defined by total expenditure.

Table 2. RDD estimates.
Effects of WFP on budget Shares
(conditional on total expenditure)

Shares	(1) OLS	(2) OLS	(3) OLS
Fuel	0.0057**	0.0058**	0.0062*
	(0.0020)	(0.0020)	(0.0025)
Food	-0.0034	-0.0032	-0.0103*
	(0.0038)	(0.0038)	(0.0048)
Clothing	-0.0035	-0.0039	-0.0074†
	(0.0032)	(0.0032)	(0.0040)
Leisure Goods	0.0032	0.0032	0.0057
	(0.0031)	(0.0031)	(0.0040)
Age Window	45-75	45-75	50-70
Data Period	2000-2008	2000-2008	2000-2008
Additional Controls		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. The additional controls are employment (of the head, and where relevant, the spouse), housing tenure, number of rooms and education controls.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors are given in parentheses
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level

Table 3: Further Specification Checks
Effects of WFP on Fuel Budget Share
(Conditional on Total Expenditure)

	(1) IV	(2) OLS
Expenditure Quartile: All	0.0056** (0.0020)	0.0066* (0.0031)
Age Window	45-75	45-75
Data Period	2000-2008	2000-2008 and 1988-1996
Additional Controls MIG	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. The additional controls are employment (of the head, and where relevant, the spouse), housing tenure, number of rooms and education controls.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors are given in parentheses.
4. † = significant at 10% level, * = significant at 5% level, ** = significant at 1% level, *** = significant at 0.1% level

Table 4. RDD estimates for different sub-groups
Effects of WFP on budget Shares
(conditional on total expenditure)

	(1) Expenditure Quartile:		(2) Season:		(3) Household Type:	
	1st	0.0135† (0.0076)	Winter	0.0061 (0.0046)	Single men	0.0105* (0.0052)
	2nd	0.0054 (0.0035)	Spring	0.0068 (0.0045)	Couple	0.0037† (0.0019)
	3rd	0.0037 (0.0028)	Summer	0.0080* (0.0040)		
	4th	0.0020 (0.0023)	Autumn	0.0038 (0.0037)		
F-test of Equality (p-value)		F(3,10129) = 0.81 (0.49)		F(3,10165) = 0.21 (0.89)		F(1,10433) = 1.55 (0.21)
Age Window		45-75		45-75		45-75
Data Period		2000-2008		2000-2008		2000-2008
Additional Controls		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. The additional controls are employment (of the head, and where relevant, the spouse), housing tenure, number of rooms and education controls.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors are given in parentheses.
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	0.0029 (0.0024)	0.0000 (0.0022)	-0.0016 (0.0023)
Age Window	45-75^6	45-75^6	45-75
Data Period	2000-2008	2000-2008	1988-1996
Additional Controls	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. The additional controls are employment (of the head, and where relevant, the spouse), housing tenure, number of rooms and education controls.
2. The age window pertains to the oldest person in the household.
3. Robust standard errors are given in parentheses.
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
6. Rebalancing the sample (for example changing the age window around 55 to be 40-70) also yields insignificant results.