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The Impacts of Free Universal Elderly Care on the Supply of Informal Care and Labour Supply*

BRUCE HOLLINGSWORTH,† ASAKO OHINATA,‡
MATTEO PICCHIO§,¶,#,†† and IAN WALKER#,‡‡

†Health Economics at Lancaster, Division of Health Research, Lancaster University, HiOne, Sir John Fisher Drive, Lancaster LA1 4YW, UK
‡Department of Economics, Accounting, and Finance, University of Leicester, Leicester LE1 7RH, UK (e-mail: ao160@le.ac.uk)
§Department of Economics and Social Sciences, Marche Polytechnic University, Piazzale Martelli 8, 60121, Ancona, Italy
¶Department of Economics, Ghent University, Sint-Pietersplein 5, 9000, Ghent, Belgium
#IZA Institute of Labour Economics, Schaumburg-Lippe-Straße 5-9, 53113, Bonn, Germany
††GLO – Global Labor Organization, Leimkugelstr. 6, 45141, Essen, Germany
‡‡Lancaster University Management School, Bailrigg, Lancaster LA1 4YX, UK

Abstract

This paper investigates the impact of introducing universal free formal personal care on informal caregiving behaviour in Scotland – in particular, we explore the extent to which free formal care might crowd out the supply of informal care. We estimate, in a difference-in-differences framework, that such a reform would: reduce the probability of co-residential informal caregiving (usually, provided by spouses) by around 18% and, conditional on co-residential caring, reduce such informal care by 1.3 hours per week. These estimates suggest that an additional hour of formal care displaces approximately 1 hour of such informal care. However, we find no displacement effect on extra-residential informal caring (often supplied by adult daughters). We also find evidence of increases in labour market participation and hours worked.

I. Introduction

Medical progress has driven longevity faster than it has driven healthy life years. Longer lifetimes imply a greater incidence of conditions that require long-term care services – for example, dementia. Informal care is an important input into elderly care.

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But the extra-residential supply of such care is falling because of lower fertility rates, rising labour market participation rates of women, and greater geographical mobility within families. Moreover, co-residential care supply is usually provided by spouses and the rise in separation rates of couples implies that elderly people are increasingly finding themselves living alone for longer periods of their later lives.

Most individuals require intensive forms of care for only a relatively short period, but a significant minority are likely to require such care for a period of several years or longer. It is usually impossible to predict, early in life, whether one is likely to be in the minority that requires long-term care. The elderly strongly prefer independent living to being cared for in a residential institution, and this is also the far cheaper option. Home-based care is the setting where informal care may be most easily substituted by formal personal care. Therefore, the relationship between formal and informal care, and the care and labour supply responses of informal carers to policy changes, is an important issue – the issue that we address in this paper.

We rely on the Scottish Community Care and Health Act (CCHA) of 2002 which offered in-kind formal personal care support to individuals with care needs, without means-testing. Scotland and the rest of Britain have similar cultural and institutional settings where most policies are common across the two regions because they are developed and enacted by the UK government in London. This suggests that England and Wales is likely to be a suitable control group for Scotland – a hypothesis that we test and are unable to reject. It is also worth noting that using England and Wales as the control group for Scotland has been adopted by other papers that study such things as: the impact of hospital performance management, dental check up and a smoking ban policy (e.g. Adda, Berlinski and Machin, 2007; Propper *et al.*, 2010; Ikenwilo, 2013). We use the 1998–2007 UK Family Resources Survey (FRS) and employ a difference-in-differences (DD) strategy. We investigate how the reform changed informal personal care behaviour, both at the intensive margin (i.e. number of hours of care supplied) and at the extensive margin (i.e. whether to supply informal care or not). In addition, we study how the policy affected the caregivers' paid labour supply at the extensive and intensive margins.

Many OECD countries offer in-kind formal long-term care financed either via taxation (e.g. Australia, Austria, Belgium, Canada and the Nordic countries) or through a national insurance scheme (e.g. Germany, the Netherlands, Japan and Korea). In some cases this in-kind supply is means-tested, either according to income or financial assets, and in most cases the extent of care available is subject to a needs assessment. The Scottish reform provides an important opportunity to study the effect of a formal care price reduction on informal care supply. The funding of long-term care in England is a complex and highly topical issue. Recent proposals suggest a hypothecated income-related levy could be used to lower lifetime costs for some (mostly better-off) elderly people. Thus, the work is relevant to an important, widespread and persistent policy issue.

Our study contributes to the existing literature, on the impact of financial support for formal care on informal caregiving, in the following ways. First, since the support offered is not means-tested, we can investigate the response over the population of those needing care, unlike many of the previous papers that focus on only those needing care in low-income households. Second, unlike previous papers, this paper studies the impact on both carer labour supply and the supply of care itself. Finally, this paper employs a dataset that includes many times more caregiver observations. Using this larger sample size, and the information included in FRS on the relationships between the care givers and the recipients, we can explore in detail how the policy impact differs across different groups of families. Moreover, having a large sample allows us to study the policy effects among those who provide care to a family member in the same house (i.e. co-residential care) as well as to a member of a different household (i.e. extra-residential care). Since the intensity and types of care are likely to differ between these two settings, the responses to changes in the supply of formal care supply may also differ.

We find large and well-determined behavioural effects, relative to the baseline. We also find that the new policy reduced the probability of co-residential care (typically, such care is provided between partners) by around 18%. Conditional on giving such co-residential care, the reduction in the number of hours of informal care induced by the reform amounts to 1.3 hours per week. We estimate that the effects are stronger among older and less educated caregivers. In contrast, we find that the supply of informal extra-residential care (typically by the adult child of an elderly parent) did not respond significantly to the policy. It seems likely that this difference between co-residential and extra-residential care effects relates to differences in the nature of the care provided in the different settings. For example, extra-residential support is likely to involve companionship and help with non-personal matters such as financial affairs and shopping, while co-residential care is likely to incorporate more significant elements of personal care. In addition, we estimate positive effects on labour force participation and on hours of work, conditional on participation. We estimate an average increase in the hours of work of 0.4 per week. Notably, the largest increases in the labour force participation and the conditional hours of work were found for those aged 55 and above.

II. Literature review

The existing literature typically shows that there is a negative correlation between informal care and the supply of labour to the market. Some of this work attempts to identify a causal effect assuming that family characteristics are valid instruments for formal supply (e.g. Ettner, 1995, 1996; Carmichael and Charles, 2003; Heitmueller, 2007; Bolin, Lindgren and Lundborg, 2008). Other work relies on controlling for individual fixed effects (e.g. Leigh, 2010; Van Houtven, Coe and Skira, 2013). However, the evidence on our precise question of whether public financial support for elderly care affects the level of labour supply is scarce. An important exception is Løken, Lundberg and Riise (2016), which uses the 1998 federal grant program in Norway that increased the amount of home care provision in selected municipalities. That paper finds that the policy drove statistically significant, and relatively large, reductions in the incidence of sickness absence, and days of sickness absence, even though there was no significant effect on the probability of working. We contribute to the limited literature on this topic by presenting more direct evidence on labour supply based, at intensive and extensive margins, on the Scottish reform.

Only a handful of studies investigate the effects of government support for formal care on informal caregiving behaviour at home, and often only for small sub-populations. For example, Ettner (1994) shows that Medicaid subsidy for formal care, offered to low income households in the United States (US), leads to substitution away from informal care. Similarly, Pezzin, Kemper and Reschovsky (1996) and Arntz and Thomsen (2011) each exploits an experiment that offers either in-kind or cash benefits to individuals in five US communities and six German counties respectively. The former oversampled lower income households while the latter was underpowered by virtue of its limited sample size. They both find that cash benefits induce substitution between informal and formal care but Arntz and Thomsen (2011) report a larger effect for in-kind support.[1] Our paper is also related to the literature that exploits variations in the availability of informal care, and their impacts on the use of formal care, to show the extent of substitutability between informal and formal care. (See, e.g. Van Houtven and Norton (2004, 2008); Bolin *et al*. (2008); Bonsang (2009)).

There are two papers that also investigate the effect of the 2002 Scottish policy on informal caregiving: Bell, Bowes and Heitmueller (2007) and Karlsberg Schaffer (2015). Both use the British Household Panel Survey (BHPS) and both implement the same DD approach used here. However, neither paper implements any tests of the usual DD assumptions. Moreover, these papers rely on small slices of a relatively small dataset that forces them to aggregate their definitions of caring.

Bell *et al*. (2007) uses a linear probability model with fixed effects and examine the effect of the policy on co-residential and extra-residential care separately. They use BHPS data from 1999 to 2003, which is only one year after the policy introduction, and they find no statistically significant effects. However, this work is underpowered to the extent that not only is the treatment interaction variable statistically insignificant, so too are the Scottish and postreform dummy variables. Moreover, as BHPS does not differentiate between childcare and long-term elderly care, inclusion of all individuals aged between 21 and above prevents the authors from isolating the impacts on the long-term elderly care. A decrease in the probability of providing extra-residential care was also found, although this result was of a small magnitude and was also statistically insignificant. The authors also depict the before and after changes in the binned distribution of hours of care and there is a suggestion that there was a relative reduction in the intensity of informal care provided in Scotland relative to England but no tests are provided.

Karlsberg Schaffer (2015) covers a longer time horizon (1996 to 2008) and considers the supply of informal care separately for men and women but aggregates co-residential with extra-residential care in order to overcome their small sample size. The paper does test the validity of this aggregation by comparing the coefficients for the two separate forms of care. However, the lack of precision in these coefficients

[1]Costa-Font, Jiménez-Martín and Vilaplana (2018) study the effect of a national-level cash subsidy on informal caregiving in Spain. They find that frail individuals increased intergenerational transfers to their family members, and family members increased their informal caregiving.

casts doubts on their failure to reject the null that the effects are equal. The paper finds a positive impact of the policy on the probability of informal caregiving (defined as supplying more than 4 hours per week) by both men and women – although only the latter is statistically significant. It is notable that the effect appears to come from a fall in the probability of informal caring in England rather than a rise in Scotland.[2,3]

Our use of a large pooled cross-section data allows us to resolve the imprecision in existing work and to test the validity of the identification strategy. Moreover, it also allows us to consider differences between co-residential and extra-residential supply, separately by the gender of the carer.

III. Context

Formal personal care costs in the United Kingdom prior to 2002 exposed individuals to significant risk. As an example, an individual receiving formal personal care in England in 2001 on average paid £ 4,742 per year, which was considerably greater than the annual amount of the basic state pension (£ 3,770) at that time. Such care refers to non-medical support offered to frail individuals to facilitate their daily activities. Importantly, such care does not require medical training and are often provided by family members who act as informal caregivers. The Royal Commission on Long-Term Care for the Elderly was set up, by the UK Labour government in December 1999, to address such financial uncertainty. The resulting Sutherland Report recommended that formal personal care for those aged 65+ should be provided free of charge, subject to rigorous assessments of individual care needs.

Scotland set up a Care Development Group in January 2001 to investigate the estimated cost of introducing such a policy and to make arrangements for implementation. The resulting proposal received Royal Assent on 12 March 2002 to become the CCHA. CCHA offers in-kind personal care worth up to £ 145 per week to those receiving care in their own homes. Table 1 lists the types of personal care defined by the Scottish Government. The reform was made possible in Scotland because it had acquired the power to set its own social care policies as the result of the 1999 devolution arrangements within the United Kingdom, that devolved some areas of policymaking from London to Scotland.[4] However, the rest of United Kingdom chose *not* to adopt the recommendations made by the Sutherland report and continued to charge for formal personal care.

The amount of personal care the individual is entitled to is determined by a needs-based assessment carried out by a social care worker sent by the local authority where

[2]McNamee (2006) presents descriptive evidence of increased formal personal care in Scotland.

[3]The details of Bell *et al*. (2007) and Karlsberg Schaffer (2015) are summarized in the Online appendix, section C.

[4]The 1999 devolution of powers to the Scottish government allowed it to set its own policies in the areas of education, housing, health and social services. During the period after devolution, two other flagship policies, aside from the CCHA, were introduced in Scotland. These were the abolition of University tuition fees, and policies to reduce homelessness. However, since the most important effects we observe in our paper are largely from people looking after elderly spouses, these additional policies are unlikely to have affected their care behaviour. In addition, the UK government retained the right to set policies related to employment and retirement (Keating *et al*., 2003) and these remained unchanged.

TABLE 1

Types of formal personal care defined by the Scottish Government

Personal Hygiene	Assisting bathing, showering, hair washing, shaving, oral hygiene, nail care
Continence Management	Assisting toileting, catheter/stoma care, skin care, incontinence laundry, bed changing
Food and Diet	Assistance with the preparation of food and the fulfilment of special dietary needs
Problems with Immobility	Dealing with the consequences of being immobile or substantially immobile
Counselling and Support	Behaviour management, psychological support, reminding devices
Simple Treatments	Assistance with medication (e.g. eye drops, application of lotions), oxygen therapy
Personal Assistance	Assistance with dressing, surgical appliances, prostheses, mechanical and manual aids. Assistance to get up and go to bed.

Notes: "Free Personal and Nursing Care" retrieved from http://www.gov.scot/Topics/Health/Support-Social-Care/Support/Older-People/Free-Personal-Nursing-Care (2017, May 03).

the individual resides. During the assessment, the social care worker evaluates whether the individual requires any of the care support listed in Table 1, as well as the home environment, the client's mental well-being, physical well-being and medical background (Scottish Executive, 2004).

The average weekly amount of care provided to those living at home in 2003, under CCHA, was equivalent to a cash value of £ 80 (National Statistics, 2013). This implies that those living in Scotland on average received £ 4,160 worth of formal personal care in a year (see Online Appendix, section B, for further details on the policy).

IV. Data, sample and variable definitions

This study employs the repeated cross-sections of the UK Family Resources Surveys (FRS). FRS has been collected by the Department for Work and Pensions on an annual basis since 1992. Every year approximately 45,000 individuals in around 24,000 private households are interviewed, and the information is collated at the household, benefit unit (defined either as an individual, or as a couple with or without dependent children), and individual levels. FRS asks individuals to report whether they look after anyone, either inside or outside the household, and their relationship with the care recipient. If they provide informal care, FRS also asks how many hours of care they offer.[5]

[5] In order to ensure that the respondents understand what FRS considers as informal care, they are shown a card that lists various care activities (see Table A.1 in the Online Appendix). There are strong similarities in the definitions of personal care reported in Table 1, that the Scottish government uses, and those in Table A.1, used by FRS. This points to the substitutability between the informal care activities with the formal care offered by the Scottish policy. There are several care activities listed in the FRS card such as 'keeping company' or 'helping with paper work' – these are activities that the Scottish policy does not provide for. These two types of care are likely to have worked as complements. Taking this into account, our estimates are likely to show the lower bounds (i.e. values closer to zero than would have been in the case had the two sources of care been perfect substitutes).

We have several outcomes of interest. First, an indicator variable that equals 1 if an individual gave informal care to an adult aged 60 or above.[6] Second, an interval-coded variable measuring the number of hours per week of informal care given to someone aged 60 or above. In addition, two more dependent variables are defined based on whether individuals provide co-residential or extra-residential informal care.[7] Further outcome variables measure labour supply, both at the intensive and the extensive margins: we use an indicator variable that equals 1 if the individual is employed, and the number of weekly working hours conditional on working. Our analysis starts from 1998 since all the relevant dependent and independent variables are available only from this year. We employ data up to 2007, since the 2008 financial crisis may have led to asymmetric impacts across regions on individual time endowments and their labour supply. We exclude Northern Ireland from our analysis because FRS does not collect data from that devolved region prior to the 2002/2003 survey. We further restrict the sample to include those older than 25 to reduce the chance of including individuals who are still in formal education. After imposing these conditions, and dropping those observations with missing data, the final sample size is 399,098. When the outcome variable is labour force participation, we further restrict the sample to those aged between 25 and 74 years of age, who report less than 60 weekly working hours, who are not retired, not students, not permanently or temporarily sick/disabled.[8] In this case, the resulting sample size is 254,402. We employ a DD framework to estimate the policy effects in Scotland (the treatment group) in comparison to England and Wales (the control group). Using the month and year of interview from our data, we define the post policy period to start at March 2002, when the Bill become law.

Table 2 presents descriptive statistics of the outcome variables before and after March 2002 in the treatment/control regions as well as the DD of the raw means.[9] It is important to define informal care precisely, and to measure the number of hours of informal care provided accurately. The FRS interview is designed to standardize the type of activities that are considered as informal care so as to provide comparable measurements of the hours of care provided. Banded hours of caregiving is collected after allowing the respondent the opportunity to read a show card that describes

[6]We focus on this age group even though the policy affected those aged 65 and above for the following reasons. First, informal carers may have changed their behaviour in anticipation of the available funding in the near future even before the care recipients reached the eligibility age. Second, it is also possible that the policy may have shifted the caregiving from a household member older than 64 to a younger member. However, we also report results when we restrict the age of care recipients to be 65 or above.

[7]It is important to point out that our regression analysis on both co-residential or extra-residential care is conducted using the entire sample and is not carried out by dividing our sample based on whether the caregivers were living with their care recipients.

[8]The labour supply related sample is restricted to include those aged between 25 and 74 because the majority of individuals retire by the age 75 and we do not observe any variation in the labour supply variable beyond the age. The sample for caregiving regressions does not eliminate the age group as the caregiving activities (typically between spouses) take place mainly at older ages. As a robustness check, we present the caregiving estimates obtained by restricting the sample to those aged 25 and 74 in Tables H.10 and H.11 in the Online Appendix. Our conclusions remain the same.

[9]Figures on the number of weekly hours of co-residential and extra-residential informal caregiving are not included in Table 2, because of its interval-coded nature. See Online Appendix D for more information on how the hours of care variables are defined.

TABLE 2

Outcomes before and after the reform for Scotland and England/Wales

	Scotland			*England & Wales*			*Difference-in-differences*		
	Mean	*SD*	*Observations*	*Mean*	*SD*	*Observations*	*Mean*	*SE*	*Observations*
(a) Informal care giver (co-residential)									
Before: 1998-2001	0.0222	0.1472	13,626	0.0219	0.1730	141,261			
After: 2002-07	0.0172	0.1300	41,687	0.0210	0.1691	202,524			
Mean difference after–before	−0.0049**	0.0013	55,313	−0.0009*	0.0005	343,785	−0.0040***	0.0010	399,098
(b) Informal care giver (extraresidential)									
Before: 1998-2001	0.0462	0.2098	13,626	0.0501	0.2180	141,261			
After: 2002-07	0.0472	0.2121	41,687	0.0504	0.2190	202,524			
Mean difference after–before	0.0010	0.0021	55,313	0.0003	0.0010	343,785	0.0007	0.0010	399,098
(c) Employment indicator									
Before: 1998-2001	0.8251	0.3799	8,370	0.8217	0.3870	91,122			
After: 2002-07	0.8677	0.3388	25,111	0.8470	0.3600	129,799			
Mean difference after–before	0.0426***	0.0044	33,481	0.0253***	−0.0016	220,921	0.0173***	0.0050	254,402
(d) Weekly working hours									
Before: 1998-2001	30.9496	18.0333	8,370	30.8794	18.4944	91,122			
After: 2002-07	32.3505	16.6316	25,111	31.5670	17.6331	129,799			
Mean difference after–before	1.4009***	0.2145	33,481	0.6876***	0.0778	220,921	0.7130***	(0.138)	254,402

Notes: In this table, we present the summary statistics of the outcome variables before and after the reform for Scotland (treatment) and England/Wales (control). *** Significant at 1%. SD and SE stand for standard deviation and standard error respectively.

examples of the relevant kinds of help (Department for Work and Pensions, 2002, p. 102).[10]

Panel (a) of Table 2 shows that the reduction in the proportion of co-residential informal caregivers is larger in Scotland: −0.5% points in Scotland compared to −0.1% points in England and Wales. The rightmost block of figures in Table 2 show the estimated DD based on regressions that exclude any covariates. In the case of co-residential informal care in panel (a) this amounts to −0.4% points and this estimate is significantly different from zero; while in panel (b) we provide an estimate of the extra-residential care supplied to parents which is not statistically significant. Comparing the employment status and working hours before and after 2002 in panels (c) and (d), we see that Scottish labour market participation rose, as does the hours of work, compared to those in England and Wales. The weekly working hours rose by 4.3% (1.4 hours) in Scotland, the counterparts in England and Wales rose by only 0.69 hours and the resulting estimated DD of 0.72 hours is highly significant. Similarly, the labour force participation rate in Scotland rose by 0.04% points while the rate in England rose by 0.025, yield a DD of 1.7% points that was also statistically significantly different from zero. In the raw data, therefore, we find some evidence suggesting that the co-residential informal caregiving and labour force participation behaviour changed in Scotland compared to England and Wales after 2002. In the multivariate analysis that follows we check the extent to which this evidence from the raw data remains after controlling for a rich set of time-varying and time-invariant determinants of the outcome variables. We also explore the possibility of heterogeneity across different regions caused by the changing economic and social environment.

Table 3 reports summary statistics for the outcome variables for co-residential caregiving and labour force participation by gender, age and education. These statistics reveal that men are almost equally likely to give co-residential informal care, perhaps because such care is commonly between spouses. On the other hand, women are more likely to provide informal care to support extra-residential individuals, while men are more likely to work in the paid labour market, and to provide longer hours of paid work. In addition, we see that individuals who left education before age 16, as well as those aged 55 and above, are more likely to give co-residential informal care, but the opposite is true for extra-residential caregiving. It is notable that, although the patterns in the data are similar across Scotland and England and Wales, the Scottish levels of care, for each slice of the data, are consistently lower than in England and Wales. Employment rates and hours of work are generally slightly higher in Scotland, with the exception of hours of work for men.

[10]This show card contains the information that is reported in Table A.1 of the Online Appendix.

TABLE 3

Means of the dependent variables by gender, education and age

	Co-residential informal care giver		*Extra-residential informal care giver*		*Employment indicator*		*Weekly working hours*	
	Scotland	*England & Wales*	*Scotland*	*England & Wales*	*Scotland*	*England & Wales*	*Scotland*	*England & Wales*
By gender								
Men	0.017	0.020	0.033	0.038	0.907	0.914	38.585	39.110
Women	0.020	0.022	0.058	0.062	0.813	0.767	26.173	24.251
By education								
Left education < 16	0.030	0.040	0.036	0.044	0.809	0.784	29.163	27.922
Left education ≥ 16	0.011	0.012	0.054	0.054	0.871	0.850	32.832	32.133
By age								
Age is [25,55)	0.006	0.007	0.060	0.057	0.864	0.846	32.802	32.273
Age is 55 and above	0.035	0.041	0.030	0.040	0.824	0.794	28.463	26.902

Notes: The table shows the average values of the dependent variables by demographic characteristics separately for England/Wales and Scotland. The data are pooled across all years from 1998 to 2007 and the data refers to the survey week. For the informal co-residential caregiving sample, the age range is restricted to be between 25 and above. The employment related estimates are based on the sample of individuals aged between 25 and 75.

V. The econometric model

Our empirical work adopts repeated cross-section data to estimate the policy effect using a canonical DD strategy.[11] We specify the following model for a generic outcome variable y for the ith individual in region r and tax year t

$$y_{irt} = \mathbf{x}'_{irt}\boldsymbol{\beta} + \boldsymbol{\gamma}_r + \boldsymbol{\phi}_t + \delta_{DD} I_{rt} + \epsilon_{irt}, \tag{1}$$

where the t is the tax year (April 6th until the following April 5th). We choose fiscal year since the postintroduction period closely corresponds to the beginning of the 2002/3 tax year. I_{rt} is the treatment of interest. This is an indicator variable equal to 1 if observation i resides in Scotland and is surveyed after the reform, March 2002.

[11]We adopt the DD strategy where there is a single treatment that is introduced to a single treatment group (Scotland) but not to the control group. In this simplest of frameworks, the DD strategy provides non-parametrically identified estimates of the average treatment effect using pooled cross-sections of data. Thus, we avoid the difficulties associated with time-varying treatments that has been the subject of recent developments (see, e.g. De Chaisemartin and d'Haultfoeuille, 2018; Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2021). Moreover, the validity of the common trends assumption is sufficient to ensure that any confounding factors are removed by differencing. Thus, under these circumstances and if the sample composition is stable over time, there is no advantage from using panel data in DD estimation. Indeed, ordinary least squares (OLS) estimation using pooled cross-sections will be more efficient than fixed effects (FE) estimation using panel data. Lechner, Rodriguez-Planas and Fernández Kranz (2016) show that the OLS and FE estimates using a DD approach may differ when there is time-varying panel non-response and, in general, OLS may be preferable, because it is likely to be more precise than the FE estimator.

Under certain assumptions described in section (Identification assumptions), the corresponding DD parameter, δ_{DD}, therefore provides estimates of the average treatment effects on the treated of the introduction of free personal care in Scotland. $\mathbf{x}_{irt}$ is the $K{\times}1$ vector of relevant individual characteristics and $\boldsymbol{\beta}$ is the corresponding vector of coefficients. The regressors in $\mathbf{x}_{irt}$ are: gender, marital status, age (and age of the spouse, if present), race, education (and education of spouse, if present), and a series of variables describing the household composition.[12] We also include a set of time-varying regional-level variables (activity rate by gender, per capita gross value added, and its variation). $\boldsymbol{\gamma}_r$ is a set of regional fixed effects. $\boldsymbol{\phi}_t$ is a set of tax year fixed effects. Finally, ϵ_{irt} is the individual level residual. Summary statistics of the control variables are reported in Online Appendix Table H.4.

The parameters of equation (1) are, depending on the outcome variable, estimated either by OLS or by interval regression. Applying OLS to the case of informal caregiving and labour force participation implies that we are estimating linear probability models for these outcomes. When we estimate the equations for informal caring we adopt interval regression. We do not impose any restrictions on the modelling of the policy effects – which are identified entirely from the DD strategy. Thus, we do not impose any restrictions on what the covariances between the residuals in each equation might be. In linear models, we estimate standard errors robust to heteroscedasticity. Although the regressor of principal interest is correlated within the cluster (i.e. region), in our framework, inference cannot take this into account easily. More discussions on the statistical inferences can be found in Online Appendix F, where we explain the problem of using the cluster-robust variance estimator (CRVE) as a way to deal with correlation within-groups (Liang and Zeger, 1986) or the wild cluster bootstrap-*t* procedure proposed by Cameron, Gelbach and Miller (2008) in our set-up.

The number of hours of informal care is interval coded and has a sizeable mass of observations at zero. We model this interval-coded variable using a generalization of the type-I Tobit model. In the case of estimating interval regressions, we assume that equation (1) represents the latent number of hours of caregiving if it were observable. We also assume that the error term has a zero-mean normal distribution with variance σ^2. These assumptions allow us to derive the probabilities of observing the realization of the latent variable being equal to zero (corner solutions), larger or smaller than an observed cut points, and between two observed cut points. The sample density is fully determined by these response probabilities up to a finite number of parameters (the parameters in equation (1) and σ) and, therefore, the model can be estimated by maximum likelihood. If we summarize the deterministic variation across observations as $w_{irt} \equiv \mathbf{x}'_{irt}\boldsymbol{\beta} + \boldsymbol{\gamma}_r + \boldsymbol{\phi}_t + \delta_{DD} I_{rt}$ then the contribution to the sample log-likelihood, of

[12]These are the number of household members, the number older than 64, and the number of dependent children that capture living arrangements and are important to include to avoid spurious findings due to possible changes in arrangements induced by the policy. In particular, the reform might have incentivized frail individuals to live alone, leading to a change in the household composition. If the household composition were not controlled for this would generate a correlation between the error term and the policy dummy I_{rt}.

observation i living in r at t with observed hours of caregiving in the interval $(c_i^{j-1},\ c_i^j]$, is given by:

$$\ell_{irt} = \begin{cases} \log\left\{\Phi[(c_i^j - w_{irt})/\sigma]\right\}, & \text{if } c_i^{j-1} = 0 \text{ and } y_{irt} \leq c_i^j; \\ \log\left\{\Phi[(c_i^j - w_{irt})/\sigma] - \Phi[(c_i^{j-1} - w_{irt})/\sigma]\right\}, & \text{if } c_i^{j-1} < y_{irt} \leq c_i^j; \\ \log\left\{1 - \Phi[(c_i^{j-1} - w_{irt})/\sigma]\right\}, & \text{if } y_{irt} > c_i^{j-1} \text{ and } c_i^j = +\infty; \end{cases} \tag{2}$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function. As is the case with the LPM modelling, the policy effects are identified only from variation induced by the reform and we are not imposing any restrictions on the covariances between the residual across the interval regression equations.

Identification assumptions

The identification of the policy effects using a DD strategy is based on three assumptions.

Assumption 1 (Parallel trends assumption). Conditional on $(\mathbf{x}_{irt},\ \boldsymbol{\gamma}_r,\ \boldsymbol{\phi}_t)$, individuals residing in Scotland would experience similar trends in the outcome variables as those in the rest of the United Kingdom if the 2002 reform was not implemented.

Assumption 1 is supported by our comparison of the trends in care supply of England/Wales vs. Scotland. We estimate equation (3), which regresses each outcome variable against all the covariates discussed earlier together with a set of time dummies whose coefficients are allowed to be different between Scotland and England/Wales:

$$y_{irt} = \mathbf{x}'_{irt}\boldsymbol{\omega} + \boldsymbol{\gamma}_r + \boldsymbol{\phi}_t^{EW} + \boldsymbol{\phi}_t^{Sc} + u_{irt}, \tag{3}$$

where $\boldsymbol{\phi}_t^{EW}$ where $\boldsymbol{\phi}_t^{S}$ are tax year dummies if individual i lives in England-Wales or Scotland respectively. The estimated coefficients on these dummy variables are plotted in Figure 1, and the estimates themselves can be found in Appendix G, Tables G.6–G.8.[13]

In Panels (a), (d) and (g) of Table 4, we present results from tests to assess the validity of parallel trends. To do this, we jointly test if, $\boldsymbol{\phi}_t^{Sc} - \boldsymbol{\phi}_t^{UK} = k$, where k is some constant, $\forall t = 1998, \ldots, 2001$. If the null hypothesis cannot be rejected, then the distance between the Scottish and British trends would be constant, that is, the trends are parallel before the reform. The P-values shown in these panels cannot reject that the prereform trends are parallel. Panels (b), (e) and (h) in Table 4 report a second test, which is performed by including the lead of order one, two and three of the policy indicator I_{rt} and testing the significance of the associated coefficients. This placebo

[13]Figure 1 presents results that are very similar to what we would have gotten with an event-study analysis with the only difference of the normalization. Event-study analysis normalizes the 2001 value (i.e. a year before the policy introduction). We instead normalized the initial Scottish value in 1999.

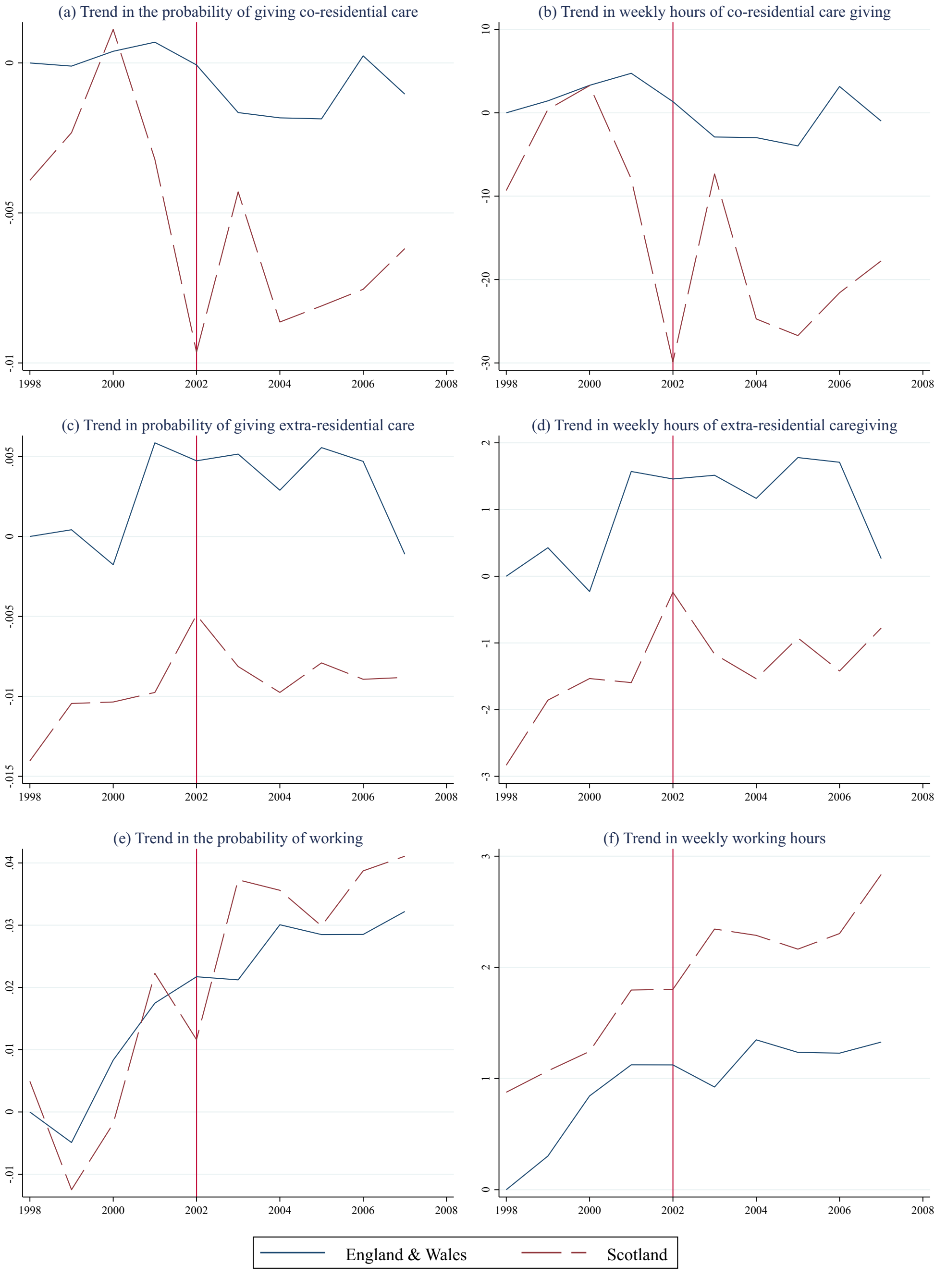

Figure 1. Testing the assumption of parallel trends in the outcome variables
Notes: We report the least squares estimates (or interval regression estimates if the dependent variable is the number of hours of caregiving) of the year dummies for Scotland and England-Wales. We obtained them by regressing each outcome variable on a set of time dummies whose coefficients are allowed to be different between Scotland and England-Wales and, as further control variables, all the other regressors listed in Table G.3. The reference time dummy is 1998 for England-Wales

TABLE 4

Identification assumption tests

	Linear probability model for co-residential care giving		*Interval regression for hours of co-residential care giving*	
	Coeff.	P-*value*	*Coeff.*	P-*value*
(a) Test of parallel pretrend		0.574		0.645
(b) Placebo test: the 2002 policy reform in previous years				
$After_{t+1}$*Scotland	−0.004	0.218	−11.949	0.297
$After_{t+2}$*Scotland	0.003	0.420	1.611	0.887
$After_{t+3}$*Scotland	0.002	0.684	7.526	0.532
Test of joint significance		0.591		0.694
(c) Placebo test: the 2002 policy reform in other regions				
After*North†	0.001	0.499	5.853	0.449
After*Center‡	−0.001	0.530	−0.239	0.973
After*South§	0.001	0.375	5.173	0.419
Test of joint significance		0.190		0.413
Observations	399,098		399,098	
	Linear probability model for extra-residential care giving		*Interval regression for hours of extra-residential care giving*	
	Coeff.	P-*value*	*Coeff.*	P-*value*
(d) Test of parallel pretrend		0.551		0.550
(e) Placebo test: the 2002 policy reform in previous years				
$After_{t+1}$*Scotland	−0.007	0.191	−1.826	0.175
$After_{t+2}$*Scotland	0.003	0.605	1.157	0.403
$After_{t+3}$*Scotland	0.003	0.594	0.462	0.746
Test of joint significance		0.568		0.548
(f) Placebo test: the 2002 policy reform in other regions				
After*North†	0.001	0.717	0.344	0.705
After*Center‡	0.004	0.157	0.900	0.294
After*South§	0.002	0.337	0.467	0.532
Test of joint significance		0.387		0.584
Observations	399,098		399,098	
	Linear probability model for employment		*Hours of work*	
	Coeff.	P-*value*	*Coeff.*	P-*value*
(g) Test of parallel pretrend		0.433		0.831
(h) Placebo test: the 2002 policy reform in previous years				
$After_{t+1}$*Scotland	0.015	0.175	0.293	0.556
$After_{t+2}$*Scotland	−0.003	0.835	−0.326	0.545
$After_{t+3}$*Scotland	−0.013	0.300	−0.139	0.796
Test of joint significance		0.413		0.847
(i) Placebo test: the 2002 policy reform in other regions				
After*North†	0.002	0.805	0.120	0.716
After*Center‡	0.001	0.916	0.128	0.726

(*Continued*)

TABLE 4

(*Continued*)

	Linear probability model for employment		*Hours of work*	
	Coeff.	P-*value*	*Coeff.*	P-*value*
After*South[§]	−0.006	0.357	−0.073	0.810
Test of joint significance		0.287		0.655
Observations	254,402		254,402	

Notes: *** Significant at 1%; ** significant at 5%; * significant at 10%. All the regressors included in the baseline models are also included in these models. The corresponding estimated coefficients are not reported for the sake of brevity and are available from the authors upon request.

[†] In the North, we include North-West, North-East and Yorkshire and the Humber.

[‡] In the Centre, we include Wales, West Midlands, and East Midlands.

[§] In the South, we include South-West, South-East and Eastern.

test, envisages that the 2002 Scottish policy was implemented 1, 2 or 3 years prior to 2002. The insignificance of these test results also support the plausibility of the parallel trends assumption. Finally, panels (c), (f) and (i) show the results from a further placebo test, that supposes that the policy was also introduced in other regions of the United Kingdom. To do this, we include interactions between each region and the post-2002 dummy. This omits London, which becomes the only control area. Since all regions outside of Scotland did not implement the policy, the estimated effects associated with these three interactions are expected to be jointly insignificant and this is what we find in all cases.

Assumption 2 (No anticipation). The Scottish individuals were not able to anticipate the introduction of the personal care reform.

Since the progression of the Bill that preceded the Act was closely followed by the UK media and received wide coverage (e.g. BBC, 2001; Inman, 2002), it is likely that households in Scotland were aware of the advent of the policy even prior to its implementation in July 2002. The Scottish observations might then have altered their caregiving behaviour and labour force participation decisions before April 2002. In section (Sensitivity analysis), we show dropping observations from March 2001 to February 2002 does not affect our results.

Assumption 3 (Stable sample composition). Conditional on $(\mathbf{x}_{irt}, \boldsymbol{\gamma}_r, \boldsymbol{\phi}_t)$, the composition of the treated and control groups are assumed to be stable before and after the policy.

Assumption 3 requires that the compositions of the Scotland and England/Wales samples to be stable across time, conditional on observed covariates. This assumption would be violated if it were the case that individuals moved from England/Wales to Scotland in response to the policy introduction because they had greater needs for formal personal care. The analysis in Ohinata and Picchio (2020), which was conducted by using the 1999–2007 British Household Panel Survey, indicates that the policy introduction did not modify the probability of the English/Welsh moving to Scotland.

Assumption 3 also requires that the observations across different waves are comparable to each other. FRS collects data using a stratified clustered probability drawn from the Royal Mail's small user's Postcode Address File. According to Clay *et al*. (2016), the FRS sampling changed in the following two ways during our observation period. From 2001/2002, the area of Scotland north of the Caledonian Canal was included in the FRS. In addition, from 2002, the FRS was extended to include a 100% boost of the Scottish sample. The latter simply sampled twice as many individuals from each postcode and this would improve the precision of our estimates but would not change the sample comparability across years. The former change, however, is potentially problematic. Although the sub-population from the north of the Caledonian Canal is only 0.25% of the UK population, the additional sample may, for example, be older on average compared to the rest of Britain. For this reason, we conducted the following analysis. We took the 2000 and 2001 samples and compared their demographic characteristics to see if we could detect any statistically significant differences. The results are presented in Table A.2 in Online Appendix, section A. As shown in panel (a), aside from three variables, the t-tests return insignificant differences. Even for those that returned significant results, the differences in the averages are very small. Since it is possible that some variables differ even if the two samples are comparable, we conducted the same exercise using the 2003 and 2004 samples and obtained very similar results. In sum, the geographical extension of the Scottish sampling frame in 2001 is unlikely to have affected our results.

Estimation results

The impact of the reform on caregiving behaviour

Panel (a) of Table 5 reports the estimated baseline policy effect for the probability of co-residential caring and for the weekly hours of co-residential caregiving, conditional on caring.[14] The 2002 Scottish reform significantly reduced the probability of giving co-residential care to other adults by 0.4% points. Given that the fraction of individuals giving care in Scotland before the policy was 2.2%, the estimated effect implies a reduction in their probability of giving care by approximately 18% of the pretreatment Scottish average.

The impact of the reform on the number of weekly hours of co-residential caregiving is also negative and significant, as shown in the right columns of panel (a) in Table 5. Because of the interval-coded nature of the outcome variable and the resulting nonlinearity of its model, we cannot quantify the impact of the policy on hours of caregiving from the estimated coefficients. Therefore, in the bottom of row of panel (a), we report the marginal effects of the policy conditional, and unconditional, on the number of hours being larger than zero. That is, the conditional marginal effects

[14]Tables H.5 in Online Appendix, section H, presents the coefficient estimates of all the covariates included in these regressions.

TABLE 5

The policy impact on informal caregiving within the household

	Linear probability model for informal caregiving within the household		*Interval regression for hours of informal caregiving within the household*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*
(a) Policy impact: Baseline				
After*Scotland (I_{rt})	−0.004***	0.001	−13.833***	4.911
Average partial effect of the policy				
ΔE(y|z, $y > 0$)	–		−1.334	
ΔE(y|z)	–		−0.268	
σ	–		142.797	
Log-likelihood	–		−47.585.172	
R^2	0.056		–	
(b) Relation with the care recipient: Spouse				
After*Scotland (I_{rt})	−0.003**	0.001	−15.719**	6.539
Average partial effect of the policy				
ΔE(y|z,$y > 0$)	–		−1.170	
ΔE(y|z)	–		−0.213	
σ	–		152.972	
Log-likelihood	–		−32,424.146	
R^2	0.056		–	
(c) Relation with the care recipient: Parent				
After*Scotland (I_{rt})	−0.001	0.001	−10.369	7.628
Average partial effect of the policy				
ΔE(y|z,$y > 0$)	–		−0.759	
ΔE(y|z)	–		−0.050	
σ	–		128.010	
Log-likelihood	–		−13,784.056	
R^2	0.031		–	
(d) Relation with the care recipient: 65 or older				
After*Scotland (I_{rt})	−0.002*	0.001	−11.434**	5.702
Average partial effect of the policy				
ΔE(y|z,$y > 0$)	–		−1.000	
ΔE(y|z)	–		−0.185	
σ	–		138.862	
Log-likelihood	–		−37,840.127	
R^2	0.067		–	
Observations	399,098		399,098	

Notes: *** Significant at 1%; ** significant at 5%; * significant at 10%. All the regressors included in the baseline models are also included in these models. ΔE(y|z,y>0) shows the marginal effects of the 2002 policy conditional on the control covariates (z) among those giving care (y>0). ΔE(y|z), on the other hand, shows the effects of the policy unconditional on the hours of care.

present the policy effects among those who provide positive hours of care, while the unconditional marginal effects allow us to see the policy effects among the Scottish individuals regardless or whether they provide care or not. The Scottish reform reduced the average number of weekly caregiving hours by approximately 0.27 hours per week. Conditional on giving care, the estimated reduction amounts to 1.33 hours. Since approximately one third of the caregivers in our sample give care for 19 hours a week or less, reduction in the magnitude of 1.33 hours per week in relative term is non-negligible.[15,16]

The behavioural change in informal co-residential caregiving induced by the policy might differ depending on the relationship between the care-giving and the care-receiving individuals. Using the household relationship information available in our dataset, we estimate the baseline equations (1) and (2) but redefine the dependent variables on the basis of whether the care is given to the spouse or to a parent in the same household. The fraction of individuals in our sample who take care of their spouse is 1.5%. The fraction of those who are taking care of their parents (living in the household) is 0.53%. In panel (b) of Table 5, we see that the policy effect on the probability of giving informal care to the spouse is negative and significant (−0.3% points). When we look at the impact on the probability of giving co-residential care to at least one parent, the size of the reduction is smaller and insignificant (−0.1% points as shown in panel (c) of Table 5, with a 95% confidence interval of the same size as that in panel (b)). A similar conclusion can be drawn when we look at the changes in co-residential caregiving at the intensive margin. Just as before, the reduction in the hours of co-residential care is significant when we look at those who were giving care to their spouses. On the other hand, the coefficient for the hours of caregiving to parents is insignificant and smaller.

So far, we have restricted the age of care recipients to those aged 60 or above when defining the outcome variables for caregiving behaviour (see the discussion in footnote 6). In panel (d) of Table 5, we restrict the dependent variable to be equal to one only when co-residential care is given to individuals aged 65 or older. The estimate indicates that the policy reduced the probability of caregiving by 0.2% points and the hours of care by approximately 0.19 hours. These results, compared to the baseline estimates in panel (a), suggest that caregivers changed their caregiving patterns even before the frail individuals became eligible to benefit from the policy. One explanation for this might be that caregivers may have started to rely on formal care earlier than they would have done in the absence of the policy because the lifetime cost of formal care decreased after 2002.

In addition to the effects on co-residential care, the policy may have affected the amount of care given to those living in different households. Our data suggest that the overwhelming majority of extra-residential caregivers are looking after their elderly parents. Table 6 shows the estimated impact on extra-residential care to parents. Focusing on care given to parents also allows us to reduce the chance of including young care recipients. This is important, since we do not observe the age of extra-residential care recipients in our data. We find that the policy had statistically insignificant effects, both at the intensive and extensive margins. In the absence of data

[15] The rise in the demand for formal care after 2002 may have increased the average price of care in Scotland. If this were the case, our estimates would present the lower bound (i.e. closer to zero than would have been in the absence of the policy). See Online Appendix, section E for more discussions.

[16] If the entry rate to nursing care homes changed during the observation period, our results would be affected. However, as shown in Online Appendix, section B, the trend in the proportion of care home residents among 65+ has been stable throughout the period in Scotland.

TABLE 6

The policy impact on informal caregiving to parents living outside the household

	Linear probability model for informal caregiving to parents living outside the household		*Interval regression for hours of informal caregiving to parents living outside the household*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*
After*Scotland (I_{rt})	0.001	0.002	0.098	0.566
Average partial effect of the policy				
$\Delta E(y \vert z, y>0)$	–		0.013	
$\Delta E(y \vert z)$	–		0.005	
σ	–		24.2460	
Log-likelihood	–		−108,060.84	
R^2	0.026		–	
Observations	399,098		399,098	

Notes: All the regressors included in the baseline models are also included in these models. The corresponding estimated coefficients are not reported for the sake of brevity and are available from the authors upon request. $\Delta E(y|z,y>0)$ shows the marginal effects of the 2002 policy conditional on the control covariates (z) among those giving care ($y>0$). $\Delta E(y|z)$, on the other hand, shows the effects of the policy unconditional on the hours of care.

on the type of care being provided we cannot be definitive about why these results should differ from those for co-residential care. One possible explanation for this result is that the types of care delivered by co-residential carers are different from the types delivered by extra-residential carers. The former may be offering the type of care that are closer substitutes to that offered by formal care workers. Beesley (2006) reports evidence that suggests this is indeed the case.

The impact of the reform on working behaviour

If the policy had the effect of reducing the time spent providing informal care of other adults in the same and other households. It is reasonable to ask to what extent this might lead to substitution towards leisure time or towards paid labour supply in the market, but economic theory does not provide unambiguous guidance on what the effects are likely to be and it is essentially an entirely empirical issue.

Table 7 presents the estimation results for employment status and the number of weekly working hours.[17,18]

We find that the free personal care reform increased the probability of employment by 0.7% points, although this result is statistically insignificant. The marginal effect on working hours was an increase by 0.41, which is statistically significant with a P-value of 0.05.

[17]All the coefficient estimates behind Table 7 can be found in Table H.6 in section H of the Online Appendix.

[18]The number of weekly working hours is a continuous variable, with a mass of individuals (16.1%) with exactly zero hours of work. Thus, we also estimated this equation for the weekly working hours using a Tobit model. This linear model and the Tobit version deliver very similar estimated parameters and marginal effects.

TABLE 7

The policy impact on the employment and weekly working hours

	Linear probability model for being employed		*Linear model for weekly working hours*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*
After*Scotland (I_{rt})	0.007	0.005	0.410*	0.209
Observations	254,402		254,402	
R^2	0.109		0.261	

Notes: * significant at 10%. The estimated coefficients of the full set of yearly dummies are not reported for the sake of brevity and are available from the authors upon request.

Heterogeneity of the reform effects across caregivers

Table 8 reports on the heterogeneity in the effects of the reform on co-residential caregiving behaviour and on the labour supply of carers – by gender, carer age and carer education.[19]

Panel (a) shows that men and women have reduced their involvement in co-residential caregiving by the same order of magnitude, both at the intensive and extensive margins. The estimated impact is more precise for male caregivers. Approximately 81% of co-residential care is given to spouses. Out of this, 87% of spousal co-residential care is offered by a single informal caregiver. The policy may have provided a major relief from such care responsibilities for these informal caregivers and the results suggest that male caregivers responded slightly more by reducing their caregiving behaviour.

Turning to the heterogeneous policy effect on working behaviour, we find some evidence of differences in the effects of the policy across gender. More specifically, men were more likely to increase labour force participation as well as working hours. However, it is important to note that the tests of equality of these coefficients suggest that the policy effects are similar in magnitude across gender. Panel (b) distinguishes between people strictly younger than 55 and those older than 55 and reveals that the reform effects in the benchmark models are mainly driven by older people. Panel (c) allows for interactions between the policy variable and both age and gender. We find that it is males aged 55 and above that adjusted their caregiving behaviour. Consistent with this, we also find that males aged 55 and over are those that responded by increasing their labour market participation. This group also increased their working hours although the size of the estimate is comparable to women in the same age category and it is not significantly different from zero. Panel (d) presents estimates that indicate that the policy effects have varied with education. Compared to those who left school at 16 or older, the estimated effects are larger in absolute terms for the less educated. Since most of the people in our

[19]The heterogeneous effects are derived from the estimated coefficients of interactions between the chosen heterogeneity dimension and the Scotland dummy, the after policy dummy, and the interaction effect of residing in Scotland after the reform into our benchmark model.

TABLE 8

Heterogeneity of the reform effect on co-residential caregiving and labour supply

	Linear probability model for co-residential informal caregiving		*Interval regression for hours of co-residential caregiving*		*Linear probability model for being employed*		*Linear model for weekly working hours*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*
(a) By gender								
Women	−0.004*	0.002	−11.820*	6.537	0.006	0.007	0.378	0.284
Men	−0.004**	0.002	−17.225**	7.396	0.011*	0.006	0.714**	0.308
Equality test (*P*-value)		0.802		0.581		0.587		0.419
(b) By age								
[25,55)	−0.001	0.001	−8.350	9.286	0.002	0.005	0.204	0.226
55 or older	−0.009***	0.003	−16.650***	5.782	0.028**	0.013	1.177**	0.544
Equality test (*P*-value)		0.017**		0.446		0.060*		0.098*
(c) By gender and age								
Age [25,55) and women	−0.001	0.002	−9.737	11.988	0.003	0.008	0.186	0.307
Age [25,55) and men	0.000	0.002	−5.642	14.706	0.005	0.006	0.524	0.329
Age 55 or older and women	−0.007	0.004	−12.621	7.745	0.026	0.021	1.477**	0.734
Age 55 or older and men	−0.011**	0.005	−21.132**	8.588	0.036**	0.017	1.233	0.801
Equality test (*P*-value)		0.081*		0.758		0.245		0.300
(d) By education								
Left education before age 16	−0.008***	0.003	−16.040**	6.259	0.011	0.011	0.873**	0.442
Left education at or later than age 16	−0.002	0.001	−14.796*	7.775	0.004	0.005	0.195	0.195
Equality test (*P*-value)		0.050*		0.900		0.570		0.175
(e) By education and age								
Age [25,55) and left education before 16	−0.002	0.003	−11.402	18.589	−0.004	0.014	0.424	0.583
Age [25,55) and left education at or after 16	−0.001	0.001	−10.179	10.708	0.002	0.005	0.097	0.244
Age 55 or older and left education before 16	−0.010***	0.004	−17.288**	6.675	0.026	0.017	1.156*	0.684
Age 55 or older and left education at or after 16	−0.008	0.005	−19.637*	11.390	0.034	0.021	1.529*	0.900
Equality test (*P*-value)		0.081*		0.919		0.246		0.243

Notes: *** Significant at 1%; ** significant at 5%; * significant at 10%. All the regressors included in the baseline models are also included in these models. For the informal co-residential caregiving sample, the age range is restricted to be between 25 and above. The employment related estimates are based on the sample of individuals aged between 25 and 75. The corresponding estimated coefficients are not reported for the sake of brevity and are available from the authors upon request.

data who left education before 15 are older than 55,[20] it is not clear whether the heterogeneity is related to low education or to the older age. Thus, in panel (e), we interact the policy dummy with each of the age group dummies and each of education level dummies. We find that age is a more important factor as we uncover stronger policy effects among those who are older than 55, but the magnitude of the effects do not differ substantially regardless of education.

Sensitivity analysis

We conduct various sensitivity checks to test the robustness of our baseline findings. In our first exercise, we exclude 2001 from our sample in order to test for the possible anticipation effect (panel (a) of Table 9). As discussed in section (Identification assumptions), from the time the Sutherland Commission was set up, the entire process until the enactment of the Scottish CCHA was widely publicized in the media. As a result of this media coverage, individuals may have anticipated the introduction of the policy. The estimates indicate that excluding 2001 from our sample raises, in absolute value, the estimated effects of the policy on caregiving and labour supply. This is potential evidence of anticipation effects, since our robustness check suggests that individuals may have already reduced their caregiving, and increased their work probability and hours from 2001.

Second, we remove households living in London from our sample. We do this because we suspect that London is likely to differ substantially from the rest of England in terms of its economic activities and demographic characteristics such as migration movements (Duranton and Monastiriotis, 2002; Hatton and Tani, 2005). In panel (b) of Table 9, we observe that the policy effects are only marginally different from those of the benchmark estimates in Tables 5 and 7. We further drop all of the Southern regions to see whether our results still remain robust.[21] The underlying idea behind this sensitivity analysis is to compare regions that are likely to be closer to Scotland in terms of its economy, social organization or cultural background. Panel (c) of Table 9 suggest that, although both the caregiving and the work related effects are less significant compared to the baseline estimates, the magnitude of the estimates are similar. In addition, just as we saw in Table 8, the estimated effects are stronger among the older individuals.

Finally, we re-estimate the two outcome equations, restricting the sample to potential carers over 49, and then further to over 59. If younger individuals are not as involved in caregiving, then we might attenuate the estimated impact by including them. Table 10 shows the effect on the probability and hours of co-residential caregiving if the sample is restricted to individuals older than 49 (panel a), or 59 (panel b). As expected, the effect is stronger the more we focus on older individuals. However, if we compare the effect on the probability of co-residential caring relative to the fraction of individuals giving care in Scotland before the reform, the proportional effect is very stable: −18% (−19%) for the over 59's (49), which is

[20]In our sample, 74.9% of those who left education before turning 16 are older than 55.
[21]That is, we drop London, South East, South West and East.

TABLE 9

Robustness checks of the reform effect on caregiving and labour force participation

	Linear probability model for co-residential caregiving		*Interval regression for hours of co-residential caregiving*		*Linear probability model for extra-residential caregiving*		*Interval regression for hours of extra-residential caregiving*		*Linear probability model for being employed*		*Linear model for weekly working hours*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*	*Coeff.*	*Std. Err.*
(a) Removing 2001												
(i) Homogeneous policy effect:	−0.005***	0.002	−15.860***	5.533	−0.000	0.003	−0.123	0.658	0.010*	0.006	0.402	0.246
(ii) Heterogeneous policy effects by age:												
[25,55)	−0.001	0.001	−12.836	9.963	0.004	0.003	0.646	0.779	0.002	0.003	0.125	0.263
55 and older	−0.009**	0.004	−17.066***	6.559	−0.006	0.004	−1.662	1.190	0.045***	0.004	1.626**	0.640
Observations	358,545		358,545		358,545		358,545		228,257		228,257	
(b) Removing London												
(i) Homogeneous policy effect:	−0.004**	0.002	−12.608**	5.339	0.002	0.002	0.399	0.597	0.009*	0.005	0.364	0.222
(ii) Heterogeneous policy effects by age:												
[25,55)	−0.000	0.001	−6.818	9.489	0.005	0.003	0.803	0.699	0.004	0.005	0.166	0.237
55 and older	−0.008**	0.003	−14.683**	6.182	−0.001	0.003	−0.199	1.058	0.031**	0.013	1.160**	0.551
Observations	359,271		359,271		359,271		359,271		226,843		226,843	
(c) Removing London and the South regions												
(i) Homogeneous policy effect:	−0.004**	0.002	−12.698**	6.324	0.004	0.003	0.781	0.752	0.003	0.006	0.238	0.269
(ii) Heterogeneous policy effects by age:												
[25,55)	−0.001	0.002	−7.181	10.214	0.006	0.004	1.045	0.849	−0.002	0.006	0.014	0.282
55 and older	−0.007**	0.004	−14.590**	7.114	0.001	0.003	0.188	1.220	0.021	0.014	0.993*	0.594
Observations	235,636		235,636		235,636		235,636		145,429		145,429	

Notes: *** Significant at 1%; ** significant at 5%; * significant at 10%. All the regressors included in the baseline models are also included in these models. For the informal co-residential caregiving sample, the age range is restricted to be between 25 and above. The employment related estimates are based on the sample of individuals aged between 25 and 75. The corresponding estimated coefficients are not reported for the sake of brevity and are available from the authors upon request.

almost identical to the relative impact obtained using the benchmark sample (−18%).[22]

VI. Conclusions

This paper studies the impact of the Scottish Care and Health Act 2002, which introduced unconditional in-kind subsidies for formal personal care costs, on the informal caregiving and working behaviours of Scottish people. We use a DD strategy since the rest of Great Britain retained the previous system of means-tested subsidies. We find that the Scottish policy reduced the probability of co-residential informal caregiving by a statistically significant 0.4% points, which amounts to a decrease of about 18% relative to the pretreatment Scottish proportion of caregivers. We also find that the number of hours per week of co-residential informal caregiving fell, by a statistically significant 0.27 hours per week. Conditional on giving co-residential care, the estimated effect suggests a reduction of about 1.33 hours per week. The effect is particularly strong among older and less educated individuals.

In contrast to our findings for co-residential care, we find no statistically significant effects for extra-residential care. One possible explanation for the lack of an effect in the extra-residential case is that, compared to those giving co-residential care, such carers may be less likely to offer care that is a close substitute to the formal care offered but are more likely to offer complementary support such as companionship. Although FRS does not include detailed information on the type of informal care offered, Beesley (2006) reports evidence, based on Maher and Green (2002), that this is the case that co-residential and extra-residential care differ in the ways suggested. Assuming that the frail individuals receiving extra-residential care are still capable of living alone, it is possible that they are less likely to need extensive amounts of personal care and the increase in formal care prompts little reduction in extra-residential care.

Turning to the labour supply outcomes, we observe that Scottish individuals statistically significantly increased their employment probability and working hours. This effect is particularly strong and significant among individuals older than 55: at the extensive margin (+2.8% points) and at the intensive margin (+1.18 hours per week). One possible explanation for finding the effects on labour supply is that those who provided informal care prior to the 2002 reform took up paid formal care work while making use of the in-kind formal personal care support offered for free. We would require a large and detailed panel dataset to evidence this suggestion and, instead, we provide suggestive evidence, in Online Appendix G, using the UK 1998–2007 Labour Force Survey that documents information on individuals' occupations. Figure F.3 shows that the shares of Scottish men and women engaged in formal care work increased after the policy was introduced. When restricting the data to those aged 55 and above, we also observe an increase in the share of this subpopulation working in the care sector. Of course, we are unable to say if this was due to a change in the

[22]The prereform share of Scottish individuals over 59 (49) giving co-residential care was 4.8% (3.6%).

TABLE 10

The policy impact on co-residential caregiving restricting the sample to older people

	Linear probability model for informal caregiving within the household		*Interval regression for hours of informal caregiving within the household*	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*
(a) Individuals aged 50 or older than 50 (201,447 observations)				
After*Scotland (I_{rt})	−0.007***	0.003	−16.503***	5.773
Average partial effect of the policy				
ΔE(y|z,y>0)	–		−1.881	
ΔE(y|z)	–		−0.539	
Relative effect with respect to fraction of individuals giving co-residential care in Scotland before the policy				
	−19.2%		–	
(b) Individuals aged 60 or older than 60 (128,091 observations)				
After*Scotland (I_{rt})	−0.009**	0.004	−16.247**	6.778
Average partial effect of the policy				
ΔE(y|z,y>0)	–		−1.980	
ΔE(y|z)	–		−0.676	
Relative effect with respect to fraction of individuals giving co-residential care in Scotland before the policy				
	−18.2%		–	

Notes: *** Significant at 1%; ** significant at 5%; * significant at 10%. All the regressors included in the baseline models are also included in these models. ΔE(y|z,y>0) shows the marginal effects of the 2002 policy conditional on the control covariates (z) among those giving care (y > 0). ΔE(y|z), on the other hand, shows the effects of the policy unconditional on the hours of care.

status of the carer, from informal to formal, for the same care recipient – that is, we cannot say if the informal carer simply became the formal carer.

For both caregiving and labour supply outcomes, we observe stronger responses among older and less educated individuals and the effect is particularly strong among men. The fact that the older subpopulation is reacting in this way is reassuring as they are likely to be providing intensive co-residential care to their spouses. The strong effect among less educated individuals presents an interesting insight into a potential redistribution of wealth from those with the higher to the lower socioeconomic backgrounds discussed by Besley and Coate (1991). Their theoretical model predicts that the public provision of health care funded by general taxation might redistribute wealth as the rich opt out, in favour of the higher quality care that is offered privately, despite having contributed through taxation. The suggestion is that it is the least wealthy that benefit most from publicly provided care.

That men responded more to the policy suggests that they were more willing to delegate their care tasks to the formal care sector once it became freely available, compared to female carers. This may be due to the fact that men are facing a higher opportunity cost of providing care. This may also reflect the cultural expectations of the female spouses of frail men, that the male spouses of frail women feel does not apply to them. Our estimated effects on informal co-residential caregiving and labour supply indicates that households, on average, substituted 1 hour of informal care for 1 hour of work. Therefore, while the introduction of the 2002 policy may well have been costly, the policy at the same time seemed to have promoted Scottish individuals to participate more in the labour market. However, it seems unlikely that the additional tax revenue from this additional labour supply would be sufficient to make the reform revenue neutral. Nonetheless, this strong increase in paid work will have offset at least some of the costs of providing free care.

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Conflict of interest

Although Asako Ohinata, Bruce Hollingsworth and Ian Walker received a grant from the UK Medical Research Council, this research and submission decision have been conducted independently of the funding body.

Ethics

The paper uses a secondary anonymized data and the ethical approval was obtained for this project from the University of Leicester and Lancaster University.

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Supporting Information

Additional Supporting Information may be found in the online version of this article:

Appendix A. Additional information on data.
Appendix B. Additional CCHA policy details affecting those in care homes.
Appendix C. Summary comparisons of the three Scottish papers.
Appendix D. Information on the dependent variables.
Appendix E. Price of care.
Appendix F. Information on the statistical inferences.
Appendix G. Trends in the share of care workers.
Appendix H. Other tables.