The London School of Economics and Political Science

# Essays in Labor and Public Economics

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# Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).



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# Statement of Conjoint Work

I confirm that Chapter 2, "Subsidizing Labor Hoarding in Recessions: The Employment and Welfare Effects of Short Time Work", was jointly co-authored with Professor Camille Landais. A version of this paper has been published as CEPR Discussion Paper No. 13310, CEP Discussion Paper No. 1585, and WorkINPS Paper No. 16. This statement is to confirm that I contributed 50 percent of this work.

I confirm that Chapter 3, "Changing the Structure of Minimum Wages: Firm Adjustment and Wage Spillovers", was jointly co-authored with Professor Stephen J. Machin. A version of this paper has been published as CEP Discussion Paper No. 1533, IZA Discussion Paper No. 11474 and CEPR Discussion Paper No. 12919. This statement is to confirm that I contributed 50 percent of this work.

I confirm that Chapter 4, "Zero Hours Contracts and Labor Market Policy", was jointly co-authored with Nikhil Datta and Professor Stephen J. Machin. The paper is forthcoming on *Economic Policy*. This statement is to confirm that I contributed 33 percent of this work.

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# Abstract

This thesis investigates the employment and welfare effects of social insurance programs and minimum wage policy.

The first chapter provides new estimates of the income effect of welfare transfers on individual labor supply. Using administrative data on survivor insurance in Italy, and quasi-experimental variation in the benefit amount received by surviving spouses, the analysis shows that benefit losses trigger equivalent increases in earned income, implying an income effect of approximately minus one. Extensive-margin responses – in the form of increased labor-market entry at younger ages and delayed retirement at older ages – emerge as the main driver of the earned income response. A revealed-preference model demonstrates that large participation responses to realized benefit drops are revealing of large implicit valuations of welfare transfers in the widowhood state.

The second chapter analyzes the employment and welfare effects of short-time work programs (STW), which subsidize hour reductions in firms affected by temporary shocks. The analysis uses administrative data from Italy and quasi-experimental variation in STW policy rules to identify the effects of STW on firms and workers, and on reallocation in the labor market. STW has a large and significant negative effect on hours, but large and positive effects on headcount employment. However, these effects disappear once the subsidy ends. Similarly, STW does not provide long-term insurance to workers. Finally, STW has significant negative reallocation effects on employment growth at the local labor market level. A conceptual framework assesses the welfare implications of STW and provides a general formula for the optimal subsidy.

The third chapter investigates the impact of minimum wages on firm behavior and the within-firm wage structure. The analysis exploits the natural experiment of the National Living Wage (NLW) introduction and matched employer-employee data on English care homes. No evidence of adverse employment effects, nor firm closure is found. Rather homes bound more tightly by the NLW exhibit smaller short-run improvements in the quality of care services. There is strong evidence of positive wage spillovers onto younger workers, but with there being no negative employment spillovers. Employers' preferences for fairness emerge as the most plausible explanation for the observed wage spillovers.

The fourth chapter investigates the nature of alternative work arrangements in the UK labor market, placing a particular focus on zero hours contracts (ZHC). Combining existing secondary data and newly collected survey data, the analysis documents the importance and characteristics of ZHC work. The chapter also explores the extent to which higher minimum wages have potential to induce a larger utilization of alternative work arrangements by firms and, consequently, a shift in the composition of their workforce towards more flexible, but also insecure jobs. Minimum wage increases are shown to have resulted in a greater utilization of ZHCs in the UK social care sector, and in low wage sectors more generally.

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I thank my parents and my brother for all the love, countless opportunities and unconditional support they have given me throughout all these years. I thank Luca for always standing by my side with patience, tenacity and love. I dedicate this dissertation to the memory of my father, whose endless love fills each and every one of my days.

To my father

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# Chapter 1

# When Income Effects are Large: Labor Supply Responses and the Value of Welfare Transfers

## 1.1 Introduction

The effect of income on labor supply is a parameter of great importance for both theory and policy analysis. Theoretically, the income effect provides the link between the uncompensated and the compensated elasticity through the Slutsky equation. Estimates of the income effect, combined with estimates of the uncompensated wage elasticity, are thus useful to back out the compensated elasticity, which is itself a key parameter for optimal tax policy design and for the calibration of business cycle models. From a policy perspective, income effects are central to the evaluation of a broad set of policies involving income transfers, such as social insurance programs, public pension schemes and tax policies. Income effects are also important for welfare analysis, since they are directly related to the marginal utility of consumption (Chetty, 2004; Chetty, 2008). Recent work has shown that income effects are also key inputs of standard balanced-growth theory, because they help rationalize the secular decrease in hours worked observed in many developed and developing countries (Boppart and Krusell, 2016).

In spite of their importance for economic analysis, we still know surprisingly little about income effects, especially in the context of welfare transfers. This is mostly due to the fact that the effect of income on labor supply is hard to identify. Identification is often complicated by the fact that social insurance, and tax and benefit programs generally involve simultaneous transfers of income and changes in work incentives, which make it hard to separately identify income and substitution elasticities. The ideal experimental setting for identification would require to randomly allocate substantial lump sums to individuals for a long period of time. In practice, such ideal experiment is hard to come by. For this reason, income effects have been typically either assumed away, or calibrated to recover compensated elasticities. Most quasi-experimental estimates of income effects are based on transfers that are either too modest to trigger a response, or relatively short-lived implying that observed responses may be substantially attenuated by optimization frictions (Pencavel, 1986; Blundell and MaCurdy, 1999; Kimball and Shapiro, 2008; Marinescu, 2018). It is therefore still unresolved whether existing estimates of income effects are indeed capturing the true effects of income on labor supply, especially in relation to welfare transfers.

In this paper, I provide novel estimates of the income effect of welfare transfers on individual labor supply, earnings and total income. To do so, I exploit a unique research setting in the context of the Italian survivor insurance scheme. Survivor insurance is a public scheme providing a pension benefit to surviving spouses of deceased retirees and workers. The benefit is computed as a fraction of the deceased's pension and starts from the beginning of the month following the death.[1] I take advantage of a policy change that introduced an exogenous, large and permanent discontinuity in the fraction of the deceased's pension received by survivors on the basis of their spouse's death date. Specifically, the reform decreased the fraction of the deceased's pension received by survivors whose benefit started on or after September 1, 1995, generating a discontinuity in the expected lifetime benefit of €100,000 (or 31 percent of the mean in the pre-reform regime) and *de facto* introducing two parallel benefit regimes of exogenously different generosity that would then coexist for a long time.[2]

Using newly released, rich administrative data on the universe of survivor insurance payments and survivors' contributory histories from the Italian Social Security (INPS), I implement a regression discontinuity design in the spousal death date – which is equivalent to the benefit start date – and compare the long-term outcomes of otherwise identical individuals receiving benefits of different generosity for a long time, in order to identify the income effect of transfers on individual labor supply and other economic behavior. The long-run identifying variation generated by the benefit reform offers a unique window on the long-run behavioral responses to a permanent reduction in benefits. Specifically, the research setting allows to estimate long-run effects that are plausibly not attenuated by short-run optimization frictions. Also, by comparing treated and control individuals similarly affected by the loss of a spouse, the identification strategy implicitly controls for the confounding role of state-dependent preferences. Finally, while most existing estimates of income effects are based on benefit expansions, this setting allows me to explore the effects of a benefit loss.[3] In this respect, it is often implied that the effect of a positive and a negative income shock would be symmetric, but this need not be the case if, for instance, agents are loss averse or have sticky consumption habits.

I find that survivors fully offset the benefit loss by increasing their earnings and, as a

[1] Entitlement to the benefit is lost upon remarriage. It otherwise continues until death.

[2] In the empirical analysis, I restrict the sample to individuals aged 55 and under at the time of their spouse's death. The expected lifetime benefit drop is computed on this sample.

[3] It is likely that individuals in the new regime expected higher survivor benefits, especially given that the reform was little anticipated.

result, do not experience any drop in disposable income. Specifically, in the fifteen years after their spouse's death, survivors affected by the reform lose on average €2,000 per year, which is equivalent to a 21 percent drop relative to old-regime surviving spouses. In response, they increase their average annual earned income by a quantity equal to the benefit loss. This translates into an estimated marginal propensity to earn out of unearned income (MPE) of approximately -1.0, indicating that earned income increases one-for-one with decreases in unearned income.[4] I document substantial heterogeneity in the income effect by the relative severity of the benefit loss.

I probe the large income response by examining its underlying mechanisms. Firstly, I decompose the earned income response along three margins: labor force participation, hours worked and the wage rate. Labor force participation is the main driver of the income response: a loss of €1,000 in benefits increases labor supply by 4 percentage points on average in the fifteen years after the spouse's death – an effect equivalent to 7 percent of the mean in the control group. The participation response is driven by both increased labor-market entry by younger survivors and delayed retirement by older survivors. Hours worked and the wage rate are found to have a muted response to changes in the benefit. Secondly, I uncover interesting dynamic patterns in the labor supply response: participation responses are silent in the two years after the spouse's death and then grow steadily larger over time, reaching a differential of 18 percent after 15 years. The observed pattern in the years immediately after the death is likely due to grief. The overall dynamic is also consistent with the notion that optimization frictions, such as adjustment costs, attenuate responses in the short run and fade away over time. The dynamics of hours worked and the wage rate are flat throughout the 15 years following the spouse's death, indicating no intensive-margin adjustments and suggesting that work experience, human capital accumulation and effort have limited returns in the context under study.

I investigate program substitution responses as an additional margin of adjustment in response to the income drop.[5] I find that survivor benefit reductions trigger a statistically significant increase in the take-up of paid family leave and unemployment insurance benefits. The magnitude of both effects is large as a percentage of the mean in the control group. I interpret the increase in paid-family-leave take-up as an indication that surviving spouses who increase their labor supply may be doing so under substantial work-time constraints due to care duties. The increase in unemployment insurance take up may instead suggest that individuals are willing to pay the cost of unemployment stigma to increase disposable income.

Finally, I discuss the normative implications of my findings. A central result of this

[4]The income effect – or marginal propensity to earn out of unearned income – is measured as the change in earned income for a unit change in unearned income.

[5]Program substitution refers to a change in take-up of other social assistance and social insurance programs (conditional on eligibility) in response to a change in a given program's generosity (Inderbitzin, Staubli, and Zweimüller, 2016).

paper is the large labor supply and taxable income response to a negative income shock. Why is the income effect that I estimate so large? On the one hand, if an individual increases labor supply sharply in response to a benefit drop, that lost income must be of high utility value. On the other hand, a substantial labor supply response may arise if the cost of adjusting labor supply is small. Understanding which of these two alternative mechanisms prevails is important for welfare analysis. I first provide evidence of there being adjustment costs associated to the observed labor supply response. I show that the participation response to a given benefit drop is a negative function of the contemporaneous unemployment rate in the region in which the survivor resides. Since the unemployment rate is arguably positively correlated with the utility cost of labor – either because finding a job requires more search effort or because keeping an existing job requires more on-the-job effort –, this evidence is consistent with the notion that adjustment costs are non-zero in the context analyzed.

I then propose a new methodology to estimate the value of transfers, based on participation responses to benefit losses. I demonstrate that survivors' labor supply responses to a realized drop in benefits reveal their implicit valuation of the benefit in the widowhood state, as measured by the gap in the marginal utility of consumption between the low-benefit and high-benefit regime. Intuitively, the extent to which individuals increase work effort in response to a drop in unearned income reveals, *ceteris paribus*, the consumption value that such lost income would have provided. Hence, larger responses must mean that the lost income is highly valued and that there are large welfare gains from recouping it. I provide conditions under which the semi-elasticity of labor force participation to an unconditional transfer scaled by the semi-elasticity of labor supply to the wage rate can be used to evaluate the marginal welfare gains from increased survivor insurance generosity. I estimate a marginal welfare gain of 0.5, which implies that the marginal utility of consumption is 50 percent higher among widow(er)s in the low-benefit regime as compared to widow(er)s in the high-benefit regime. This is in the higher end of the range of existing estimates of the value of social insurance, unemployment insurance in particular. It follows that widowhood is a state with a high marginal utility of consumption and that increasing the generosity of survivor insurance would generate substantial welfare gains.

Whilst there is a selection issue of studying survivor insurance benefits, nonetheless I believe there is scope for generalizability of the findings obtained in this context. Individuals in my sample are spouses – prevalently women – who become widow(er)s in their mid forties. Single-parenthood, and single-motherhood in particular, are among the states most at risk of income insecurity and single parents make up a large proportion of welfare recipients of programs such as Earned Income Tax Credit (EITC), Aid to Families with Dependent Children (AFDC) and Temporary Assistance to Needy Families (TANF). Hence, to the extent that widow(er)s in my sample can be representative of single parents in general, my findings can be relevant to a larger set of public policies in the US and in

Europe. Losing a spouse at a young age is a low-probability and relatively unpredictable event against which households are likely to be limitedly insured. In this respect, my estimates of the labor supply and income response are likely to provide an upper bound of what would be expected for "shocks" that are easier to anticipate and insure *ex ante*, such as own or spousal job loss and skills obsolescence. Finally, the elasticity of labor supply may differ between marriage and widowhood, due to leisure complementarities between spouses (Goux, Maurin, and Petrongolo, 2014), a potentially increased desire to engage in working activities due to loneliness during widowhood, or the (in)ability to share family-related duties with a partner. To the extent that labor supply elasticities are higher (lower) in the widowhood state, the effects estimated in this paper are likely to provide an upper (lower) bound for what would be expected for married individuals.

**Related literature and main contribution.** The findings in this paper inform a long-standing line of research on the income effect of welfare transfers on labor supply. Early studies of the US negative income tax (NIT) and the Canada Mincome experiments that took place simultaneously in the 1970s tend to find negative, but small and statistically insignificant effects of income guarantees on employment and hours worked (Robins, 1985; Burtless, 1986; Ashenfelter and Plant, 1990; Hum and Simpson, 1993). Whilst the combination of wealth transfers and marginal tax rate changes that characterizes NIT-like experiments makes it complex to disentangle income and substitution effects, consensus estimates set the income effect between -0.10 and 0.00. The ability to draw conclusions from NIT experiments is however limited by data collection issues such as selective attrition and earnings misreporting, and by the short duration of the programs, which raises concerns that observed responses may be attenuated by frictions.[6]

To compellingly isolate income effects, subsequent studies have examined the few existing examples of unconditional cash transfers and lottery wins, as both settings do not entail alterations of the tax structure faced by individual recipients. Studying the Eastern Band of Cherokee Indians Casino Dividend, Akee et al. (2010) find results consistent with a zero income effect four years after the start of the payments. Similar results are found in the context of the Alaskan Permanent Fund Dividend (Jones and Marinescu, 2018). However, in both studies, dividend payments may be correlated with increased job opportunities, implying that micro-level income effects may be compensated by opposite-signed macro labor demand effects. Studies of lottery winners in Massachusetts (Imbens, Rubin, and Sacerdote, 2001) and Sweden (Cesarini et al., 2017) both estimate marginal propensities to earn out of lottery wins of -0.10. Being closest to the ideal experiment for the identification of a causal effect, lottery studies provide internally valid and credibly identified estimates of wealth effects on labor supply. There are, however, concerns about the generalizability of their findings to other contexts, due to the selected nature of both

[6]Price and Song (2018) study the long-term effects of NIT experiments taking place in Denver and Seattle in the 1970s on individual outcomes up to four decades after the programs ended. Treated individuals have lower post-experimental annual earnings and a higher propensity to apply for disability insurance compared to individuals in the control group, suggesting that income support may have important dynamic effects.

the population of lottery players and the type of wealth shock that lottery wins constitute.[7,8] I contribute to this literature by providing well-identified estimates of the income effect from a large and permanent drop in unearned income, in the long term and in the context of publicly provided benefits.

This paper is also related more broadly to the literature on the labor supply and program substitution effects of social insurance programs, such as disability insurance (Bound, 1989; French and Song, 2014; Kostol and Mogstad, 2014; Autor et al., 2016; Deshpande, 2016a; Deshpande, 2016b; Autor et al., 2017), health insurance (Garthwaite, Gross, and Notowidigdo, 2014), earned income tax credits (Eissa and Liebman, 1996; Saez, 2010) and retirement wealth (Krueger and Pischke, 1992; Gelber, Isen, and Song, 2016). Two of these studies provide estimates of the income effect that are much larger than consensus estimates in the literature and more in line with the findings in this paper. Specifically, Deshpande (2016b) estimates a parental earnings response to the loss of Supplemental Security Income payments of approximately -1.4, while Gelber, Isen, and Song (2016) and Gelber, Isen, and Song (2017) estimate an upper bound of the elderly earnings response to the Social Security "Notch" of -0.6 for men and -0.89 for women.[9]

This paper is also partly related to a large literature on the divergence between steady-state macro and micro elasticities of labor supply.[10] Macroeconomic models of cross-country variations in hours worked imply elasticities that are much larger than those estimated using micro-level sources of identifying variation. Optimization frictions (Chetty, 2012) and the indivisibility of labor (Rogerson, 1988; Ljungqvist and Sargent, 2007; Rogerson and Wallenius, 2009) have been identified as the two main factors that can reconcile such divergence. As previously argued, the long-run variation offered by the 1995 reform is useful in identifying a parameter estimate likely not attenuated by short-run optimization frictions. I show that the – arguably frictionless – micro elasticity that I estimate is indeed statistically compatible with macro elasticities of labor supply and with the steady-state frictionless hour elasticity identified in Chetty (2012). I also discuss the relationship between my findings and macroeconomic models of indivisible labor.

Finally, this paper contributes to a growing body of work that attempts to evaluate the welfare gains of social insurance using empirically estimable "sufficient statistics". Applied predominantly in the context of unemployment insurance, consumption-based

[7] The cited studies find that lottery players tend to differ in their observables from the general population: specifically, they are more likely to be male, older, less educated and with lower earnings. Lottery players may as well differ in their unobservable characteristics, such as the degree of risk aversion. Moreover, to the extent that lottery wins and welfare transfers are not fungible (Thaler, 1990), lottery-based estimates need not be representative of responses to welfare transfers or other public policy schemes. Finally, the magnitude of lottery wins is in most cases small.

[8] Unconditional cash transfers have been widely studied in developing countries. Recent surveys of the existing literature are unanimous in concluding that cash transfers had no detrimental effects on employment (Bastagli et al., 2016; Banerjee et al., 2017).

[9] Interestingly, both those and this study exploit exogenous *reductions* in benefits relative to the *status quo ante* (contrary to the majority of studies in the literature), hinting at potentially asymmetric responses to benefit gains and losses (Deshpande, 2016b).

[10] See Chetty et al. (2011) for a review of this literature.

approaches use consumption responses to unemployment or job loss (combined with measures of the coefficient of relative risk aversion) to infer the value of unemployment insurance (Baily, 1978; Gruber, 1997; Chetty, 2006a). The main limitation of consumption-implementation approaches is their reliance on consumption data, for which availability is limited and often partial, and where there are issues of mis-measurement and difficulties with assignment to individuals within a household. From a theoretical standpoint, the approach also relies on the potentially strong assumptions of state-independent preferences and no anticipation effects. Partly in response to these limitations, recent work has developed revealed-preference, optimization-based approaches that exploit behavioral responses to estimate the welfare gains from social insurance. While most work has been done in the context of unemployment insurance (Shimer and Werning, 2008; Chetty, 2008; Landais, 2015; Hendren, 2017), similar approaches have been developed for social insurance against fatal and non-fatal health shocks (Fadlon and Nielsen, 2018; Dobkin et al., 2018).[11] I contribute to this literature by providing a simple revealed-preference method based on *within*-state participation responses to benefit losses that allows for state dependence and is applicable to a broad class of public policies involving income transfers. Moreover, because it requires labor supply rather than consumption data, the revealed-preference approach that I propose has the advantage of being widely applicable given the increasingly large availability of detailed data on individual labor supply from administrative and other sources.

The paper proceeds as follows. Section 1.2 outlines the institutional details of the Italian survivor insurance scheme and of its 1995 reform, and discusses the expected effects of the reform on individual labor supply. Section 1.3 describes the INPS administrative data and illustrates the empirical strategy. Estimates of the effect of survivor insurance benefits on total income, labor supply and program substitution are presented in Section 1.4. Section 1.5 examines the external validity of the findings and their relation to theories of labor supply. Section 1.6 discusses the normative implications of the findings and provides a theoretical framework to evaluate the welfare gains from increased survivor insurance generosity. Section 1.7 concludes.

## 1.2 Institutional Setting and Conceptual Framework

### 1.2.1 Background on the Italian Survivor Insurance Scheme and its 1995 Reform

The largest across OECD countries, the Italian survivor insurance scheme amounts to 2.4 percent of GDP and involves up to 4.4 million recipients in 2017 (INPS, 2018). The

[11] In recent work, Landais and Spinnewijn (2018) propose a revealed-preference approach based on marginal propensities to consume that allows for state-dependent preferences and accounts for unobserved margins of adjustment.

scheme provides benefits to the relatives of deceased retirees and disability-insurance recipients entitled to a state pension (in which case the survivor pension is called *pensione di reversibilità*), and of deceased workers who have a minimum number of accrued weeks of compulsory contributions towards their state pension (in which case the survivor pension is called *pensione indiretta*).[12]

The benefit is universally provided to the following surviving relatives: the surviving spouse, even if separated or divorced provided that alimony rights have been granted and that the spouse has not remarried; dependent children, who are minors, incapacitated or students (including university students); fully-dependent minor grandchildren; absent the above, dependent parents aged 65 and over, and siblings, who are not simultaneously entitled to other social security benefits. The benefit starts at the beginning of the calendar month following the death date, irrespective of when the application is filed. For surviving spouses, entitlement to the benefit ends once they remarry; for dependent children and grandchildren, once they turn 18 or lose their incapacitation status; for parents and siblings, once they lose their dependency or incapacitation status, or once they become entitled to other social security benefits.[13] Dependent children and grandchildren aged 18-21 who are high-school students and not working are entitled to the benefit up to age 21. University students up to age 26 are also entitled to the benefit, provided that they are not working.

The amount of the benefit is computed as a percentage of the pension that the deceased was or would have been entitled to at the time of death.[14] Table 1.1 summarizes the replacement rates – i.e. the percentage of the deceased's pension received by surviving relatives – for different types of survivors. As reported in the first column, a spouse without dependent children or grandchildren receives 60 percent of the pension of the deceased, a spouse with one dependent child 80 percent and a spouse with two or more dependent children 100 percent. Absent the spouse, the replacement rate for a sole dependent child is 60 percent, for two dependent children 80 percent and for three or more dependent children 100 percent. Absent the spouse, children or grandchildren, dependent parents and siblings are entitled to 15 percent of the deceased's pension each, up to a total of 100 percent.

[12]In order to qualify for survivor insurance (*pensione indiretta*), deceased workers who have not yet retired at the time of their death, must have accrued a minimum of 780 weeks of contributions or a minimum of 260 weeks of contributions, of which 156 in the five years prior to death. In case these requirements are not met, survivors are entitled to a one-off lump-sum payment. Survivors of deceased workers with at least one year of contribution in the five years prior to their death and whose contributory history started on or before December 31, 1995 are entitled to a benefit equal to 45 times the amount of their contributions, up to a cap of €2,979.90. Conditional on having an income below the social assistance threshold, survivors of deceased workers whose contributory history started after December 31, 1995 are entitled to a one-off payment equivalent to the number of years of contributions of the deceased times €448.00.

[13]Once the surviving spouse remarries, he/she receives a one-time lump-sum payment equivalent to two years of benefits.

[14]In those cases in which the deceased had not yet retired at the time of death, the survivor benefit is based on the pension that he or she would have been entitled to at the time of death, as determined by pension contributions paid up to that date. In case the deceased was on disability insurance at the time of death, the survivor benefit is computed on the basis of the disability benefit.

As part of the 1995 reform of the Italian social security system (Law 335/95), the survivor insurance scheme moved from universal to means tested. The reform, which was passed on August 8, 1995, affected all benefit payments starting on or after September 1, 1995 whose beneficiary is a spouse with no dependent children. As illustrated in the second column of Table 1.1, the new-regime replacement rate for surviving spouses with no dependent children nor grandchildren decreases sharply when the survivor's annual taxable income exceeds certain thresholds. Specifically, the replacement rate drops to 45 percent if the survivor's income is above three times the annual minimum pension, 36 percent if above four times the annual minimum pension and 30 percent if above five times the annual minimum pension.[15] The income measure used to determine the replacement rate is individual taxable income, inclusive of all forms of labor income from employment and self-employment, retirement income, pensions and retirement annuities, capital income and rental income, with the exclusion of the survivor pension. During the application stage and in each subsequent year, survivors are required to report their taxable income to the Social Security Administration.[16] If they fail to do so, they receive a pension equivalent to the minimum pension. The minimum pension is a minimum amount provided by the social security to pensioners whose pension benefit is below a subsistence threshold. The minimum pension level is set by law each year. Table 1.B1 reports the nominal value of the minimum pension and of its multiples for the years from 1990 to 2017.

Figure 1.1 illustrates the replacement rate schedule for individual spouses in the old and new regime. The x-axis represents the surviving spouse's taxable income net of the survivor benefit, denoted by $z$, and the y-axis represents the survivor replacement rate $b = \frac{B}{P}$, where $B$ is the survivor benefit amount and $P$ the pension of the deceased spouse. The dashed line refers to the old regime, in which a flat replacement rate of 60 percent applies uniformly irrespective of the level of $z$. The solid line refers instead to the new regime, whereby survivors with taxable income in the second, third and fourth income brackets are subject to reduced replacement rates. Denoting by $j$ the income bracket, where $j = \{1, 2, 3, 4\}$, $I_j$ indicates the taxable income threshold between bracket $j$ and $j+1$.[17] Importantly, the replacement rate schedule is kinked and not notched. This feature stems from a provision of the law preventing that the sum of individual income and survivor benefit in a higher income bracket be lower than what would be obtained in a lower income bracket. Formally, the benefit formula in the old regime is $B^O_j = b^O_j \cdot P \; \forall \, j$, where $b^O_j = 0.6$. In the new regime, the benefit formula reads $B^N_j = \max\{b^N_j \cdot P \, , \, b^N_{j-1} \cdot P + I_{j-1} - z\} \; \forall \, z$ in bracket $j$, where $b^N_1 = 0.6$, $b^N_2 = 0.45$, $b^N_3 = 0.36$ and $b^N_4 = 0.3$.

**Interaction with personal income taxation**. The tax base for personal income taxation

[15] The 1995 reform left unchanged the replacement rates for all other categories of recipients, with the exception of single dependent children whose replacement rate increased from 70 to 80 percent.

[16] Reported income refers to the previous fiscal year.

[17] More precisely, $I_1$ is equivalent to three times the minimum pension, $I_2$ four times the minimum pension and $I_3$ five times the minimum pension.

includes all forms of labor income from employment and self-employment, retirement income, pensions and retirement annuities, capital income and rental income, *and* the survivor benefit. Importantly, since survivors under both the old and new regimes are subject to the same personal income tax schedule, income taxes do not affect the income wedge between old- and new-regime survivors and can therefore be assumed away in the analytical framework.[18]

**The 1995 pension reform.** The 1995 reform of survivor benefits was part of a broader set of measures, known as the "Dini Reform", whose main objective was to improve the financial sustainability of the Italian social security system. While remaining pay-as-you-go, the new pension system moved from a defined-benefit to a notionally defined-contribution scheme, and introduced greater flexibility in the retirement age. The reform initiated a progressive transition to the new system: workers with at least 18 years of contributions as of December 31, 1995 remained under the old defined-benefit system; workers with less than 18 years of contributions would be subject to a pro-rata system, with pension benefits computed using the defined-benefit formula up to the end of 1995 and the notionally defined-contribution formula starting from January 1, 1996; workers entering the labor market on or after January 1, 1996 would be fully subject to the new notionally-defined contribution system. In spite of the close timing of the pension and survivor insurance reforms, the former is unlikely to have any confounding effect on the identification of the causal impact of the latter, since the threshold dates of the two reforms are different. In Section 1.3.2, I will provide evidence that the effect of the pension reform is smooth at the September 1, 1995 cutoff.

### 1.2.2 Expected Effects of the 1995 Reform of Survivor Insurance

As illustrated in Section 1.2.1, the 1995 reform of survivor insurance generated a substantial change in the benefit schedule of surviving spouses without dependent children nor grandchildren. In this section, I describe the impact of the reform on the budget constraint of those spouses and its expected effects on their labor supply decisions.

Figure 1.2 illustrates the effect of the reform on the survivor's budget set in the $(z, c)$ plane, where $z$ indicates taxable income net of the survivor benefit and $c$ denotes disposable income. Specifically, $c = z + B(P, b(z)) - T(z + B(P, b(z)))$, where $B(\cdot)$ is the survivor benefit, which is a function of the pension of the deceased $P$ and of the replacement rate $b(z)$; $T(\cdot)$ is a tax function representing personal income taxes payable on taxable income including the survivor benefit ($z + B(\cdot)$). The dashed line represents the individual budget constraint under the old regime, while the solid line the individual budget constraint under the new regime. The vertical bars indicate the income brackets relevant to the determination of the benefit replacement rate in the new regime. Without any loss of generality, the plotted budget is constructed using the mean value of $P$, the

[18] Personal income tax brackets do not coincide with the income thresholds relevant to the computation of the survivor benefit in any of the years in the analysis.

income thresholds and the personal income tax parameters in effect at the time of the 1995 reform.[19] Individual utility increases with disposable income $c$, since disposable income provides consumption, and decreases with taxable income $z$, since it is costly to gain income.

In a static framework, survivors with taxable income above $z > I'_3$ experience a pure negative income effect with no change in the net-of-tax rate. Under the standard assumption that leisure is a normal good, individuals should respond to the negative income shock by increasing labor supply and hence taxable income. The same is true for individuals with taxable income in the ranges $[I'_1, I_2]$ and $[I'_2, I_3]$. Individuals with income $z \in [I_j, I'_j]$ for $j = 1, 2, 3$ experience both a negative income effect and an increase in the marginal tax rate on taxable income.[20] By reducing the net reward from additional work, the reform creates substitution incentives to reduce labor supply and taxable income for these individuals. In particular, given that the marginal tax rate is effectively equal to 100 percent for $z \in [I_j, I'_j]$, it is suboptimal for individuals to locate in this range. Assuming that the income-generating ability distribution is smooth, we should expect all treated individuals who would counterfactually locate in $[I_j, I'_j]$ to bunch at the convex kink $I_j$. However, since the range of taxable incomes over which the reform creates substitution incentives is narrow, one might expect negative labor supply responses to be limited. Conversely, positive labor-supply responses to the pure income effect are expected to arise over most of the taxable income distribution. The reform does not affect individuals with income $z < I$. Thus, we do not expect to observe any changes for these individuals.

From a dynamic perspective, individuals under new-regime rules face lower net returns from each additional year of work. This is illustrated in Figure 1.A1, which shows the relationship between lifetime consumption and the number of years of work (out of the total number of available years). The dashed line represents the individual lifetime budget constraint under the old regime, while the solid line under the new regime. It is clear from the graph that the reform creates dynamic income effects and substitution incentives with opposite expected effects on labor supply: income effects play in the direction of increasing labor supply at the extensive margin, for instance through a delay of labor-market exit and retirement; substitution incentives have the opposite effect.[21] The fact that income and substitution incentives work in opposite directions implies ambiguous expected effects on labor force participation along both the entry and the exit margin. Hence, it is an empirical question whether dynamic income or substitution effects prevail in the context of analysis.

[19] It is apparent from the graph that, by affecting individuals under both regimes in the same way, personal income taxation does not add to the wedge between the old and new regime benefit schedules.

[20] Formally, $I' = (b^N_{j-1} - b^N_j)P + I_{j-1}$.

[21] The discussion rests implicitly on the assumption that leisure is a normal good.

## 1.3 Data and Empirical Strategy

### 1.3.1 Data

I use novel, confidential administrative data from the Italian Social Security (INPS) on the universe of survivor benefits in Italy. The survivor insurance archive comprises all survivor insurance benefits paid out to survivors of deceased retirees, disability insurance recipients and workers in the private sector, with starting date between January 1, 1990 and December 1, 2000. The archive includes detailed annual benefit information for each individual beneficiary within the household. Available information includes the start and end dates of the benefit, the pension of the deceased, the amount of the benefit before and after means testing, the number of beneficiaries in the household and their relationship to the deceased, the survivor's taxable income used to determine the replacement rate and the reason for benefit entitlement loss in case of benefit exhaustion.[22]

The survivor benefit archive can be linked to individual survivors' contributory histories that span from as early as 1900 up to 2017. The contributory archives provide detailed information on the entire working history of individuals, including both employment spells and spells related to in-work and out-of work social insurance, such as parental and family leave, sick leave and unemployment insurance. Information is also available on the duration of each spell and on earnings in each employment spell. The sample covers all employees of the private and public sector, as well as self-employed workers, independent contractors and professionals. For the subgroup of private-sector employees, I link the contributory records to the UNIEMENS file, which gives information on the type of contract held by the worker (i.e. whether full-time or part-time), a unique identifier of the firm, the 5-digit industry code and the province in which he or she works in each year from 1983 to 2017. Finally, the data can be linked to the demographic archive, which provides information on gender, municipality of birth, birth date, retirement date and death date.

Combining the survivor benefit, contributory history and demographic archives, I build up a panel of individual working and benefit histories of survivor benefit recipients at annual frequency. The final dataset is a balanced panel of approximately 95,000 survivors spanning from six years before to 15 years after the spouse's death.[23] The sample comprises all surviving spouses aged 55 and under and not yet retired at the time of their spouse's death, and whose benefit started between September 1, 1993 and August

[22]The data do not report the cause of death.

[23]The balanced panel is conditional on the surviving spouse being alive in the 15 years after the spouse's death, and unconditional on employment and remarriage. When considering a balanced sample of survivors unconditional on survival, the survival rate 15 years after the spouse's death is not discontinuous at the cutoff. Similarly, there is no discontinuity in the survival rate 19 years after the spouse's death in the balanced sample used in the analysis. The remarriage rate – measured 15 years after the spouse's death – is approximately 5.6 percent. Remarriage occurs on average 10 years after the former spouse's death. There is no statistically significant discontinuity in the remarriage rate 15 years after the spouse's death nor in the time to remarriage.

1, 1997.[24] Information on the number of formally dependent children and grandchildren is included in the data.

The first two columns in Table 1.B2 report the mean and standard deviation of a set of individual characteristics for the main sample. The sample is predominantly female (90 percent) and the average age in $t = 0$ is 46.9 years. At the time of their spouse's death, 45 percent have dependent children, aged 13 years on average, and 40 percent are employed. Average annual labor earnings (unconditional on employment) are €6,200.[25] The average monthly survivor benefit in $t = 0$ amounts to €690, which translates into an average annual benefit of €9,700.[26] The table also reports separate summary statistics for surviving spouses whose benefit started before ("control" group) and after ("treatment" group) September 1, 1995.

### 1.3.2 Empirical Strategy and Identification Checks

The 1995 reform naturally defines a treatment and a control group as a function of the spouse's death date: benefits starting before September 1, 1995 fall under the universal scheme (henceforth, "control" group), while benefits starting on or after that date under the means-tested scheme ("treatment" group). In this way, the reform introduces a new, less generous benefit schedule that will coexist parallel to the old one until all old-regime benefits will have been exhausted. Such quasi-experimental variation allows to estimate the causal effect of unearned income on individual labor supply, earnings and total income, by comparing otherwise identical individuals subject to exogenously different benefit schedules for the rest of their lives. The ability to estimate labor supply and total income responses from long-run variation in unearned income is an important feature of this research setting, because it allows to obtain estimates likely not attenuated by short-run optimization frictions and therefore closer to the structural parameter of interest. A second important feature of this research setting is that, by comparing treated and control individuals similarly affected by a spouse's death, it implicitly controls for state dependent preferences and potential anticipation effects.

The structural model that describes the causal relationship of interest is:

$$Y_{it} = \alpha + \beta \cdot B_{it} + X'_{it} \cdot \gamma + \varepsilon_{it} \tag{1.1}$$

where $Y_{it}$ is the outcome of interest $Y$ for individual $i$; $t$ indicates event-time years after the death event; $B_{it}$ is the amount of the survivor benefit received by $i$ in $t$; and $X_{it}$ represents a vector of controls. In this model, the parameter of interest is $\beta$, which captures the causal effect of unearned income $B_{it}$ on the outcome $Y_{it}$. For $Y = z$, where $z$ is taxable

[24] The choice of restricting the sample to spouses experiencing the shock at or before age 55 is motivated by the fact that I analyze long-run labor supply responses up to 15 years after the spouse's death. The modal age of retirement in the data is 60 and retirement can be considered an absorbing state in the Italian context.

[25] All monetary quantities are expressed in 2010 prices.

[26] The annual benefit is equivalent to 14 monthly instalments. Survivors receive twice the monthly benefit amount in July and December each year.

income, $\beta$ identifies the marginal propensity to earn out of unearned income MPE $= \frac{dz}{dB}$. Given the potential endogeneity of $B$, I exploit exogenous variation in the benefit replacement rate due to the 1995 reform and use the September 1995 cutoff as an instrument for $B$.

The policy change lends itself to the implementation of a regression discontinuity (RD) design in the benefit start date around the September 1, 1995 cutoff.[27] The empirical strategy for this RD design is illustrated in Figure 1.3. The x-axis represents the month-year of benefit start, with the vertical line indicating the September 1995 threshold. The graph shows the average replacement rate by month-of-benefit-start bin for surviving spouses with taxable income in the second, third and fourth income brackets in the first year of benefit receipt. The graph provides compelling evidence of the reform implementation: all benefit payments with start date prior to the cutoff had a replacement rate of 0.6; benefit payments with start date immediately after the cutoff have a substantially lower replacement rate. At the threshold, the replacement rate drops by 14 percentage points to 0.44. Because of this sharp and exogenous discontinuity in the benefit replacement rate, I can estimate the structural form in model 1.1 using the September 1995 threshold as an instrument for $B$.

The first stage equation is estimated using a parametric RD specification of the following form:

$$B_{it} = \alpha_0 + \beta_0 \cdot \mathbb{I}[\tau_i \geq 0] + \sum_{k=1}^{K} \alpha_k \cdot \tau_i^k + \sum_{k=1}^{K} \beta_k \cdot \tau_i^k \cdot \mathbb{I}[\tau_i \geq 0] + X'_{it} \cdot \delta + \mu_{it} \tag{1.2}$$

where $\tau_i$ is the benefit start date for survivor $i$ normalized so that $\tau = 0$ at the cutoff date of September 1, 1995, and all other variables are defined as before. The coefficient of interest capturing the effect of the discontinuity at $\tau = 0$ is $\beta_0$. Polynomials in $\tau$ of order $K$ are included to control in a flexible way for the effect of benefit start date $\tau$ on the outcome variable. The reduced-form equation is equivalent to equation 1.2 with $Y_{it}$ as outcome variable:

$$Y_{it} = \theta_0 + \eta_0 \cdot \mathbb{I}[\tau_i \geq 0] + \sum_{k=1}^{K} \theta_k \cdot \tau_i^k + \sum_{k=1}^{K} \eta_k \cdot \tau_i^k \cdot \mathbb{I}[\tau_i \geq 0] + X'_{it} \cdot \lambda + \nu_{it} \tag{1.3}$$

The key assumption for identification in an RD design is that treatment is as good as randomly assigned in a neighborhood of the cutoff and that counterfactual outcomes are smooth at the cutoff. This identification requirement would be invalidated if there were some strategic manipulation around the threshold in anticipation or in response to the policy change. Figure 1.A2 plots the probability density function of benefit recipients by month-year of benefit start for the entire sample (Panel A) and for the subgroup of individuals with taxable income in the second or higher income bracket at time $t = 0$

[27] Note that using the benefit start date as running variable is essentially equivalent to using the deceased's death date, since benefits start on the first day of the month immediately after the death.

(Panel B). There is no visible discontinuity in the density around the threshold in none of the two plots. The McCrary test statistics reported on each panel do not reject the null hypothesis of no discontinuity at the threshold. On top of providing supporting evidence for the identifying assumption, these results also show that the reform had no effect on survivor benefit take-up.

The RD identifying assumption implies that individuals around the cutoff are comparable in their observable and unobservable characteristics. I perform a covariate balancing test using parametric and non-parametric RD specifications. As reported in Table 1.B3, covariates are balanced under both the linear and quadratic parametric specifications, and the local linear regression specification. It is worth emphasizing that the proportion of individuals subject to a defined-benefit pension regime is smooth at the cutoff, indicating that the 1995 reform is not a confounder in the estimation of the causal effect of the 1995 survivor benefit reform. Based on the balancing test results, I select the parametric RD with a second-order polynomial fit and with covariates as my preferred specification. Output tables also report estimates for the parametric linear specification. Estimates are based on month-of-benefit-start bins and a symmetric bandwidth of 24 months. Figures 1.A3, 1.A4 and 1.A5 report parametric quadratic RD estimates of the main outcomes of interest for a set of different bandwidths.

As illustrated in Section 1.2.2, the reform only affects individuals with incomes in the second or higher brackets. In order to focus on the subgroup of individuals that are more likely to be affected by the reform, ideally I would need a measure of the counterfactual income bracket in which treated individuals would locate absent the reform. On the one hand, the observed income bracket in $t = 0$ (i.e. in the first year in which it is recorded in the data) may be sufficiently exogenous to labor supply choices in response to the reform in a neighborhood of the 1995 cutoff. However, it is unlikely to be a good proxy for the long-run income bracket in both the treatment and control group, due to idiosyncratic income shocks correlated with the spouse's death. On the other hand, the observed longer-run income bracket is endogenous to the policy change for individuals in the treatment group. To overcome these limitations, I employ statistical-learning techniques and develop an empirical model to predict the long-run counterfactual income bracket in the treatment group using observations in the control group. Having randomly selected ten percent of individuals in the control group (training sample), I predict their income bracket at time $t = 10$ using a rich set of pre-determined demographic characteristics and variables related to their working history prior to widowhood. Among this rich set of covariates, I select a parsimonious subset of most relevant predictors using a Lasso estimator. Finally, I apply the coefficients of the prediction model – an OLS regression of income bracket in $t = 10$ on the selected covariates – to observations in the treatment group and predict their long-run counterfactual income bracket. This procedure allows to define a group of individuals, in both treatment and control groups, with predicted income in the second or higher income brackets. I conduct the empirical analysis on this sample, since it is the one likely most affected by the reform.

Summary statistics for the sample with predicted income in the second or higher income bracket are reported in Table 1.B4, both for the full sample and for the treatment and control groups separately. As one would expect, the sample of "affected" survivors has larger average labor income (€24,200) and a much higher labor force participation rate (0.96 in $t = -1$) as compared to the full sample. The sample is still predominantly – albeit less prominently – female (64 percent) and slightly younger than the main sample (43.5 years old on average). The average monthly benefit is also higher, consistent with the notion of assortative mating.

As discussed in Section 1.2.2, the policy change creates a large income effect for all individuals with taxable income in the second or higher income brackets. At the same time, substitution incentives may arise as a result of marginal tax rate changes over small portions of the taxable income distribution. In order to identify the marginal propensity to earn out of unearned income – the income effect –, I first estimate the effect of the benefit on taxable income using the IV-RD strategy described above. Formally, if substitution incentives matter and the compensated elasticity is greater than zero, then the IV-RD estimate of $\beta$ provides a lower bound of the true income effect.[28] Secondly, I provide evidence consistent with substitution incentives having a limited role and conclude that the estimated $\hat{\beta}$ coefficient from model 1.1 indeed provides a measure of the structural marginal propensity to earn out of unearned income.

## 1.4 Results

### 1.4.1 First Stage

Based on the empirical strategy outlined in the previous section, I use having benefit start date on or after September 1, 1995 as an instrument for the amount of survivor benefit received. Figure 1.4 shows the first-stage effect of benefit start date on expected lifetime benefit in $t = 0$. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The discontinuity in lifetime benefits is estimated to be approximately €100,000 and is equivalent to a 31 percent drop when compared the mean in the control group.[29] The RD estimate is large and highly statistically significant, indicating that having benefit start date on or after the cutoff date indeed translates into a substantial reduction in benefits.

Table 1.2 reports estimates of the coefficient $\beta_0$ from equation 1.2 using either the annual benefit in $t = 0$ or the expected lifetime benefit in $t = 0$ as outcome variable.

[28] From the Slutsky equation, the total (uncompensated) labor supply response to a benefit change is the sum of a positive compensated effect and a negative income effect ($= dz/dB$).

[29] The mean in the control group is measured as the average of the outcome variable for surviving spouses with benefit start date between May and August 1995.

Estimates in the top panel are based on the sample of individuals with predicted second or higher income bracket, while those in the bottom panel on the full sample of surviving spouses. According to the estimates reported in column (4) of the top panel, individuals with benefit start date after the cutoff receive annual benefits in $t = 0$ that are on average €2140 or 25.2 percent lower than those received by otherwise identical individuals in the control group. The second row of the top panel of Table 1.2 reports the RD estimate of the effect of the reform on survivor's lifetime benefit, which was discussed in the previous paragraph.

The bottom panel of Table 1.2 shows similar estimates for the full sample of surviving spouses. Consistent with part of this sample having taxable income $z < I_1$ and hence not being affected by the reform, the estimated effect is smaller than the one reported in the top panel. Specifically, the annual benefit drop in $t = 0$ is of €600 (7.1 percent of the mean in the control group) and the expected lifetime benefit drop as of $t = 0$ is of €23,600 (3.9 percent of the mean in the control group). These results confirm that the prediction model described in Section 1.3.2 well identifies a subgroup of individuals most heavily affected by the reform.

### 1.4.2 Effect of the Benefit on Taxable and Disposable Income

In this section, I estimate the long-run effect of the benefit on taxable income and disposable income. I first provide reduced-form evidence of the effect of the 1995 reform on the outcomes of interest. I then complement the reduced-form evidence with structural-form estimates of the marginal propensity to earn out of unearned income from IV estimation of model 1.1. In the analysis, I follow an extensive literature that uses taxable income as an all-encompassing measure of labor and other behavioral margins of response to changes in the tax and benefit system (Feldstein, 1995; Saez, Slemrod, and Giertz, 2012). Of course, there could be additional sources of income that are unobserved in the data, for instance undeclared income and income support from relatives. If anything, the effect that I estimate should be a lower bound of the true effect if unobserved income plays a similar role in response to the benefit reduction.

Panel A of Figure 1.5 reports the reduced-form RD effect of the reform on the average annual benefit over the fifteen years after the spouse's death. The graph is constructed pooling event time years from $t = 0$ to $t = 15$. Individuals in the treatment group receive approximately €2,000 less in survivor benefits on average each year – an amount equivalent to 20.7 percent of the mean in the control group. At the same time, their reported taxable income (excluding the survivor benefit) is on average €2,300 or 15.8 percent larger than that in the control group over the same time period (Panel B). The sum of these two roughly equally sized but opposite signed effects implies that the net reduced-form effect on disposable income is quantitatively small and precisely equivalent to €300 or 1.5 percent over the mean in the control group (Panel C). Regression estimates of the reduced-form model for average annual benefit, taxable income and disposable income are reported in Table 1.3. The reduced-form results indicate that individuals fully offset the benefit loss with a tantamount increase in taxable income in the fifteen years following their spouse's death. This is also confirmed by the IV-RD estimates of the $\beta$ coefficient of model 1.1 reported in Table 1.4. According to the estimates in column (3), the marginal propensity to earn out of unearned income is equal to -1, i.e. a €1 decrease in average annual survivor benefits is associated with a €1 increase in taxable income.[30] Such estimated effect is large and provides a lower bound of the true income effect for a positive compensated elasticity. The 95 percent confidence interval around the estimate allows to reject parameter estimates lower than 0.4 in absolute value, which is itself at least twice as large as most existing estimates in the literature. Consistent with the reduced-form evidence, the net effect on disposable income is essentially zero (column (4) of Table 1.4).

Rescaling the estimated income effect by the ratio of the benefit to taxable income provides a measure of the income elasticity, i.e. the percent change in taxable income for a one percent change in the benefit. Since the ratio $B/z$ is approximately 0.6 in a left neighborhood of the threshold, it follows that the income elasticity is approximately -0.6. Based on a 95 percent confidence interval, I can reject elasticities lower than 0.25 in absolute value.

Mean income effects mask substantial heterogeneity across subgroups. As shown in Table 1.B5, the income effect is one order of magnitude larger, in absolute value, for women than for men. Such heterogeneity in income effects likely reflects heterogeneity in the severity of the income shock across gender. Since women are predominantly secondary earners in the household, the benefit drop that female survivors face as a consequence of the reform is, on average, larger than that of male survivors. This is confirmed by the results in Table 1.B5, which show that female survivors lose approximately €2000 per year, while male survivors only €700. Moreover – as secondary earners – female survivors tend to have lower taxable incomes than male survivors, as illustrated in Panel A of Figure 1.A6. The graph plots the empirical distribution of the predicted taxable income bracket by gender and shows that, indeed, female survivors tend to have lower predicted taxable incomes then male survivors. The greater severity of the income shock faced by female survivors is a factor that can help explaining the substantially larger income response among this group. Turning to heterogeneity by age at the time of the spouse's death, the income effect is monotonically decreasing over the life cycle. This pattern may be explained by the fact that the ability to increase earned income declines at older ages, due to both higher disutility from work and slimmer labor market opportunities. The availability of sources of self-insurance other than labor supply, such as savings and children's labor supply, may also be greater at older ages, thus limiting the need to adjust

[30]The estimate of the income effect is robust to different parametric specifications. The linear and quadratic specifications are statistically similar (Table 1.4). The parametric quadratic estimates are stable across bandwidths, with the exception of the 18-month bandwidth (Figure 1.A3).

taxable income.[31]

**Validating the identification of the income effect.** I now turn to investigating how important substitution incentives are in the context of analysis. Firstly, I show that the estimated income effect is robust to the exclusion of individuals with taxable income in a neighborhood of the convex kinks created by the reform. Table 1.B6 reports the IV estimate of the marginal propensity to earn out of unearned income, based on the sample of individuals with predicted income in the second or higher income bracket, with the exclusion of individuals whose observed taxable income falls in the second or third income bracket. The IV-RD estimate of the income effect is in line with the one obtained in the main sample, though less precisely estimated due to smaller sample size.

Secondly, I take advantage of the discontinuities in the marginal tax rate introduced by the 1995 reform to infer the value of the compensated elasticity using a bunching estimator (Saez, 2010; Kleven, 2016). Let individual preferences be defined over disposable income (consumption) and taxable income (work effort). A utility function representing such preferences is $U = u(z - T(z), z/\theta)$, where $T(\cdot)$ is a tax function and $\theta$ is income-generating ability, distributed with probability density function $f(\theta)$. If $T(\cdot)$ is linear and $f(\theta)$ smooth, then the probability density function of $z$ is also smooth. Figure 1.A7 illustrates a theoretical density function of taxable income $z$. The dashed line illustrates the case of a smooth density function. By introducing discrete changes in the marginal tax rate, the reform creates three convex kinks in the budget constraint of treated individuals at $z = I_j$ for $j = 1, 2, 3$. Absent the kink (as under old-regime rules), individuals would locate smoothly along the old-regime budget set. Once introduced, the convex kink creates a disincentive for individuals to locate in the range $[I_j, I'_j]$ (since the marginal unit of income is taxed away at a 100 percent tax rate over that range) and induces individuals who would counterfactually locate in that range to bunch at $I_j$. This behavior will give rise to excess bunching in the taxable income density function at the kink point and to a left-shift in the density above the kink, as illustrated by the solid line in Figure 1.A7. Hence, the presence of bunching provides compelling evidence of taxable income responses to the marginal tax rate change. As shown in Saez (2010), the amount of excess bunching is proportional to the compensated elasticity of taxable income and can be used to identify such elasticity.

The 1995 reform introduced three convex kinks in the budget set of individuals in the treatment group (Figure 1.2). If substitution incentives are at play, we should observe bunching around kinks in the treatment group and a smooth density in the control group. Figure 1.6 plots the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$.[32]

[31] As Panel B of Figure 1.A6 shows, differences in the empirical distribution of predicted taxable income bracket across age groups are limited. For individuals in the 50-55 age group at the time of their spouse's death, the distribution has slightly more mass at the lower end of the distribution.

[32] Results do not change when replicating the analysis around each of the three kinks separately and for each of the event-time years separately.

The vertical bar represents the location of the convex kink. Each dot refers to a €200 bin in the range $[-2,700;2,700]$ centered around the kink. Black circles represent observations in the treatment group (kinked budget), while hollow circles observations in the control group (smooth budget). The empirical distributions of both groups appear rather similar throughout the range and equally smooth around the kink, in spite of the rather different incentives faced by the two groups at that point of the income distribution.

In principle, the absence of excess bunching is consistent with different theoretical interpretations: on the one hand, it is consistent with the compensated structural elasticity being small; on the other hand, it is also consistent with the compensated structural elasticity not being small, but the observed elasticity being attenuated by optimization frictions. Optimization frictions may come in the form of costs of adjusting labor supply, such as hour constraints, or in the form of imperfect information, inattention and inertia. Adjustment costs are believed to be of less importance for self-employed workers and to become more attenuated over time. Yet, even when splitting the sample between self-employed and wage earners – as illustrated in Panels A and B of Figure 1.A8, for individuals in the treatment and the control group respectively –, there appears to be no visible difference in the empirical densities nor excess bunching at the kink for self-employed in the treatment group. This evidence is thus consistent with the fact that adjustment costs may not be responsible for the lack of excess bunching. Adjustment costs, imperfect information and inertia should all fade away in the long term. The graphs in Figures 1.6 and 1.A8 are both constructed using observations for event-time years from $t = 0$ to $t = 15$ – a time span that should be sufficiently long for adjustment costs, information frictions and inertia to dissipate. Thus, the lack of bunching over such a long period of time seems unlikely to be due to these types of frictions.

Cognitive biases may make individuals misperceive the way in which the new-regime benefit schedule affects the budget constraint.[33] One such possibility is that the benefit schedule (and in turn the budget constraint) is understood as notched and not kinked. If this were the case, however, one should still expect to see excess bunching at the kinks, making this type of misperception unsuitable to explaining the lack of bunching. A type of cognitive bias consistent with the absence of bunching is "ironing", whereby individuals make decisions based on average rather than marginal tax rates and, therefore, do not react to the latter. Cognitive bias, inattention and low salience of the benefit schedule are all factors that could explain the absence of bunching. Whilst I cannot completely rule out their playing a role, nonetheless I can exclude that individuals are responding to static substitution incentives in the context that I study. Individuals may still be responding to dynamic substitution incentives, in which case the estimated $\hat{\beta}$ is a lower bound (in absolute value) of the structural income effect.

**Comparison with existing quasi-experimental estimates.** The taxable income response estimated in this paper is substantially larger than the existing empirical estimates

[33] This is what Liebman and Zeckhauser (2004) call "schmeduling".

of the marginal propensity to earn out of unearned income. As outlined in the introductory section, the literature on NIT experiments and lottery wins places a consensus estimate of the income effect at approximately -0.10 (Robins, 1985; Hum and Simpson, 1993; Ashenfelter and Plant, 1990; Imbens, Rubin, and Sacerdote, 2001; Cesarini et al., 2017). Yet, recent studies have found larger income effects on earnings in the context of disability insurance and social security wealth in the US. Deshpande (2016b) estimates a parental earnings response to the loss of Supplemental Security Income of approximately -1.4, while Gelber, Isen, and Song (2016) and Gelber, Isen, and Song (2017) estimate an upper bound of the elderly earnings response to the Social Security "Notch" of -0.6 for men and -0.89 for women.

The results in this paper are not necessarily inconsistent with the smaller estimates found in the literature. I here consider potential explanations for finding large income effects. Firstly, differences in the observable and unobservable characteristics – such as the degree of risk aversion – of the populations of analysis may explain differences in their marginal propensities to earn out of unearned income.[34] Secondly, responses may differ with respect to the type of income shock. In this regard, individuals may respond asymmetrically to gains and losses of unearned income (Deshpande, 2016b): responses to unearned income losses are likely to be larger than responses to unearned income gains whenever individuals are loss averse, have minimum income targets or sticky consumption habits (Kőszegi and Rabin, 2006; Chetty and Szeidl, 2007; Chetty and Szeidl, 2016).[35] The degree to which individuals are *ex-ante* insured against the income shock is also a factor that can influence the magnitude of the income effect. Finally, individuals may behave differently with respect to different types of income and, consequently, have different marginal propensities to consume or earn out of different sources of unearned income (e.g. lottery wins as opposed to welfare transfers). This is what Thaler, 1990 refers to as (absence of) fungibility. Given the available data, I have limited ability to probe these explanations.

**Compatibility with macro elasticities of labor supply.** I also examine whether the micro elasticity that I estimate is consistent with macro elasticities of labor supply. It has long been recognized that estimates of steady-state macro elasticities diverge from micro ones. Specifically, macroeconomic models of cross-country variations in hours worked imply elasticities that are much larger than those estimated using sources of identifying variation at the micro level (Chetty et al., 2013). The literature has identified two main factors that can reconcile the macro and micro evidence on the elasticity of labor supply: optimization frictions (Chetty, 2012) and the indivisibility of labor (Rogerson, 1988;

[34]As shown in Chetty (2004), the coefficient of risk aversion is directly related to the size of the income effect on labor supply. *Ceteris paribus*, large income effects are evidence that utility over consumption is highly curved. Intuitively, if an individual increases labor supply sharply in response to a given drop in unearned income, it must mean that the marginal utility of consumption increases quickly when income falls, meaning that the individual is highly risk averse.

[35]Whilst individuals in the new regime never got the higher, old-regime benefit level, it is still the case that they may have expected higher benefits, especially given that the reform was little anticipated.

Ljungqvist and Sargent, 2007; Rogerson and Wallenius, 2009). Specifically, optimization frictions are likely responsible for the substantial attenuation of micro elasticities. The small and short-run policy variation that is typically exploited to identify micro elasticities is unlikely to generate large labor supply responses precisely due to optimization frictions such as adjustment costs. On the other hand, labor supply indivisibility – whereby agents face fixed labor-market entry costs or intensive-margin rigidities – is a feature of several macroeconomic models that reproduce large labor supply elasticities, and show that both intensive and extensive margins of labor supply are important to describe hour fluctuations.

Using data for OECD countries from 1985 to 2015, I run a simple regression of the logarithm of hours of work per person on the logarithm of GDP per hour, controlling for country and calendar year fixed effects.[36] Figure 1.A9 reports a binned scatter plot of the regression of interest. The estimated elasticity of hours worked to GDP per hour is -0.56.[37,38] The latter is statistically compatible with my micro estimate, and with the steady-state frictionless hour elasticity identified in Chetty (2012). Overall, this result suggests that the long-run identifying variation exploited in this paper can indeed prove useful in delivering a parameter estimate not attenuated by short-run optimization frictions.

### 1.4.3 Labor Supply Responses

The large taxable income response to the benefit cut prompts several questions. In this and the following section, I probe the mechanisms behind the income response. I first investigate the anatomy and dynamic of the labor supply response, and then examine effects on program substitution.

**Anatomy of labor supply responses.** The effect on earned income can be decomposed along three margins: labor force participation, hours of work and the wage rate. Figure 1.7 shows the reduced-form effect of the reform on labor force participation, pooling event-time years from $t = 0$ to $t = 15$. Labor force participation is 7.6 percentage points higher to the right of the cutoff. This effect is equivalent to a 12.7 percent increase over the mean in the control group. The IV-RD estimate reported in Table 1.6 indicates that an average annual €1,000 decrease in the survivor benefit leads to a 4 percentage point increase in labor force participation (6.6 percent over the mean in the control group). Another measure of the extensive margin of labor supply is the number of years of cumulated experience in the 15 years after the spouse's death. As reported in Table 1.5,

[36] The countries included in the sample are Australia, Belgium, Canada, Germany, Denmark, Finland, France, the United Kingdom, Italy, Japan, the Netherlands and the United States.

[37] The robust standard error of the coefficient estimate is 0.065. The estimated macro elasticity is likely to conflate both substitution and income effects, and thus likely to provide a lower bound of the income elasticity itself.

[38] I also run an alternative specification in which I regress the logarithm of hours of work per person on the logarithm of total factor productivity, controlling for country and calendar year fixed effects. The estimated elasticity is -0.30 (robust standard error 0.077). Results are available upon request.

cumulated experience in $t = 15$ is approximately 1.1 year higher for individuals in the treatment group.

The observed participation response could be due to either increased entry in the labor market or delayed exit from the labor market. Figure 1.8 shows a decomposition of the increase in cumulated experience over the 15 years after the death along the entry and exit margin. Specifically, the first bar to the left reports the average increase in cumulated work experience over those 15 years (equivalent to 1.1 years). The remaining three bars decompose such effect into increased entry (second bar from the left) and delayed exit, distinguishing between delayed exit in the form of delayed non-employment (third bar from the left) and delayed retirement (fourth bar from the left). I measure the entry margin by looking at the participation response of individuals who were not working in $t = -1$ and weight the estimate by the share of such individuals in the full sample. I measure delayed non-employment and delayed retirement as the reduced-form effect on the number of years not in employment (excluding retirement) and the number of years in retirement over the 15 years after the spouse's death (weighted by the share of individuals who were working in the year before their spouse's death). The observed participation response is driven both by increased entry and postponed retirement. In particular, delayed retirement appears to be the main driver of the labor supply response. The delay-effect on retirement is also confirmed by the reduced-form estimates in Table 1.5 and the IV estimates in Table 1.6: according to the latter, an average annual €1,000 decrease in the survivor benefit leads to a 10 percentage point decrease in the retirement rate in $t = 15$, representing a 19 percent decrease relative to the mean in the control group.[39]

Being an average effect, the result in Figure 1.8 is largely driven by the age composition of the sample and masks important responses along the entry margin by individuals at younger ages. To shed light on this point, Figure 1.9 outlines the profile of the participation response by age in $t = 0$. The shaded area shows the 95 percent confidence interval of the reduced-form RD estimate for labor force participation for individuals in different age groups, irrespective of their employment status in $t = -1$. Black circles report the same coefficient for individuals who were not working in $t = -1$, and hollow circles for individuals who were working in $t = -1$. The mean effect on participation (represented by the shaded area) is therefore a weighted average of an entry effect (represented by the black circles) and a delayed exit effect (represented by the hollow circles). Comparing the magnitude of the labor supply response in the full sample with that in the subgroup that was not working in $t = -1$, one can infer that the entirety of the labor force participation response of individuals in younger age groups (20-40 and 41-50 years

[39] I probe the heterogeneity of the retirement rate response between individuals employed in the private and the public sector. To this end, I focus on individuals who were working in the years prior to their spouse's death and construct an indicator variable for being employed in the public or private sector based on their employment history in $t < 0$. I find that the retirement rate response is entirely driven by individuals employed in the private sector. This is consistent with the notion that public-sector employees have limited ability of adjusting the retirement margin. Results are available upon request.

old) is in the form of labor market entry. Conversely, the participation response comes predominantly from delayed labor market exit for individuals in older age groups (51-55 years old).

Having established substantial extensive-margin responses, I now move to investigating intensive-margin and wage rate responses. Since the data provide information on days worked but not hours worked, I use days worked as a measure of the intensive margin of employment. The wage rate is defined as earnings per day worked conditional on employment. When analyzing outcomes conditional on employment, I control for potential endogenous selection into employment by including the number of years of work experience in $t = 0$ among the individual-level covariates (Schmieder, Wachter, and Bender, 2016). Albeit imperfectly, this allows to isolate the effect of the reform on hours worked and the wage rate from that of compositional changes of the workforce due to extensive-margin responses to the reform itself. The IV estimates in Table 1.6 show a statistically significant, yet mild effect of the benefit on days worked and on the wage rate: a €1,000 benefit drop is associated with a decrease in days worked of 2.6 days per annum and a decrease in the daily wage of €2.2 on average. The results suggest that surviving spouses may be moving to part-time, slightly lower paid jobs. However, both effects are especially small, both in absolute terms and in percent of the mean in the control group (0.7 and 2.3 percent respectively).

I further investigate the anatomy of the labor supply response by looking at the conditional probability of holding a full-time job, and of changing firm, industry and province of work.[40] I observe these outcomes only for the subsample of individuals with a job in the private sector. According to both the reduced-form and IV estimates reported in Tables 1.5 and 1.6 respectively, no statistically significant effect can be detected on the probability of holding a full-time job nor of changing firm, industry or province.

**Dynamic of labor supply responses.** The participation response estimated pooling all event-time years masks interesting dynamics. Figure 1.10 uncovers the evolution of the participation response over event-time years from $t = -6$ to $t = 15$.[41] Black circles report the reduced-form RD estimate at each event-time year in percent of the mean in the control group. Vertical capped bars indicate 95 percent confidence intervals. Consistent with the absence of anticipation of the reform and/or manipulation around the threshold, there is no discontinuity in the participation rate in the years before the spouse's death. The participation response unfolds progressively over time, being small and statistically insignificant in the first two years after the shock and then growing quite steadily over time, from 7 percent in $t \in [2;3]$ to 18 percent in $t \in [14;15]$. Analogous to Figure 1.10, Figure 1.A10 reports the evolution of the labor force participation response in levels. The effect is muted up to event time $t = 1$, grows to a statistically significant 6.4 percentage point difference at event time $t \in [2;3]$ and then stabilizes at an approximately 7

[40] I look at transitions across 3-digit industries.

[41] To improve the precision of the estimates, I estimate dynamic effects pooling event-time years into biennia.

percentage point difference in subsequent event-time years. The evolution of the labor supply response is consistent with the notion that optimization frictions, such as adjustment costs or attention costs, attenuate responses in the short-run and fade away over time, allowing to uncover frictionless structural responses only in the medium-long run.

I also examine the dynamic of hours worked and the wage rate. Figures 1.11 and 1.12 report the reduced-form effect on the number of days worked and the daily wage conditional on employment at each event-time year. The dynamic of both variables is essentially flat throughout the 15 years following the spouse's death, indicating no intensive-margin adjustments and suggesting that work experience, human capital accumulation and effort have a limited role in the context under study.

**Heterogeneity of labor supply responses.** There is substantial heterogeneity in participation responses by gender. As reported in Table 1.B5, the female participation rate increases on average by 10 percentage points (15.8 percent of the mean in the control group) in the 15 years after the spouses death, while the male participation rate by 4.5 percentage points (8.1 percent of the mean in the control group). This difference is consistent with what found for the income effect in Section 1.4.2. A stark gender differential also emerges when examining the dynamic pattern of labor force participation over event-time years, as shown in Panel A of Figure 1.A11: the dynamic of the female subgroup displays a spectacular growth over event-time years, while that of the male subgroup is rather steady. There is no statistically significant difference by gender in the intensive margin response, as measured by the number of days worked (Table 1.B5). As for the wage rate, male survivors experience a statistically significant decrease in the daily wage, conditional on employment, of approximately €5.85, equivalent to 7 percent of the mean in the control group. No significant effect is detected for female survivors.

The dynamic of the participation response by age at the time of the spouse's death confirms the role of retirement as a margin of adjustment: the labor force participation response of individuals in the 51-55 age group increases sharply over event-time years 2 to 7, and then drops to zero in subsequent years. This is consistent with a delay in retirement occurring in the late fifties and early sixties. Interestingly, an analogous increase in labor force participation emerges around event-time years 12 to 15 for individuals in the 41-50 age group. As reported in Table 1.B5, the age profiles of the intensive margin response and the wage rate response, conditional on employment, are essentially flat and statistically insignificant, except for a small, positive and statistically significant effect on the wage rate for individuals in the 20-40 age group, which are found to increase by approximately 1.8 percent.

### 1.4.4 Program Substitution

The reduction in survivor insurance generosity may induce survivors to take up more of other social insurance and social assistance programs in order to increase their disposable income. This is what previous studies have defined *program substitution* (Inderbitzin, Staubli, and Zweimüller, 2016).[42]

**Social insurance take-up.** The data provide information on the take-up of work-related social insurance benefits, such as paid family leave, paid sick leave and unemployment benefits. Paid family leave includes both maternity/paternity leave and parental leave provided to individuals who need to take time off work to care for an ill child or relative. Paid sick leave is a benefit paid to workers who need to take time off work while sick. Unemployment benefits are publicly-provided benefits granted to laid-off private-sector employees. Since the take-up of these social insurance benefits is conditional on being employed at the time of take-up or on having been employed in the previous months, I restrict the sample to surviving spouses in employment in $t$ or $t-1$. Moreover, in order to control for potential endogenous selection into program eligibility due to the conditioning on employment status, I control for the number of years of working experience in $t = 0$. According to the IV estimates in Table 1.6, every €1,000 decrease in benefits increases the probability of taking up paid family leave by 0.3 percentage points, which represents 37.5 percent of the mean in the control group.[43] The increase in paid family leave suggests that surviving spouses who increase their labor supply may be doing so under substantial work-time constraints due to family and care duties. While no significant effect can be detected on the probability of taking up paid sick leave, unemployment insurance take-up increases by 1.7 percentage points for every €1,000 decrease in benefits (a 100 percent increase over the mean in the control group). These results indicate that individuals in the new regime compensate for the less generous survivor benefits by increasing their take-up of alternative welfare programs. In this respect, the increase in unemployment insurance take up suggests that individuals may be willing to pay the cost of unemployment stigma to increase disposable income.

**Children's dependency period.** The 1995 reform reduced the benefit replacement rate for surviving spouses with no dependent children, while leaving unchanged the replacement rate for surviving spouses with one or more dependent children, who face a replacement rate of 80 percent and 100 percent respectively. Hence, the benefit drop experienced when children lose their dependency status is larger for surviving spouses with benefit start date on or after the September 1, 1995 threshold. This is confirmed by the results in Figure 1.A12 and in the first row of Table 1.8. The latter reports the estimated effect of the reform on the benefit received by surviving spouses upon loss of children's dependency. Individuals in the treatment group suffer a €1,305 larger benefit loss than individuals in the control group. The level effect corresponds to 16.6 percent of the mean in the control group. It follows that, at the margin, one extra year with children as dependent is much more valuable for individuals in the treatment than the control group. Indeed, as shown in Figure 1.13, the number of years with dependent

[42] In principle, less generous benefits may also affect the take-up of survivor insurance itself. However, the results presented in Figure 1.A2 allow to exclude any differential take-up around the threshold.

[43] Reduced-form estimates are reported in Table 1.7.

children is 1.2 years greater in households with benefit start date to the right of the cutoff. The IV-RD effect reported in Table 1.6 indicates that a €1,000 benefit drop increases the dependency period by 0.7 years – a 10.7 percent increase above the control mean.[44] To be classified as dependent, a child must be either aged under 18, or enrolled in high school and not working up to age 21, or enrolled at university and not working up to age 26. Thus, extending children's dependency period can be viewed as a costly action – namely paying enrolment fees – that surviving spouses undertake in order to increase disposable income.

## 1.5 Interpretation

### 1.5.1 External Validity and Policy Relevance

Whilst there is a selection issue of studying survivor benefit recipients, nonetheless there may be scope for generalizing the findings obtained in this context. Individuals in my sample are spouses – prevalently women – who become widow(er)s in their mid forties. Single-parenthood, and single-motherhood in particular, are among the states most at risk of income insecurity and single parents make up a large proportion of welfare recipients of programs such as Earned Income Tax Credit (EITC), Aid to Families with Dependent Children (AFDC) and Temporary Assistance to Needy Families (TANF). Hence, to the extent that widow(er)s in my sample can be representative of single parents in general, my findings can be relevant to a larger set of public policies in the US and in Europe.

To assess the validity of this hypothesis, I compare the characteristics of survivor benefit recipients in my sample with EITC recipients in the US, using data from the March Current Population Survey (CPS) in the years 1993 to 1997. Summary statistics for the sample of household heads receiving EITC are reported in Table 1.B8, where column (1) refers to both married and single household heads and column (2) to single parents or single individuals. EITC recipients tend to be younger than individuals in my sample. Consequently, a higher fraction has dependent children and dependent children tend also to be younger. Apart from the age composition, labor force participation and taxable income are in line with those of benefit recipients in my sample.

Losing a spouse at a young age is a low-probability and relatively unpredictable event against which households are likely to be limitedly insured. In this respect, my estimates

[44] A potential concern is that the estimated increase in the dependency period is spuriously driven by the fact that the cutoff date is in September – the month in which school years start and university enrolment takes place – and children who lost one of their parents in August may end up delaying their school or university enrolment by approximately one year. I test the validity of this alternative hypothesis by running placebo RD regressions around the September cutoff in the three years before and after 1995. Results are reported in Table 1.B7. The estimates reveal a statistically significant positive effect only around the September 1995 cutoff. The estimated effect for September 1994 is statistically significant, but of negative sign. Overall, these results lend support to the idea that the observed increase in the dependency period is indeed a behavioral response to the incentives created by the 1995 reform.

of the labor supply and income response are likely to provide an upper bound of what would be expected for "shocks" that are easier to anticipate and insure *ex ante*, such as own or spousal job loss.

Finally, the elasticity of labor supply may differ between marriage and widowhood. For instance, the lack of leisure complementarities between spouses and the desire to engage in working activities due to loneliness may make widow(er)s' labor supply more elastic. Conversely, the inability to share family-related duties with their partners may make widow(er)s' labor supply less elastic. To the extent that labor supply elasticities may differ across marital statuses, the effects estimated in this paper are likely to provide an upper or a lower bound for what would be expected for married individuals.

### 1.5.2 Implications for Theoretical Models of Labor Supply

In this section I examine how the findings in this paper connect with theories of labor supply. One simple way to theoretically rationalize the magnitude of the estimated income effect is to assume that individual preferences are quasi-linear in labor. Similar to the framework introduced in Section 1.4, suppose individual preferences are defined over consumption $c$ (disposable income) and work effort $\frac{z}{\theta}$, where $z$ is income from work and $\theta$ income generating ability. Individuals maximize a utility function $U(c,z)$ that is concave in consumption and linear in work effort

$$U(c,z) = u(c) - \frac{z}{\theta} \tag{1.4}$$

subject to the budget constraint $c = z + B$. The optimal levels of consumption and work $(c^*, z^*)$ are such that $\partial c^*/\partial B = 0$ and $\partial z^*/\partial B = -1$.[45] In response to a drop in unearned income, income from work increases one-for-one and the level of consumption remains unchanged.

The "time-averaging" or "career-length" model proposed by Ljungqvist and Sargent (2007) is one example of a dynamic model that – in the reduced form – delivers predictions that are observationally equivalent to those of the above static model with quasi-linear preferences. Ljungqvist and Sargent (2007) construct a non-stochastic, continuous-time life-cycle model with time-separable preferences and labor supply indivisibility, in which a representative agent decides what fraction of her lifetime to devote to work.[46] A model of this type delivers a high labor supply elasticity at the extensive margin (i.e. in the number of years of work), which is observationally consistent with the finding in this paper that individuals lengthen their careers by delaying retirement in response to a negative income shock.

[45]The optimal levels of consumption and work are (implicitly) defined by $u'(c^*) = \frac{1}{\theta}$ and $z^* = \frac{1}{\theta} - B$.

[46]The model also assumes a constant wage rate and no credit-market constraints.

## 1.6 Normative Implications

A central result of this paper is that benefit losses trigger large labor supply and earned income responses. Why is the income effect that I estimate so large? On the one hand, if an individual increases labor supply sharply in response to a benefit drop, that lost income must be of high utility value. On the other hand, a substantial labor supply response may arise if labor-supply adjustment costs are low. Understanding which of these two alternative mechanisms – high utility value vs. low adjustment costs – prevails is important for welfare analysis.

To gain more formal intuition of the interplay between utility value and adjustment costs, assume individuals choose their consumption and labor force participation status to maximize a utility function that satisfies

$$u(c) - \mathbf{I}\{l = 1\} \cdot \phi \tag{1.5}$$

where $u(\cdot)$ is a concave function, $c$ is consumption, $l \in \{0,1\}$ a binary labor force participation decision and $\phi$ an additively separable utility cost of work that individuals incur when participating to the labor market. Assume that $\phi$ is distributed according to a type III extreme value distribution with cumulative distribution function $F(\cdot)$ and probability density function $f(\cdot)$, with $f' < 0$.[47] Utility is maximized subject to a budget constraint $c = \{l = 1\} \cdot z + B$, where $z$ is labor income and $B$ the survivor benefit. Denoting the utility-maximizing labor force participation rate with $\Phi$, the labor force participation response to a benefit change can be written as

$$\frac{d\Phi}{dB} = -\gamma \cdot \frac{d\Phi}{dz} \cdot \frac{z}{B} \tag{1.6}$$

where $\gamma$ is a coefficient of relative risk aversion and $d\Phi/dz$ is the labor force participation response to a wage-rate change. The latter is a negative function of the utility cost of work ($\phi$). The formula shows that labor supply responses to unearned income losses are larger whenever: *(i)* utility over consumption is highly curved and the marginal utility of consumption rises sharply as consumption falls (as captured by higher values of $\gamma$); or *(ii)* the responsiveness of labor force participation to the wage rate is high, or equivalently the utility cost of adjusting labor supply is low.[48] In the following sections I first provide evidence of there being adjustment costs associated to the observed labor supply response. I then develop a revealed-preference method to infer the value of the benefit from observed participation responses to benefit losses.

[47] A distribution with these characteristics is a Weibull distribution with shape parameter $\sigma < 1$.
[48] This result is based on Chetty (2004), Chetty (2006b), and Chetty (2008).

### 1.6.1 Evidence on Adjustment Costs

I investigate how the participation response to a given benefit drop correlates with a measure of the cost of adjusting labor supply. Evidence of a negative correlation between the labor supply response and such measure is consistent with the notion that adjustment costs are non-zero in the context analyzed. Figure 1.14 shows heterogeneity in the semi-elasticity of labor force participation to the benefit by different levels of the regional unemployment rate in the region where the individual resides.[49] Individual observations are binned into the quartiles of the distribution of the regional unemployment rate in each calendar year.[50] The graph shows that the participation response is larger – in absolute value – for lower levels of the unemployment rate.[51] The cost of increasing labor supply is likely to be larger when the unemployment rate is higher, either because finding a job requires more search effort or because keeping the current job requires more on-the-job effort. All in all, these results are suggestive of there being important labor supply adjustment costs, which may be especially pronounced for some groups of individuals.

### 1.6.2 A Revealed-Preference Approach for Estimating the Value of Transfers

The extent to which individuals increase work effort in response to a drop in unearned income reveals, *ceteris paribus*, the consumption value that such lost income would have provided. Larger responses must mean that the lost income is highly valued and that there are large welfare gains from recouping it. In this section, I demonstrate that the extent to which surviving spouses increase their labor supply in response to a realized drop in the survivor benefit reveals their implicit valuation of the benefit itself and can therefore be used to measure the value of transfers within the widowhood state. The value of the marginal unit of transfers (denoted by $MB$) is captured by the percent change in the marginal utility of consumption between the low-benefit and the high-benefit states

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \tag{1.7}$$

This ratio provides a measure of the welfare gain from transferring a unit of benefit from the high- to low-benefit state. The higher the marginal utility of consumption in the low- relative to the high-benefit state, the larger the gains from such transfer and, more generally, from increasing the generosity of survivor insurance benefits.

[49] Data on the regional unemployment rate at annual frequency are taken from ISTAT. I match each individual-year observation with the regional unemployment rate in that same year in his/her region of residence.

[50] I combine the second and third quartiles to improve the precision of the estimates.

[51] In the Italian economy, higher rates of unemployment are typically correlated with higher rates of undeclared work, which may also explain the pattern obtained in Figure 1.14. In order to control for the potential confounding role of the level of the black economy, I include the regional rate of undeclared work at the annual level among the regression covariates. The rate of undeclared work is measured as the ratio of estimated irregular full-time-equivalent employment over estimated total full-time-equivalent employment. Data on the rate of undeclared work are taken from ISTAT.

**Proposition 1.** *Let individual utility be given by* $u(c) - \mathbf{I}\{l = 1\} \cdot \phi$*, where c is consumption,* $l \in \{0, 1\}$ *a binary labor force participation decision and* $\phi$ *an additively separable utility cost of work, distributed with cumulative distribution function* $F(\phi)$*. Consumption c cannot exceed the sum of labor income z and the survivor benefit B. Denote the optimal level of labor force participation with* $\Phi$*. Then,*

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \approx -\frac{\left[\frac{d\Phi}{d\log B}\right]}{\varepsilon} \tag{1.8}$$

*where* $\varepsilon$ *is the semi-elasticity of labor supply to labor earnings.*

*Proof.* See Appendix 1.C.1. □

Proposition 1 shows that the value of the benefit can be identified by scaling the semi-elasticity of labor force participation to the benefit by the semi-elasticity of labor supply to labor earnings. If labor supply is relatively inelastic to changes in the wage rate, then larger participation responses indicate that surviving spouses have a high valuation of extra resources in the widowhood state.[52] The intuition behind this result is that the extent to which individuals undertake costly actions to increase their consumption in the low-benefit state provides a measure of the utility gain that they would get from more generous transfers. From a theoretical standpoint, by exploiting labor supply responses *within* the widowhood state, Proposition 1 allows for both state-dependent preferences and anticipation responses. By relying on optimizing behavior, revealed-preference methods work under the assumption that individuals are not subject to optimization frictions that prevent them from optimally responding along the relevant adjustment margin. They also assume the absence or separability of other margins of adjustment.

**Empirical implementation.** For the empirical implementation of the result in Proposition 1, I use the IV-RD estimate of the semi-elasticity of labor force participation to the benefit reported in column (2) of Table 1.6 and the simulated value of $\varepsilon = 0.6$ as in Blundell et al. (2016).[53] The value of $-d\Phi/d\log B$ that I estimate in the data is approximately 0.3. Rescaling it by 0.6, I obtain a measure of the value of the marginal unit of transfer equivalent to 0.5.[54] This suggests that the marginal value of consumption is 50 percent higher among widow(er)s in the low-benefit regime as compared to widow(er)s in the high-benefit regime.

It is useful to consider how this result compares to other existing estimates in the literature. Several papers have both developed estimation methods and provided actual estimates of the welfare gain from increased welfare generosity. This has been more

[52] The model builds on previous work by Chetty (2008) and Landais (2015).

[53] Based on simulated data, Blundell et al. (2016) calculate a semi-elasticity of participation of 0.38 for single women with no children and 0.78 for lone mothers. Weighting these two elasticities by the share of survivors with and without dependent children in my sample, I obtain a weighted average of approximately 0.6.

[54] The 95 percent confidence interval of this effect ranges from 0.33 to 0.62.

marked in the context of unemployment insurance (Gruber, 1997; Chetty, 2008; Landais, 2015; Hendren, 2017; Landais and Spinnewijn, 2018), but recent work has also focused on survivor insurance (Fadlon and Nielsen, 2015; Fadlon and Nielsen, 2018). In the context of unemployment insurance, consumption-implementation approaches that exploit the causal effect of job loss on consumption provide estimates in the ballpark of 0.2 (Gruber, 1997). More recent evidence using *ex-ante* consumption and spousal labor supply responses finds a value of unemployment insurance of approximately 0.5-0.6 (Hendren, 2017). To the best of my knowledge, the only existing estimate of the value of survivor insurance is provided by Fadlon and Nielsen (2015). Rescaling changes in survivors' labor supply around spousal death by a measure of the utility cost of work, they estimate a value of survivor insurance that ranges from 0.03 for surviving spouses aged less than 60, to 0.94 for spouses of older ages. In comparison with existing estimates, the value of $MB = 0.5$ that I estimate appears therefore relatively high, suggesting that widowhood is a state with a high marginal utility of consumption and in which increased survivor benefit generosity would deliver substantial welfare gains.

## 1.7 Conclusion

In this paper, I provide novel estimates of the income effect of welfare transfers on individual labor supply, earnings and total income, in the context of the Italian survivor benefit program. I find that surviving spouses respond to benefit losses with one-for-one increases in earned income, implying a marginal propensity to earn out of unearned income of -1.0. This large earnings response stems from increased labor force participation, in the form of increased entry into the labor market by younger survivors and delayed retirement by older survivors. No intensive-margin nor wage rate response to the benefit drop is detected. Individuals are found to significantly increase the take-up of paid family leave and unemployment insurance benefits in response to the benefit drop. Because the presence of dependent children grants a more generous allowance, households that will experience the largest benefit drops upon loss of dependency, extend tertiary education enrolment by almost 20 percent in order to delay the benefit drop. Thus, labor force participation and program substitution both emerge as margins through which individuals increase their disposable income in response to a realized drop in unearned income.

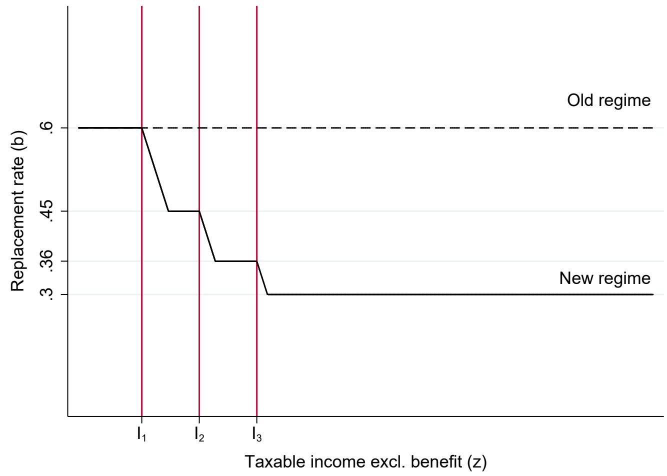
I develop a simple model of extensive labor supply choices and demonstrate that participation responses to realized benefit losses are revealing of the implicit valuations of welfare transfers in the widowhood state. The intuition behind this result is that the extent to which individuals undertake costly actions to increase disposable income in response to a benefit drop are informative of their valuation of the extra consumption that would be provided by the benefit. According to the model's predictions, the large observed participation responses imply that widowhood is a state with high marginal utility of consumption and that substantial welfare gains could be obtained from increased survivor insurance generosity. Because it requires labor supply rather than consumption data, the revealed-preference approach that I propose is potentially widely applicable given the increasingly large availability of detailed data on individual labor supply from administrative and other sources. Moreover, being based on estimates of participation responses to unearned income, the approach can be applied to a broad class of public policies involving income transfers.

Whilst there is a selection issue of studying survivor benefit recipients, nonetheless I believe there is scope for generalizing the findings in this paper to other contexts. To the extent that widow(er)s in my sample can be representative of single parents, and single mothers in particular, my findings can be relevant to a larger set of public policies in the US and in Europe. On the other hand, the likely low probability and predictability of spousal death at younger ages implies that households are probably limitedly insured against the associated income shock. In this respect, my estimates of the income effect and of the implicit valuation of the benefit are likely to provide an upper bound of what would be estimated for "shocks" that can be anticipated and that are easier to insure *ex ante*.

The normative assessment that I draw in this paper is based on a partial equilibrium framework. A comprehensive evaluation of the welfare implications of reduced survivor insurance generosity would require an appraisal of the long-term consequences on intergenerational educational and labor market outcomes, and on individual well-being. From a general-equilibrium standpoint, it would also be important to understand how the provision of survivor insurance benefits affects decisions regarding human capital accumulation, marriage and fertility (Borella, De Nardi, and Yang, 2017; Low et al., 2018; Persson, forthcoming). I see these as interesting avenues for future research.

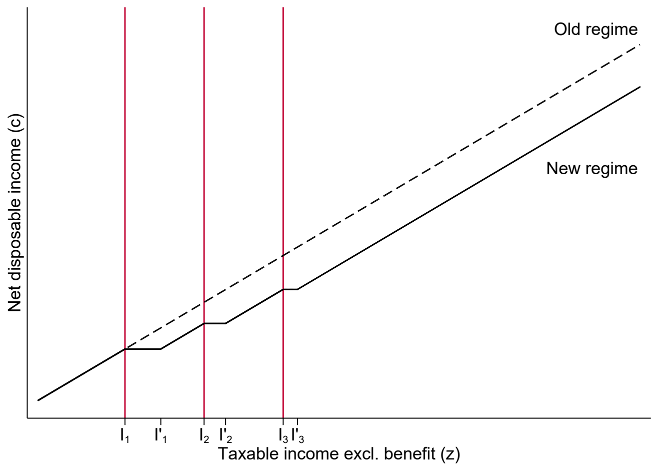
## 1.8 Figures

FIGURE 1.1: BENEFIT REPLACEMENT RATE SCHEDULE



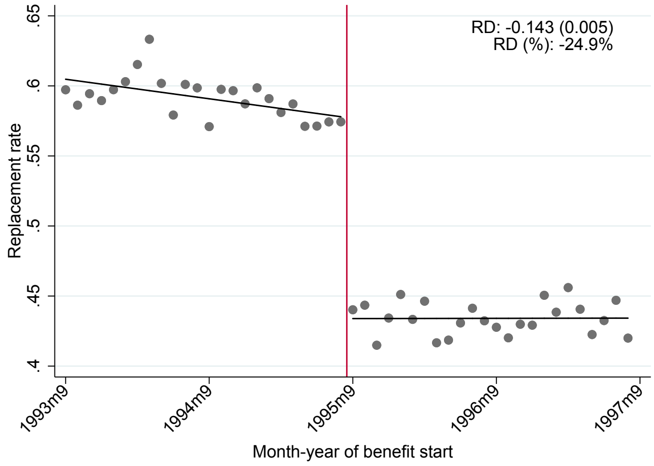

**Notes:** The graph reports the benefit replacement rate schedule for surviving spouses without dependent children or grandchildren in the old and new regime. The x-axis represents the surviving spouse's taxable income net of the survivor benefit ($z$) and the y-axis represents the survivor replacement rate ($b$). The dashed line refers to the old regime, while the solid line refers to the new regime. $I_j$ for $j = 1, 2, 3$ indicates the income thresholds at which the replacement rate changes under new-regime rules: $I_1$ is equivalent to three times the annual minimum pension, $I_2$ to four times the annual minimum pension and $I_3$ to five times the annual minimum pension. The nominal values of the minimum pension and of its multiples for the years from 1990 to 2017 are reported in Table 1.B1.

FIGURE 1.2: BUDGET CONSTRAINT



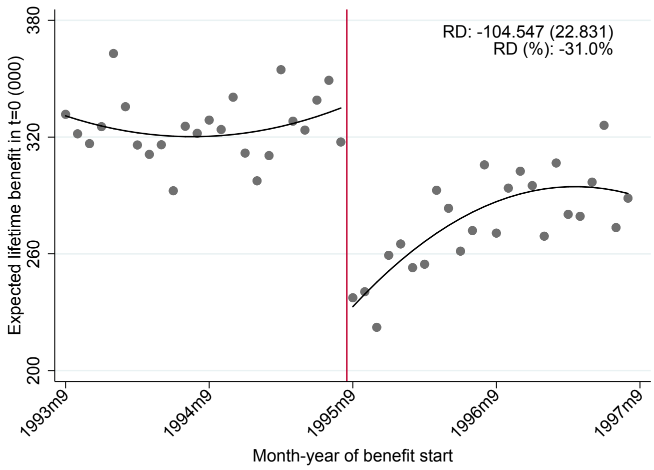

**Notes:** The graph illustrates the effect of the 1995 reform on the budget set of a surviving spouse without dependent children nor grandchildren in the $(z, c)$ plane, where $z$ indicates taxable income net of the survivor benefit and $c$ denotes disposable income. Specifically, $c = z + B(P, b(z)) - T(z + B(P, b(z)))$, where $B(\cdot)$ is the survivor benefit, which is a function of the pension of the deceased $P$ and of the replacement rate $b(z)$; $T(\cdot)$ is a tax function representing personal income taxes payable on taxable income including the survivor benefit $(z + B(\cdot))$. The dashed line represents the individual budget constraint under the old regime, while the solid line the individual budget constraint under the new regime. The vertical bars indicate the income brackets relevant to the determination of the benefit replacement rate in the new regime: $I_1$ is equivalent to three times the annual minimum pension, $I_2$ to four times the annual minimum pension and $I_3$ to five times the annual minimum pension. The thresholds $I' = (b^N_{j-1} - b^N_j)P + I_{j-1}\ \forall j = 1, 2, 3$ indicate the convex kinks in the budget constraint. The budget constraint is constructed using the mean value of $P$, the income thresholds and the personal income tax parameters in effect at the time of the 1995 reform.

FIGURE 1.3: EFFECT OF THE REFORM ON THE BENEFIT REPLACEMENT RATE



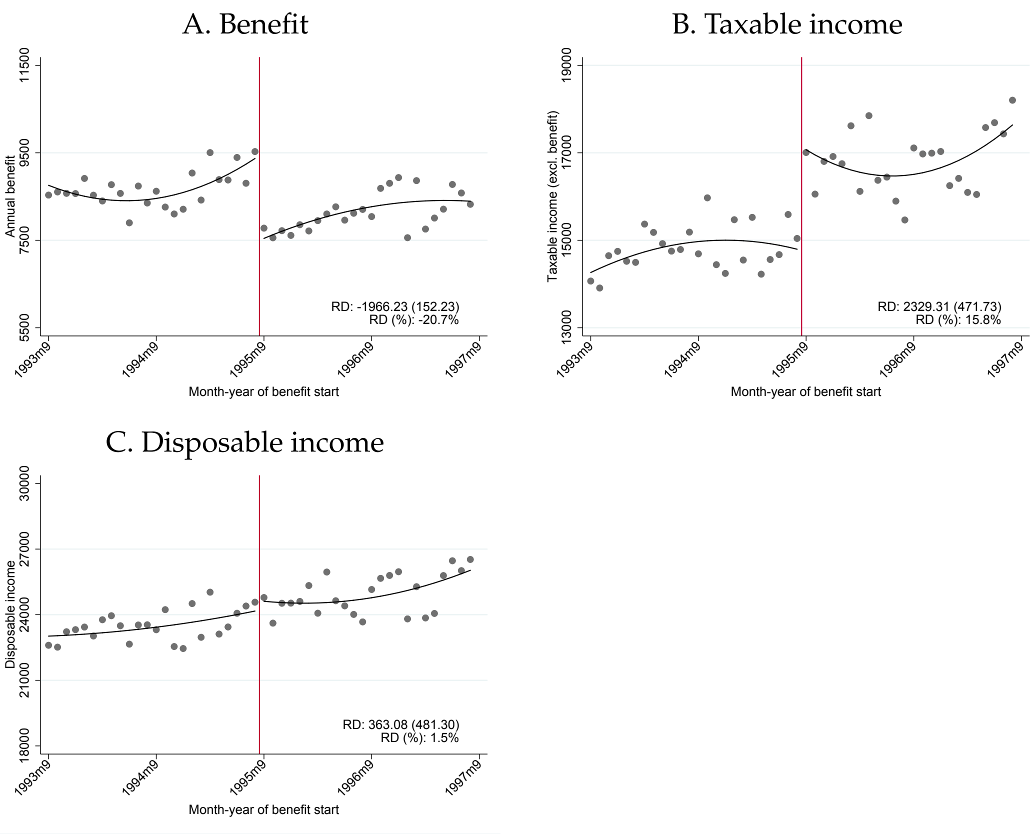

**Notes:** The graph shows the average replacement rate by month-of-benefit-start bin for surviving spouses with taxable income in the second or higher income brackets in the first year of benefit receipt. It also reports the coefficient $\eta_0$ and associated robust standard error from estimating equation 1.3, using the benefit replacement rate $b$ in $t = 0$ as outcome variable. The estimated $\eta_0$ is also reported as a percent of the mean outcome in the control group.

FIGURE 1.4: EFFECT OF THE REFORM ON THE EXPECTED LIFETIME BENEFIT



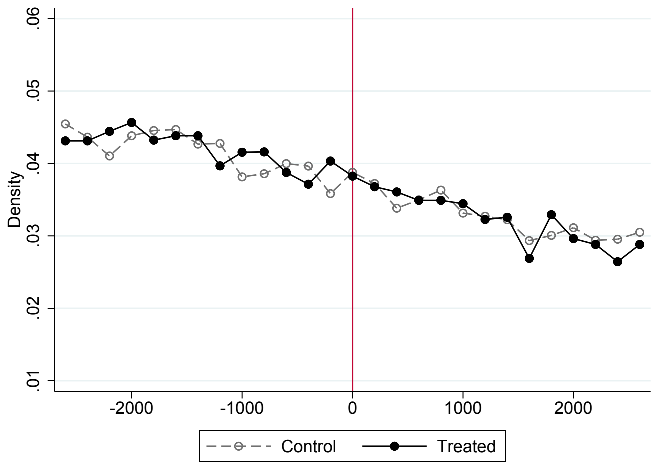

**Notes:** The graph shows the expected lifetime benefit by month-of-benefit-start bin for surviving spouses with taxable income in the second or higher income brackets in the first year of benefit receipt. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The graph also reports the coefficient $\beta_0$ and associated robust standard error from estimating equation 1.2, using the expected lifetime benefit as outcome variable. The estimated $\beta_0$ is also reported as a percent of the mean outcome in the control group.

FIGURE 1.5: EFFECT OF THE REFORM ON ANNUAL BENEFIT, TAXABLE INCOME AND DISPOSABLE INCOME



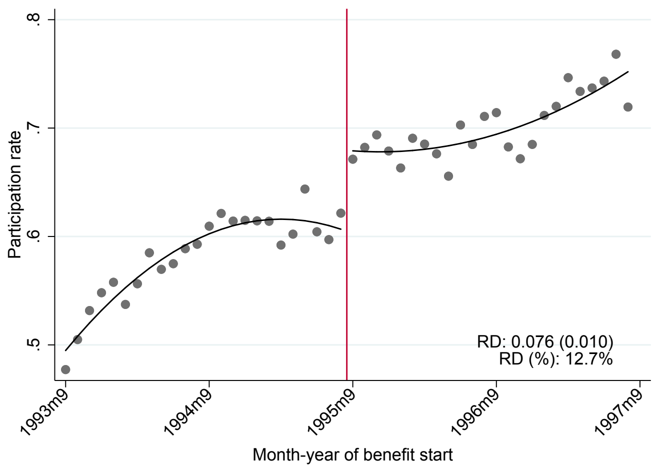

**Notes:** The graphs show the mean value of different outcome variables by month-of-benefit-start bin, pooling event-time years from $t = 0$ to $t = 15$. The solid dark lines display predicted values from the quadratic parametric regression in equation 1.3. Each graph also reports the coefficient $\eta_0$ and associated robust standard error from estimating equation 1.3, and the estimated $\eta_0$ as a percent of the mean outcome in the control group. Panel A refers to the annual survivor benefit $B$, Panel B to taxable income $z$ and Panel C to disposable income $z + B$.

FIGURE 1.6: EMPIRICAL DENSITY OF TAXABLE INCOME AROUND CONVEX KINKS BY TREATMENT STATUS



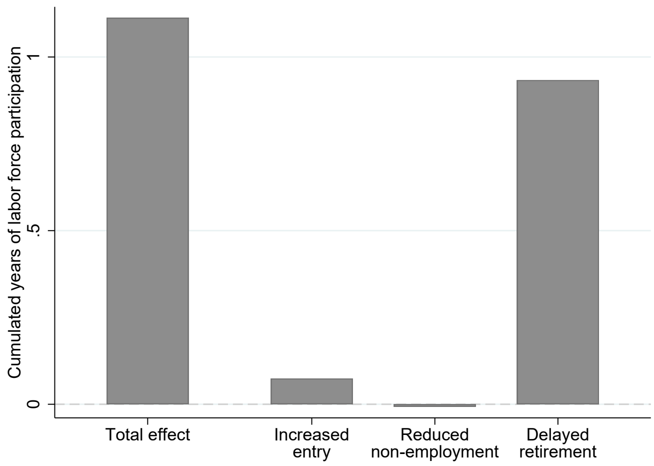

**Notes:** The graph plots the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$. The vertical bar represents the location of the convex kinks. Each dot refers to a €200 bin in the range $[-2,700;2,700]$ centered around the kink. Black circles represent observations in the treatment group and hollow circles to observations in the control group.

FIGURE 1.7: PARTICIPATION RESPONSE



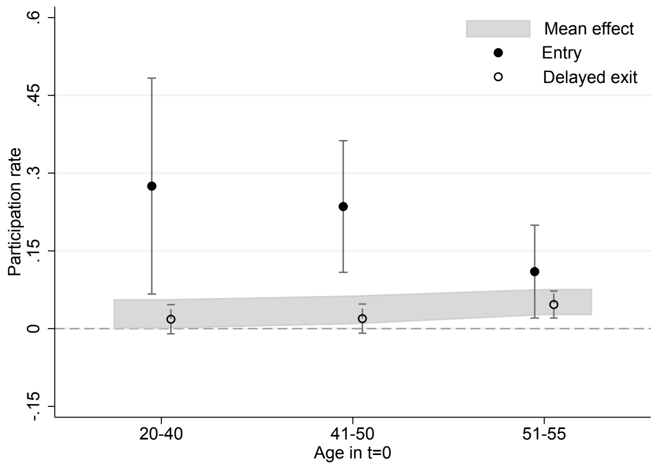

**Notes:** The graph shows the mean values of the participation rate in each month-of-benefit-start bin, pooling event-time years from $t = 0$ to $t = 15$. The solid dark lines display predicted values from the quadratic parametric regression in equation 1.3. The graph also reports the coefficient $\eta_0$ and associated robust standard error from estimating equation 1.3, and the estimated $\eta_0$ as a percent of the mean outcome in the control group.

FIGURE 1.8: DECOMPOSITION OF PARTICIPATION RESPONSE ALONG ENTRY AND EXIT MARGINS



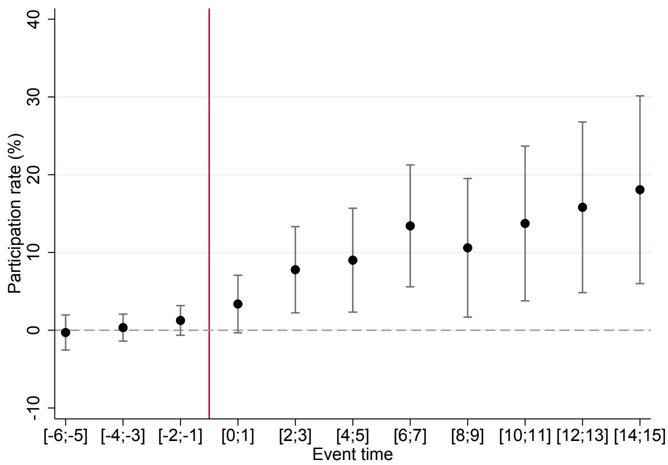

**Notes:** The graph shows a decomposition of the reduced-form effect on cumulated years experience between $t = 0$ and $t = 15$ along the entry and exit margin. Specifically, the first bar to the left reports reports the coefficient $\eta_0$ from estimating equation 1.3 using cumulated work experience in $t = 15$ as outcome variable. The remaining three bars decompose such effect into increased entry (second bar from the left) and delayed exit, distinguishing between delayed exit in the form of delayed non-employment (third bar from the left) and delayed retirement (fourth bar from the left). The second bar from the left reports the coefficient $\eta_0$ from estimating equation 1.3 using cumulated work experience in $t = 15$ as outcome variable for individuals who were not working in $t = -1$ and weighting the estimate by the share of such individuals in the sample. The third bar from the left reports the coefficient $\eta_0$ from estimating equation 1.3 using the (negative of the) number of non-employment years (excluding retirement) between $t = 0$ and $t = 15$ for individuals who were working in $t = -1$ and weighting the estimate by the share of such individuals in the sample. The fourth bar from the left is analogous to the third, but uses the number of years of retirement between $t = 0$ and $t = 15$ as outcome variable.

FIGURE 1.9: PROFILE OF THE PARTICIPATION RESPONSE BY AGE IN $t = 0$ AND EMPLOYMENT STATUS IN $t = -1$



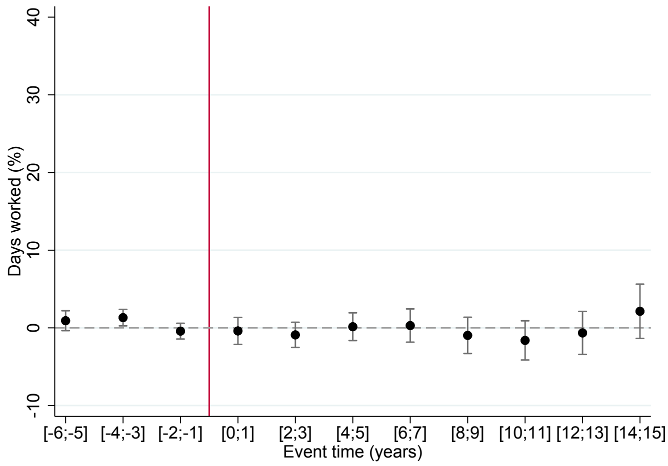

**Notes:** The graph outlines the profile of the labor force participation response by age in $t = 0$ and employment status in $t = -1$. The graph reports the estimated coefficient $\eta_0$ and associated 95 percent confidence interval from equation 1.3, using the participation rate as outcome variable and pooling event-time years from $t = 0$ to $t = 15$. The shaded area shows the 95 percent confidence interval of the coefficient $\eta_0$ for individuals in different age groups, irrespective of their employment status in $t = -1$. Black circles report the same coefficient for individuals who were not working in $t = -1$, while hollow circles for individuals who were working in $t = -1$. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The mean effect on participation (represented by the shaded area) is therefore a weighted average of an entry effect (represented by the black circles) and a delayed exit effect (represented by the hollow circles).

FIGURE 1.10: DYNAMIC OF THE PARTICIPATION RESPONSE



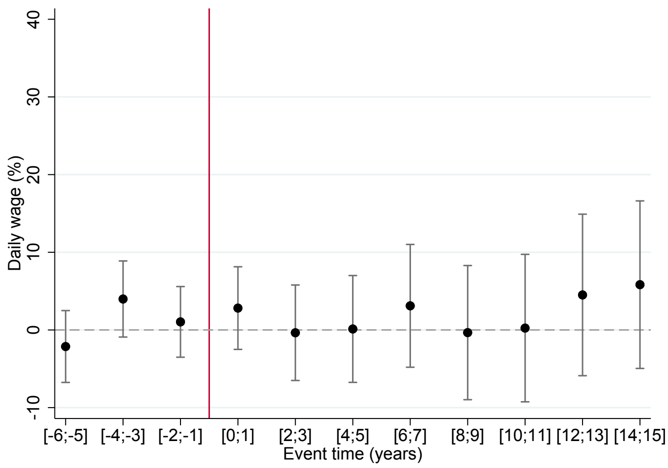

**Notes:** The graph reports the coefficient $\eta_0$ from estimating equation 1.3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated $\eta_0$ in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

FIGURE 1.11: DYNAMIC OF THE INTENSIVE MARGIN RESPONSE



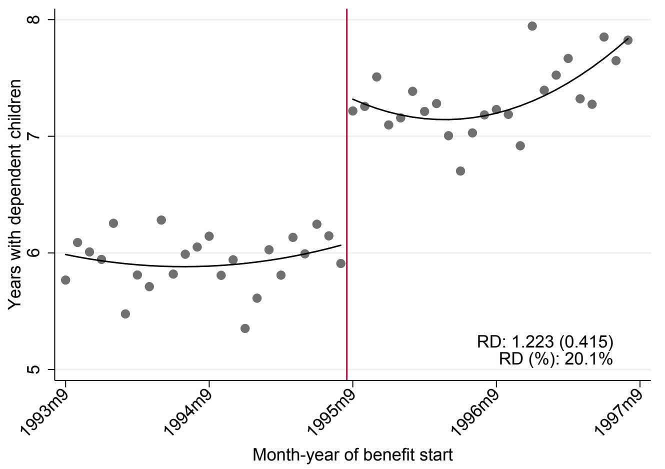

**Notes:** The graph reports the coefficient $\eta_0$ from estimating equation 1.3 using the number of days worked as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated $\eta_0$ in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The estimates are conditional on employment and on work experience in $t = 0$.

FIGURE 1.12: DYNAMIC OF THE WAGE RATE RESPONSE



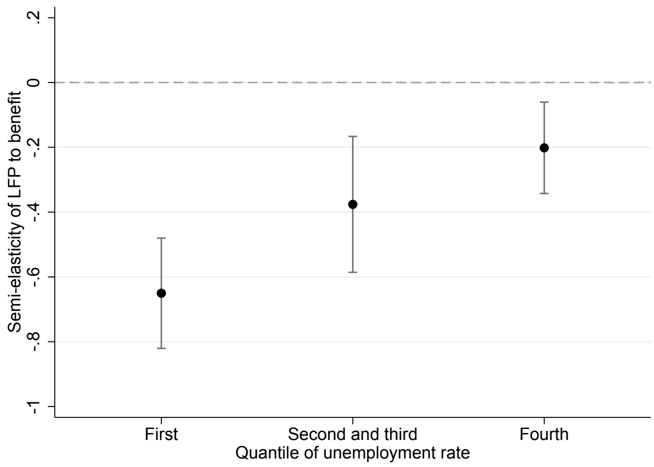

**Notes:** The graph reports the coefficient $\eta_0$ from estimating equation 1.3 using the daily wage rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated $\eta_0$ in percent of the mean in the control group for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors. The estimates are conditional on employment and on work experience in $t = 0$. The wage rate is computed as annual earnings divided by the number of days worked.

FIGURE 1.13: EFFECT OF THE REFORM ON THE NUMBER OF YEARS WITH DEPENDENT CHILDREN




**Notes:** The graph shows the mean values of the number of years with dependent children in each month-of-benefit-start bin. The solid dark lines display predicted values from the quadratic parametric regression in equation 1.3. The graph also reports the coefficient $\eta_0$ and associated robust standard error from estimating equation 1.3, and the estimated $\eta_0$ as a percent of the mean outcome in the control group.

FIGURE 1.14: HETEROGENEOUS TREATMENT EFFECTS BY REGIONAL UNEMPLOYMENT RATE IN $t$




**Notes:** The graph reports the coefficient $\beta$ from estimating equation 1.1 using an indicator for labor force participation as outcome and $logB_{it}$ as main regressor. Black circles indicate the estimated $\beta$ and the capped vertical bars report 95 percent confidence intervals based on robust standard errors. The graph shows heterogeneity in the semi-elasticity of labor force participation to the benefit by different quartiles of the distribution of the regional unemployment rate, in the region in which the surviving spouse resides. Data on the regional unemployment rate are at annual frequency and are taken from ISTAT. Individual-year observations are matched with the regional unemployment rate in the same year in the region where the individual resides. Individual observations are then binned into the quartiles of the distribution of the regional unemployment rate in each year. To improve the precision of the estimates, the second and third quartiles are combined. In order to control for the potential confounding role of the level of the black economy, I include the regional rate of undeclared work – as measured by estimated irregular full-time-equivalent employment over estimated total full-time-equivalent employment – at the annual level among the regression covariates. Data on the rate of undeclared work are taken from ISTAT.

## 1.9 Tables

TABLE 1.1: BENEFIT REPLACEMENT RATES

| | Benefit start date | |
|---|---|---|
| | Before Sept 1, 1995 | After Sept 1, 1995 |
| | (1) | (2) |
| *Spouse (with and without dependent children)* | | |
| Spouse only | | |
| Survivor's taxable income $\leq 3 \times$ minimum pension | 60% | 60% |
| Survivor's taxable income $\leq 4 \times$ minimum pension | 60% | 45% |
| Survivor's taxable income $\leq 5 \times$ minimum pension | 60% | 36% |
| Survivor's taxable income $> 5 \times$ minimum pension | 60% | 30% |
| Spouse with one dependent child or grandchild | 80% | 80% |
| Spouse with two or more dependent children or grandchildren | 100% | 100% |
| *Dependent children (absent the spouse)* | | |
| One dependent child or grandchild | 60% | 70% |
| Two dependent children or grandchildren | 80% | 80% |
| Three or more dependent children or grandchildren | 100% | 100% |
| *Dependent parents or siblings (absent the spouse, children or grandchildren)* | | |
| Each dependent relative | 15% | 15% |

**Notes:** The table reports the benefit replacement rates for different types of survivors and separately for benefits with start date before or after September 1995. Dependent children and grandchildren aged 18-21 who are high-school students and not working are entitled to the benefit up to age 21. University students up to age 26 are also entitled to the benefit, provided that they are not working. Children, grandchildren, parents or siblings that are disabled or incapacitated are also considered dependent. Each parent or sibling receives 15% of the pension of the deceased, up to 100%.

TABLE 1.2: EFFECT OF THE REFORM ON THE BENEFIT AMOUNT IN $t = 0$

| | Regression discontinuity | | | | Control mean |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Predicted second or higher income bracket* | | | | | |
| Benefit in $t = 0$ | -1510.21*** | -1684.83*** | -2137.66*** | -1963.66*** | 8494.83 |
| | (260.413) | (296.800) | (376.689) | (407.618) | |
| Lifetime benefit (000) | -67.032*** | -85.273*** | -99.641*** | -104.547*** | 337.387 |
| | (13.811) | (16.691) | (20.155) | (22.831) | |
| Obs. | 13556 | 13556 | 13556 | 13556 | - |
| *Full sample* | | | | | |
| Benefit in $t = 0$ | -465.171*** | -558.993*** | -593.922*** | -602.776*** | 8371.92 |
| | (73.548) | (85.830) | (109.989) | (120.938) | |
| Lifetime benefit (000) | -18.917*** | -24.567*** | -23.623*** | -25.120*** | 298.57 |
| | (3.243) | (3.916) | (4.879) | (5.511) | |
| Observations | 94578 | 94578 | 94578 | 94578 | - |
| Benefit-start-month FE | | x | | x | - |
| Calendar year FE | | x | | x | - |
| Linear trend | x | x | x | x | - |
| Quadratic trend | | | x | x | - |

**Notes:** The table reports the coefficient $\beta_0$ from estimating equation 1.2 using the benefit amount in $t = 0$ as outcome variable. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. The top panel reports estimates for the sample with predicted second or higher income bracket. The bottom panel reports estimates for the full sample. The lifetime benefit is computed by multiplying the annual benefit in $t = 0$ by life expectancy at time $t = 0$. Life expectancy tables are obtained from the Italian Statistical Institute (ISTAT) and are split by gender, age, calendar year and region of residence. The lifetime benefit is in thousands of euros.

TABLE 1.3: EFFECT OF THE REFORM ON BENEFIT, TAXABLE INCOME AND DISPOSABLE INCOME

| | Regression discontinuity | | | | Control mean |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Benefit | -1155.25*** | -1306.96*** | -1771.21*** | -1966.23*** | 9462.31 |
| | (103.033) | (110.320) | (145.140) | (152.225) | |
| Taxable income | 1674.92*** | 1473.23*** | 2508.59*** | 2329.31*** | 14470.64 |
| | (380.664) | (407.731) | (455.254) | (471.733) | |
| Disposable income | 519.674 | 166.277 | 737.385 | 363.081 | 23932.95 |
| | (386.337) | (414.151) | (464.363) | (481.298) | |
| Observations | 216896 | 216896 | 216896 | 216896 | - |
| Benefit-start-month FE | | x | | x | - |
| Calendar year FE | | x | | x | - |
| Linear trend | x | x | x | x | - |
| Quadratic trend | | | x | x | - |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth.

TABLE 1.4: IV ESTIMATE OF THE EFFECT OF THE BENEFIT ON TAXABLE INCOME AND DISPOSABLE INCOME

| | Taxable income | Disposable income | Taxable income | Disposable income |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Benefit | -1.205*** | -0.205 | -1.008*** | -0.008 |
| | (0.337) | (0.337) | (0.303) | (0.303) |
| Observations | 216896 | 216896 | 216896 | 216896 |
| Benefit-start-month FE | x | x | x | x |
| Calendar year FE | x | x | x | x |
| Linear trend | x | x | x | x |
| Quadratic trend | | | x | x |

**Notes:** The table reports the IV-RD coefficient $\beta$ from estimating equation 1.1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The IV estimates in columns (1) and (2) are based on a first stage with linear parametric specification, while those in columns (3) and (4) on a first stage with quadratic parametric specification with individual controls. All estimates are based on a 24-month symmetric bandwidth.

TABLE 1.5: EFFECT OF THE REFORM ON LABOR SUPPLY, RETIREMENT AND OTHER WORK-RELATED OUTCOMES

| | Regression discontinuity | | | | Control mean | Obs. |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Participation rate | 0.019*** | 0.024*** | 0.071*** | 0.076*** | 0.603 | 216896 |
| | (0.007) | (0.007) | (0.010) | (0.010) | | |
| Cumulated experience in $t = 15$ | 0.307 | 0.570* | 1.061** | 1.113** | 10.256 | 13556 |
| | (0.285) | (0.325) | (0.421) | (0.466) | | |
| Retirement rate in $t = 15$ | -0.012 | -0.041 | -0.072* | -0.079* | 0.516 | 13556 |
| | (0.025) | (0.029) | (0.037) | (0.041) | | |
| Days worked | 0.147 | -0.248 | -1.169 | -1.561 | 351.482 | 123829 |
| | (0.995) | (1.033) | (1.491) | (1.521) | | |
| Daily wage | 0.382 | -0.427 | 0.998 | -0.047 | 76.755 | 123829 |
| | (0.818) | (0.860) | (1.217) | (1.251) | | |
| Full-time job | -0.020** | -0.027*** | -0.004 | -0.013 | 0.891 | 68253 |
| | (0.005) | (0.005) | (0.007) | (0.008) | | |
| Change firm | -0.008 | -0.012 | 0.005 | 0.003 | 0.082 | 68253 |
| | (0.007) | (0.008) | (0.010) | (0.011) | | |
| Change industry | 0.001 | 0.001 | 0.011* | 0.012* | 0.029 | 68253 |
| | (0.004) | (0.005) | (0.007) | (0.007) | | |
| Change province | -0.008* | -0.011*** | -0.005 | -0.008 | 0.025 | 68253 |
| | (0.004) | (0.004) | (0.006) | (0.006) | | |
| Benefit-start-month FE | | x | | x | - | - |
| Calendar year FE | | x | | x | - | - |
| Linear trend | x | x | x | x | - | - |
| Quadratic trend | | | x | x | - | - |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group and column (6) the number of observations. All estimates are based on a 24-month symmetric bandwidth. Cumulated experience and the retirement rate are measured at event-time $t = 15$. The wage rate is computed as annual earnings divided by the number of days worked. The probability of holding a full-time job, changing firm, changing industry (at three-digit level) and changing province are estimated on the sample of individuals employed in the private sector. The estimates for days worked, the wage rate, the probability of holding a full-time job, changing firm, changing industry and changing province are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls.

TABLE 1.6: IV ESTIMATES OF THE EFFECT OF THE BENEFIT ON LABOR SUPPLY, PROGRAM SUBSTITUTION AND DEPENDENCY PERIOD

| | Benefit (000) (1) | ln Benefit (2) | Control mean | Observations |
|---|---|---|---|---|
| Participation rate | -0.040*** | -0.286*** | 0.603 | 216896 |
| | (0.006) | (0.043) | | |
| Cumulated experience in $t = 15$ | -0.842** | | 10.256 | 13556 |
| | (0.414) | | | |
| Retirement rate in $t = 15$ | 0.100** | | 0.516 | 13556 |
| | (0.047) | | | |
| Days worked | 2.563*** | | 351.482 | 123829 |
| | (0.731) | | | |
| Daily wage | 2.210*** | | 76.755 | 123829 |
| | (0.618) | | | |
| Full-time job | 0.010* | | 0.891 | 68253 |
| | (0.005) | | | |
| Change firm | -0.004 | | 0.082 | 68253 |
| | (0.005) | | | |
| Change industry | -0.002 | | 0.029 | 68253 |
| | (0.003) | | | |
| Change province | -0.000 | | 0.025 | 68253 |
| | (0.003) | | | |
| Paid family leave | -0.003** | | 0.008 | 117264 |
| | (0.002) | | | |
| Paid sick leave | -0.003 | | 0.043 | 117264 |
| | (0.003) | | | |
| Unemployment benefits | -0.017*** | | 0.017 | 117264 |
| | (0.003) | | | |
| Dependency period | -0.653** | | 6.095 | 5595 |
| | (0.272) | | | |
| Benefit-start-month FE | x | x | - | - |
| Calendar year FE | x | x | - | - |
| Linear trend | x | x | - | - |
| Quadratic trend | x | x | - | - |

**Notes:** The table reports the IV-RD coefficient $\beta$ from estimating equation 1.1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Estimates of the first stage are based on a quadratic parametric specification with individual controls and a 24-month symmetric bandwidth. Cumulated experience and the retirement rate are measured at event-time $t = 15$. The wage rate is computed as annual earnings divided by the number of days worked. The probability of holding a full-time job, changing firm, changing industry (at three-digit level) and changing province are estimated on the sample of individuals employed in the private sector. The estimates for days worked, the wage rate, the probability of holding a full-time job, changing firm, changing industry and changing province are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls. Estimates for paid family leave, paid sick leave and unemployment benefits are conditional on employment in $t$ or $t - 1$, and include the number of years of work experience in $t = 0$ among the individual controls. The benefit amount is in thousands of euros.

TABLE 1.7: EFFECT OF THE REFORM ON SOCIAL INSURANCE TAKE-UP

| | Regression discontinuity | | | | Control mean |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Paid family leave | 0.004* | 0.005* | 0.011*** | 0.012*** | 0.008 |
| | (0.002) | (0.002) | (0.003) | (0.004) | |
| Paid sick leave | 0.019*** | 0.016*** | 0.020*** | 0.016** | 0.043 |
| | (0.005) | (0.005) | (0.007) | (0.007) | |
| Unemployment benefits | -0.004 | 0.002 | 0.007 | 0.013*** | 0.017 |
| | (0.003) | (0.003) | (0.004) | (0.005) | |
| Observations | 117264 | 117264 | 117264 | 117264 | - |
| Benefit-start-month FE | | x | | x | - |
| Calendar year FE | | x | | x | - |
| Linear trend | x | x | x | x | - |
| Quadratic trend | | | x | x | - |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. All estimates are conditional on employment in $t$ or $t-1$, and include the number of years of work experience in $t = 0$ among the individual controls.

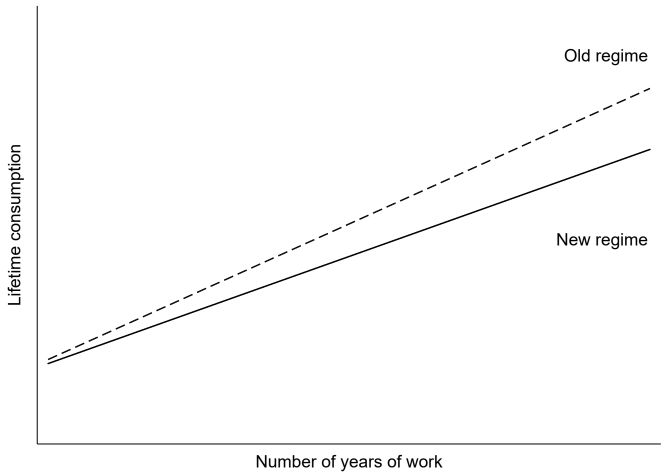
TABLE 1.8: EFFECT OF THE REFORM ON THE BENEFIT UPON LOSS OF DEPENDENCY AND ON THE DEPENDENCY PERIOD

| | Regression discontinuity | | | | Control mean |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Benefit upon dep. loss | -1242.33*** | -1186.79*** | -1508.89*** | -1305.21*** | 7875.19 |
| | (261.391) | (287.243) | (388.340) | (408.394) | |
| Dependency period | 0.737*** | 1.255*** | 1.220*** | 1.223*** | 6.095 |
| | (0.222) | (0.290) | (0.335) | (0.415) | |
| Observations | 5595 | 5595 | 5595 | 5595 | - |
| Benefit-start-month FE | | x | | x | - |
| Calendar year FE | | x | | x | - |
| Linear trend | x | x | x | x | - |
| Quadratic trend | | | x | x | - |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 using different outcome variables. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. The benefit is measured in the year after all children have lost their dependency status. The dependency period is measured as the number of years with dependent children within the household.

## 1.A Appendix Figures

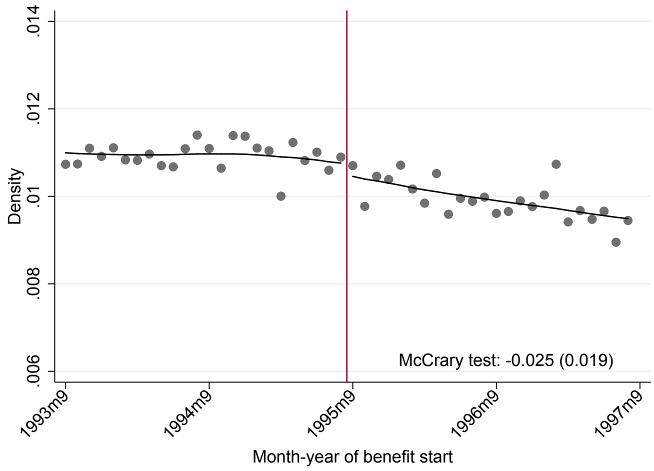
FIGURE 1.A1: DYNAMIC FRAMEWORK



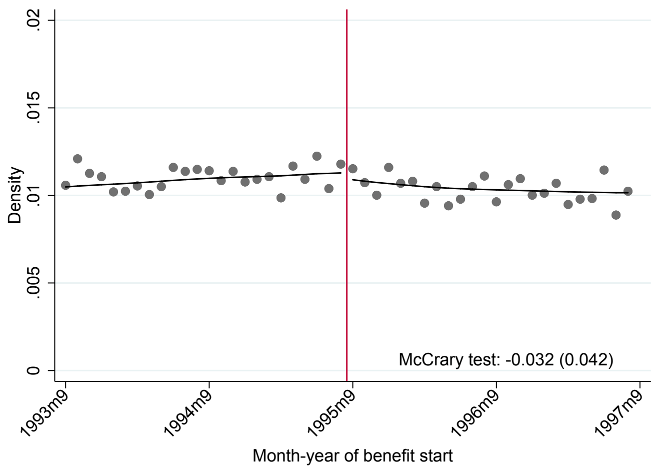

**Notes:** The graph illustrates the effect of the 1995 reform on the dynamic budget set of a surviving spouse without dependent children nor grandchildren. The x-axis reports the number of years of work out of the total number of available years. The y-axis reports the level of lifetime consumption associated to each number of years of work. Without loss of generality, the graph is constructed under the assumption that, when working, individuals earn a fixed annual wage and receive 30 percent of their spouses's pension as survivor benefit; when not working, individuals do not earn any wage and receive 60 percent of their spouse's pension as survivor benefit. The dashed line represents the individual lifetime budget constraint under the old regime, while the solid line the individual lifetime budget constraint under the new regime.

FIGURE 1.A2: DISTRIBUTION OF BENEFITS BY START DATE AND MCCRARY TESTS

A. Full sample

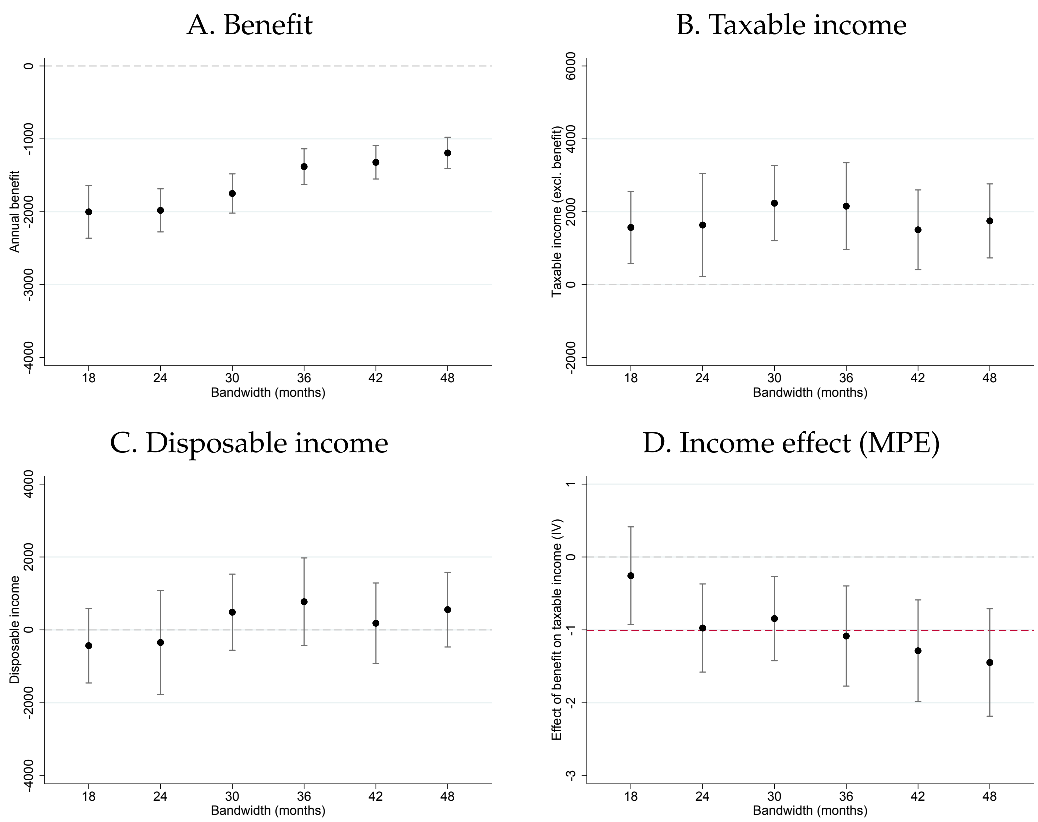



B. Individuals with taxable income in second or higher income bracket



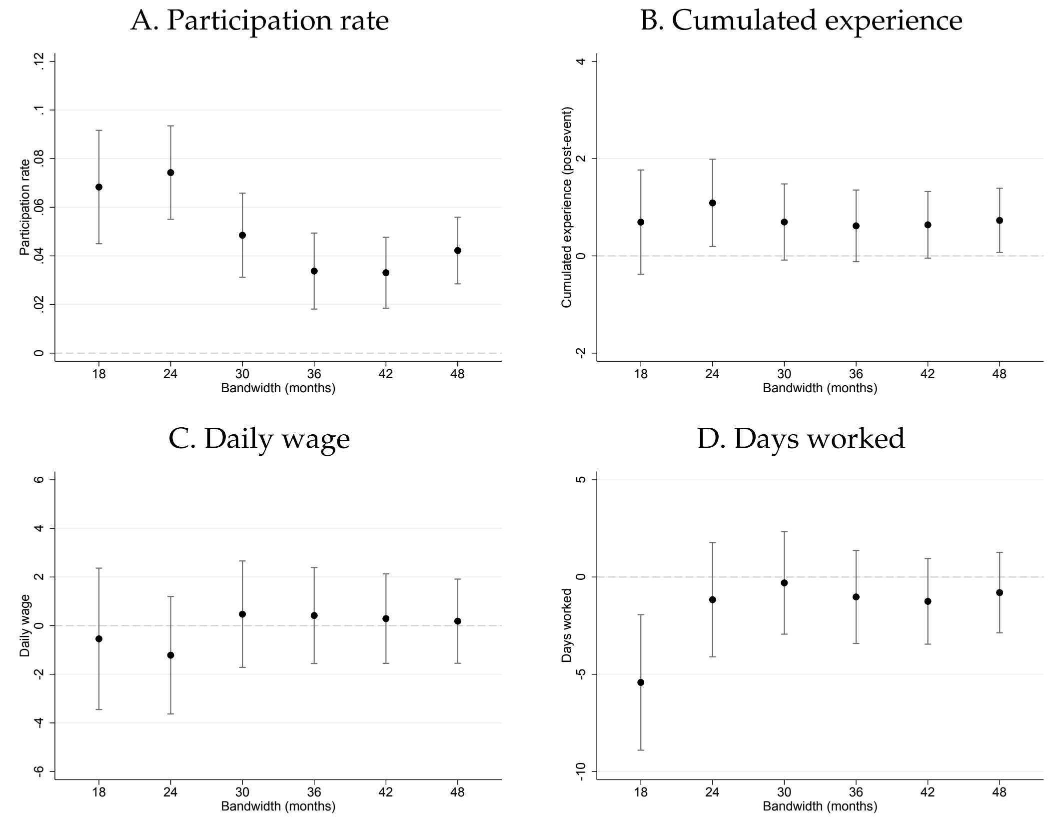

**Notes:** The graphs plot the empirical probability density function of benefit recipients by month-year of benefit start for the entire sample (Panel A) and for the subgroup of individuals with taxable income in the second or higher income bracket in $t = 0$ (Panel B). Each graph reports the test statistics and associated standard error in parenthesis of a McCrary test of the discontinuity in the probability density function of the running variable at the September 1995 threshold.

FIGURE 1.A3: RD COEFFICIENTS AND CONFIDENCE INTERVALS BY BANDWIDTH



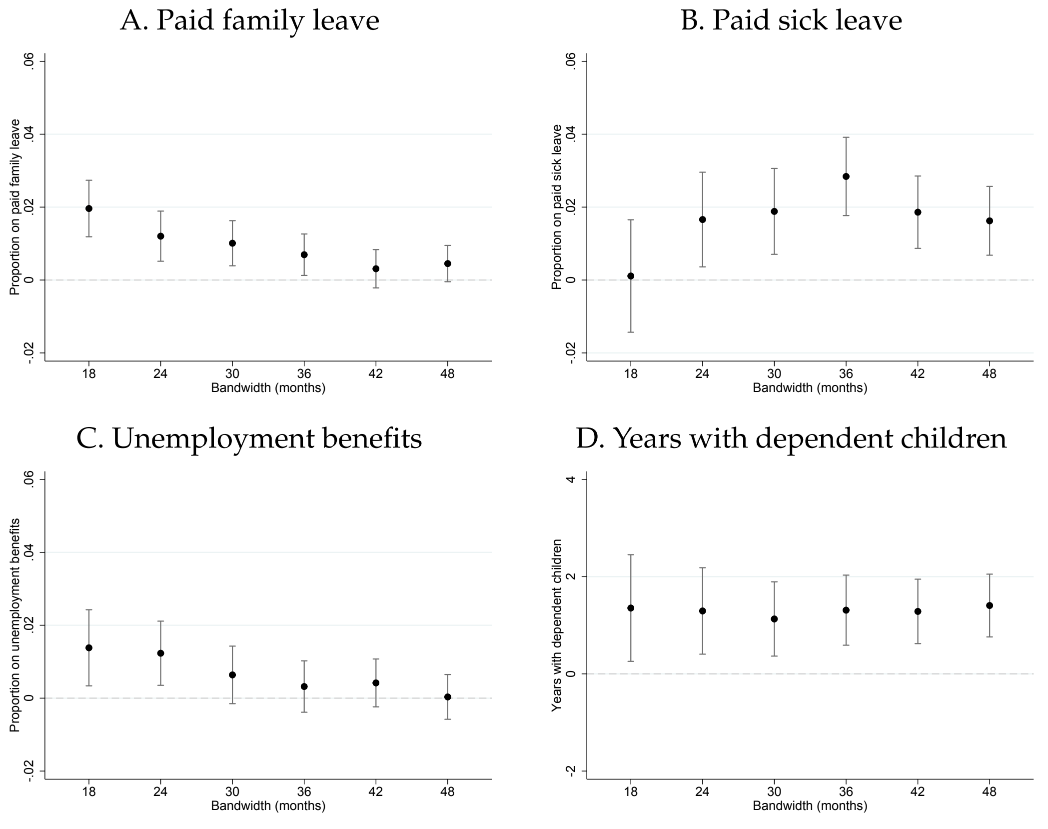

**Notes:** Panels A, B and C report the coefficient $\eta_0$ from estimating equation 1.3 using a quadratic parametric specification and different bandwidths. Panel D reports the coefficient $\beta$ from an IV estimation of equation 1.1 using a quadratic parametric specification for the first stage and different bandwidths. Solid circles indicate the estimated coefficients. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

FIGURE 1.A4: RD COEFFICIENTS AND CONFIDENCE INTERVALS BY BANDWIDTH



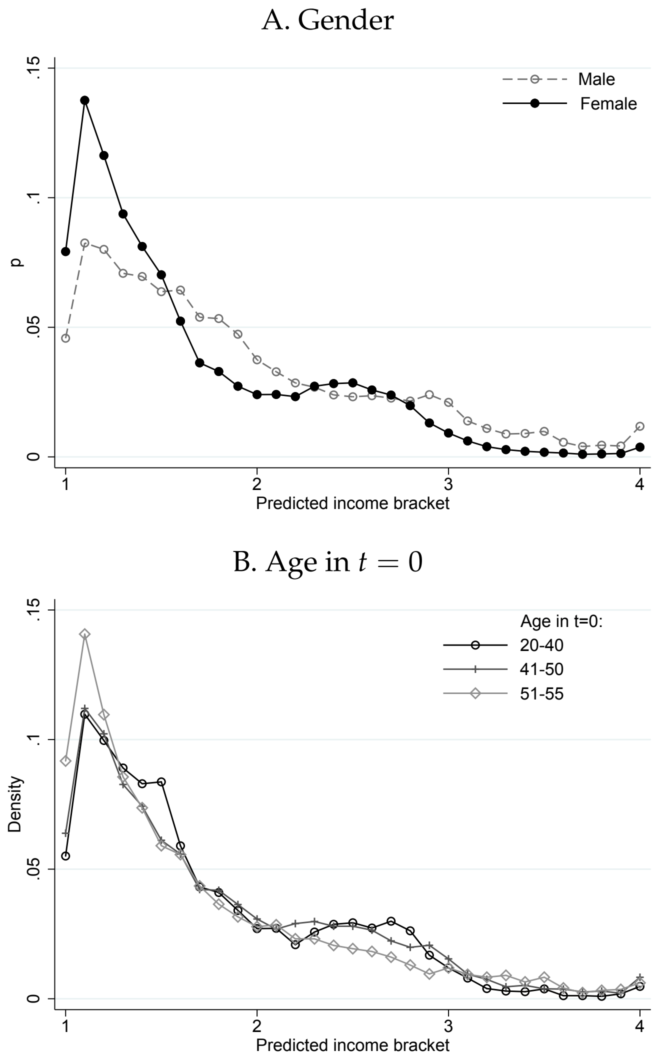

**Notes:** The graphs report the coefficient $\eta_0$ from estimating equation 1.3 using a quadratic parametric specification and different non-parametric local linear regression. Solid circles indicate the estimated $\eta_0$ for specifications with different symmetric bandwidths. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

FIGURE 1.A5: RD COEFFICIENTS AND CONFIDENCE INTERVALS BY BANDWIDTH



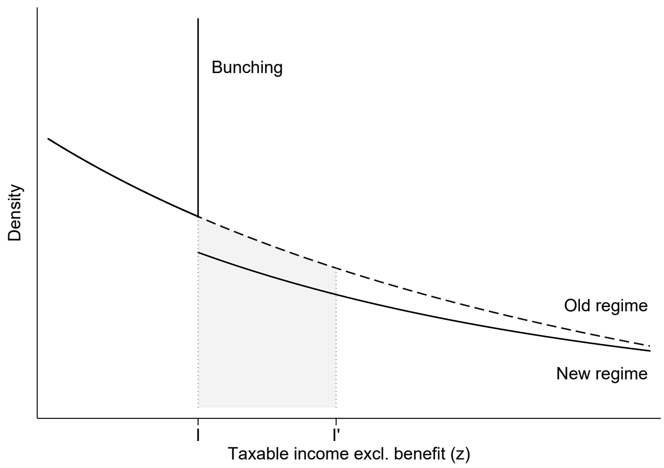

**Notes:** The graphs report the coefficient $\eta_0$ from estimating equation 1.3 using a quadratic parametric specification and different non-parametric local linear regression. Solid circles indicate the estimated $\eta_0$ for specifications with different symmetric bandwidths. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

FIGURE 1.A6: EMPIRICAL DENSITY OF PREDICTED TAXABLE INCOME BRACKET BY GENDER AND AGE IN $t = 0$



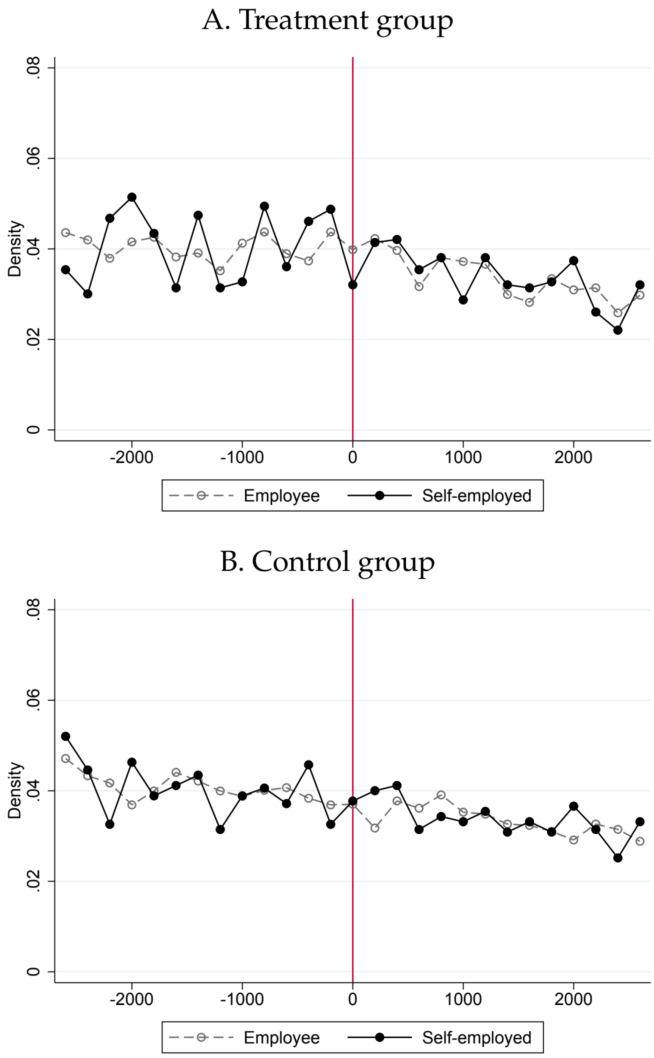

**Notes:** The graphs report the empirical distribution of predicted taxable income bracket by gender (Panel A) and age at event time $t = 0$ (Panel B). The graph reports the distribution for individuals with predicted taxable income in the second or higher income bracket. Each dot refers to a 0.1 bin.

FIGURE 1.A7: DENSITY OF TAXABLE INCOME



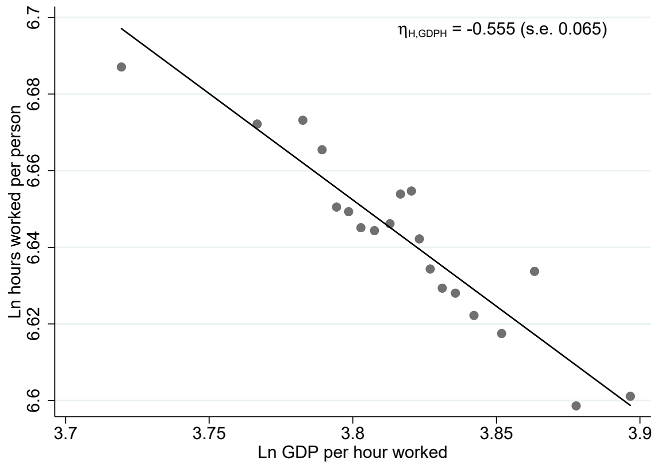

**Notes:** The graph plots a theoretical density function of taxable income $z$. The dashed line illustrates the case of a smooth density function. By introducing a discrete change in the marginal tax rate, the reform creates a convex kink in the budget constraint of treated individuals at $z = I$. Absent the kink, individuals would locate smoothly along the old-regime budget set generating a smooth taxable income density. Once introduced, the convex kink creates a disincentive for individuals to locate in the range $[I, I']$ and induces individuals who would counterfactually locate in that range to bunch at $I$. This behavior will give rise to excess bunching in the taxable income density function at the kink point and a left-shift in the density above the kink, as illustrated by the solid line and the shadowed region in the graph.

FIGURE 1.A8: EMPIRICAL DENSITY OF TAXABLE INCOME AROUND CONVEX KINKS BY TREATMENT STATUS AND EMPLOYMENT STATUS



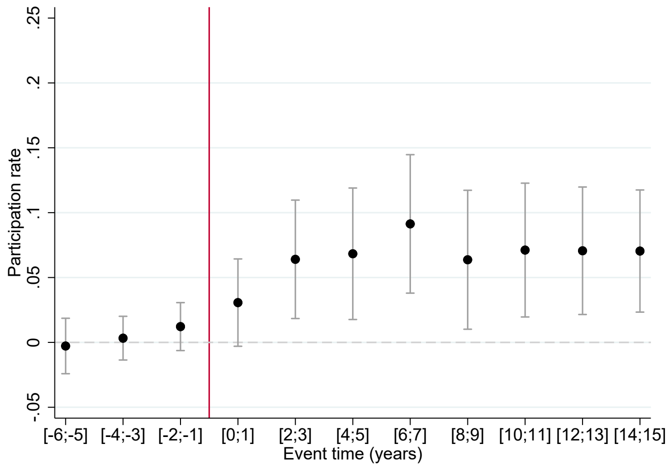

**Notes:** The graphs plot the empirical distribution of taxable income pooling observations around the three convex kinks created by the reform and pooling all years from $t = 0$ to $t = 15$. The vertical bar represents the location of the convex kinks. Each dot refers to a €200 bin in the range $[-2,700; 2,700]$ centered around the kink. Black circles represent self-employed individuals, while hollow circles represent wage earners. Panel A is based on observations in the treatment group and Panel B to observations in the control group.

FIGURE 1.A9: MACRO-ELASTICITY OF HOURS WORKED PER PERSON TO GDP PER HOUR



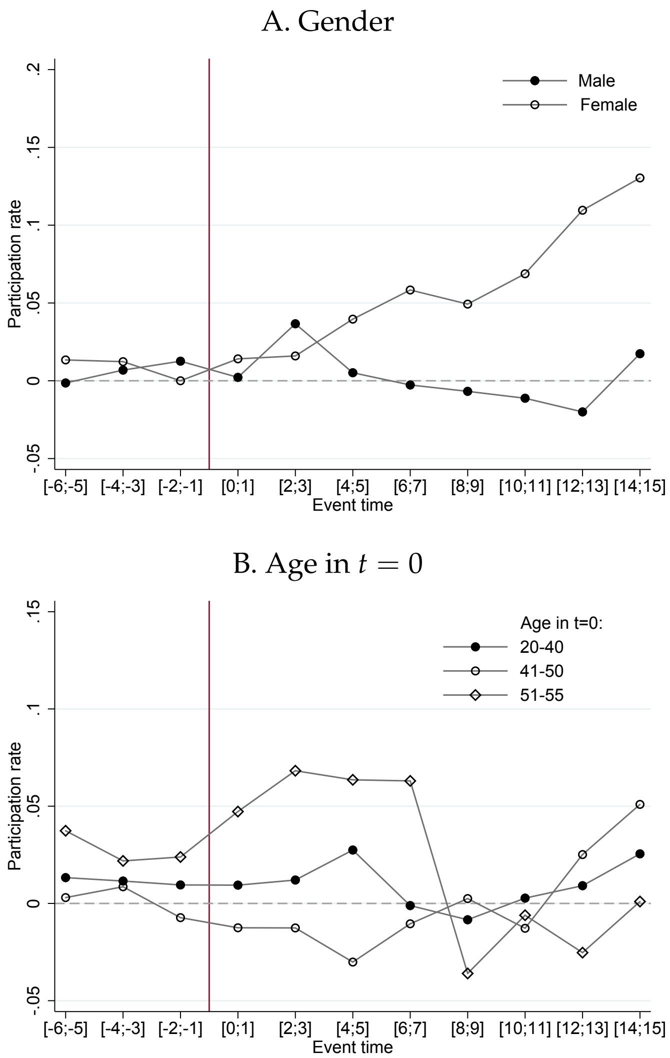

**Notes:** The graph reports a binned scatter plot of the logarithm of hours of work per person on the logarithm of GDP per hour, controlling for country and calendar year fixed effects. The plot is based OECD data at the country-year level from 1985 to 2015 for the following countries: Australia, Belgium, Canada, Germany, Denmark, Finland, France, the United Kingdom, Italy, Japan, the Netherlands and the United States. The graph also reports the estimated elasticity of hours worked to GDP per hour and its associated robust standard error. Gray circles represent binned observations and the black line the regression fitted line.

FIGURE 1.A10: DYNAMIC OF THE PARTICIPATION RESPONSE (LEVEL EFFECT)



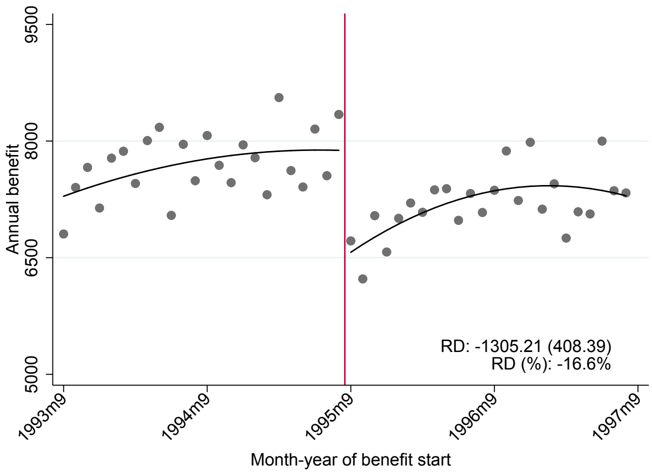

**Notes:** The graph reports the coefficient $\eta_0$ from estimating equation 1.3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Black circles indicate the estimated $\eta_0$ for different event-time years. The capped vertical bars report 95 percent confidence intervals based on robust standard errors.

FIGURE 1.A11: HETEROGENEOUS DYNAMIC EFFECTS OF LABOR FORCE PARTICIPATION BY GENDER AND AGE IN $t = 0$




**Notes:** The graphs report the coefficient $\eta_0$ from estimating equation 1.3 using the participation rate as outcome variable and pooling event-time years from $t = -6$ to $t = 15$ into biennia. Markers indicate the estimated $\eta_0$ for each event-time year. Panel A shows heterogeneity in the dynamic of the participation response by gender. Panel B shows heterogeneity in the dynamic of the participation response by age in $t = 0$.

FIGURE 1.A12: EFFECT OF THE REFORM ON THE BENEFIT UPON LOSS OF DEPENDENCY STATUS




**Notes:** The graph shows the mean value of the annual benefit by month-of-benefit-start bin. The benefit is measured in the year after all children have lost their dependency status. The solid dark lines display predicted values from the quadratic parametric regression in equation 1.3. The graph also reports the coefficient $\eta_0$ and associated robust standard error from estimating equation 1.3, and the estimated $\eta_0$ as a percent of the mean outcome in the control group. The estimates are based on the sample of individuals with dependent children in $t = 0$ and with predicted taxable income in the second or higher income brackets.

## 1.B Appendix Tables

TABLE 1.B1: ANNUAL MINIMUM PENSION (IN EURO)

| Year | Amount | × 3 | × 4 | × 5 |
|---|---|---|---|---|
| 1990 | 3,142.98 | 9,428.93 | 12,571.90 | 15,714.88 |
| 1991 | 3,354.98 | 10,064.94 | 13,419.93 | 16,774.91 |
| 1992 | 3,696.41 | 11,089.23 | 14,785.64 | 18,482.06 |
| 1993 | 3,825.68 | 11,477.04 | 15,302.72 | 19,128.40 |
| 1994 | 4,006.80 | 12,020.41 | 16,027.21 | 20,034.01 |
| 1995 | 4,205.95 | 12,617.84 | 16,823.79 | 21,029.74 |
| 1996 | 4,433.21 | 13,299.64 | 17,732.86 | 22,166.07 |
| 1997 | 4,606.10 | 13,818.29 | 18,424.39 | 23,030.49 |
| 1998 | 4,684.32 | 14,052.95 | 18,737.26 | 23,421.58 |
| 1999 | 4,768.58 | 14,305.73 | 19,074.30 | 23,842.88 |
| 2000 | 4,844.78 | 14,534.34 | 19,379.12 | 24,223.89 |
| 2001 | 4,970.67 | 14,912.00 | 19,882.66 | 24,853.33 |
| 2002 | 5,104.97 | 15,314.91 | 20,419.88 | 25,524.85 |
| 2003 | 5,227.56 | 15,682.68 | 20,910.24 | 26,137.80 |
| 2004 | 5,358.34 | 16,075.02 | 21,433.36 | 26,791.70 |
| 2005 | 5,465.59 | 16,396.77 | 21,862.36 | 27,327.95 |
| 2006 | 5,558.54 | 16,675.62 | 22,234.16 | 27,792.70 |
| 2007 | 5,669.82 | 17,009.46 | 22,679.28 | 28,349.10 |
| 2008 | 5,760.56 | 17,281.68 | 23,042.24 | 28,802.80 |
| 2009 | 5,950.88 | 17,852.64 | 23,803.52 | 29,754.40 |
| 2010 | 5,992.61 | 17,977.83 | 23,970.44 | 29,963.05 |
| 2011 | 6,076.59 | 18,229.77 | 24,306.36 | 30,382.95 |
| 2012 | 6,246.89 | 18,740.67 | 24,987.56 | 31,234.45 |
| 2013 | 6,440.59 | 19,321.77 | 25,762.36 | 32,202.95 |
| 2014 | 6,517.94 | 19,553.82 | 26,071.76 | 32,589.70 |
| 2015 | 6,524.57 | 19,573.71 | 26,098.28 | 32,622.85 |
| 2016 | 6,524.57 | 19,573.71 | 26,098.28 | 32,622.85 |
| 2017 | 6,524.57 | 19,573.71 | 26,098.28 | 32,622.85 |

**Notes:** The table reports the nominal value of the minimum pension and of its multiples for the years from 1990 to 2017. The minimum pension is a minimum amount provided by the social security to pensioners whose pension benefit is below a subsistence income threshold. The minimum pension level is set by law each year.

TABLE 1.B2: SUMMARY STATISTICS FOR THE FULL SAMPLE OF SURVIVING SPOUSES

| | Full sample | | Treatment group | | Control group | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Female | 0.90 | 0.30 | 0.91 | 0.29 | 0.90 | 0.30 |
| Age in $t = 0$ | 46.85 | 7.19 | 46.88 | 7.11 | 46.83 | 7.26 |
| Prop. aged $< 40$ in $t = 0$ | 0.16 | 0.37 | 0.16 | 0.37 | 0.16 | 0.37 |
| Prop. aged 40-50 in $t = 0$ | 0.45 | 0.50 | 0.46 | 0.50 | 0.44 | 0.50 |
| Prop. aged 51-55 in $t = 0$ | 0.39 | 0.49 | 0.38 | 0.49 | 0.40 | 0.49 |
| Prop. with dependent children in $t = 0$ | 0.45 | 0.50 | 0.44 | 0.50 | 0.46 | 0.50 |
| Age of dependent children in $t = 0$ | 13.16 | 5.14 | 13.22 | 5.15 | 13.10 | 5.14 |
| Prop. in first bracket in $t = 0$ | 0.86 | 0.34 | 0.85 | 0.36 | 0.88 | 0.33 |
| Prop. in second bracket in $t = 0$ | 0.07 | 0.25 | 0.08 | 0.27 | 0.06 | 0.23 |
| Prop. in third bracket in $t = 0$ | 0.03 | 0.18 | 0.04 | 0.19 | 0.03 | 0.16 |
| Prop. in fourth bracket in $t = 0$ | 0.04 | 0.19 | 0.04 | 0.20 | 0.04 | 0.19 |
| Prop. ever employed in $t \leq -1$ | 0.81 | 0.39 | 0.81 | 0.39 | 0.81 | 0.40 |
| Years of experience in $t = -1$ | 14.25 | 10.12 | 14.31 | 10.18 | 14.20 | 10.06 |
| Prop. employed in $t = -1$ | 0.40 | 0.49 | 0.40 | 0.49 | 0.40 | 0.49 |
| Prop. employed in private sector in $t = -1$ | 0.60 | 0.49 | 0.60 | 0.49 | 0.60 | 0.49 |
| Prop. employed in public sector in $t = -1$ | 0.06 | 0.24 | 0.06 | 0.25 | 0.06 | 0.23 |
| Prop. employed in para-public sector in $t = -1$ | 0.02 | 0.15 | 0.02 | 0.15 | 0.02 | 0.15 |
| Prop. self-employed in $t = -1$ | 0.31 | 0.46 | 0.30 | 0.46 | 0.32 | 0.47 |
| Prop. in professional occupation in $t = -1$ | 0.00 | 0.06 | 0.00 | 0.07 | 0.00 | 0.06 |
| Labor income in $t = -1$ | 6237.35 | 10761.88 | 6205.20 | 10745.04 | 6266.57 | 10777.19 |
| Daily wage in $t = -1$ | 47.27 | 67.47 | 47.10 | 43.00 | 47.43 | 83.50 |
| Days worked in $t = -1$ | 327.30 | 88.52 | 325.94 | 90.26 | 328.53 | 86.90 |
| Benefit in $t = 0$ | 9691.92 | 7597.10 | 9712.08 | 7358.12 | 9673.44 | 7333.20 |
| Income of deceased in $t = 0$ | 16256.70 | 14759.14 | 17322.87 | 14849.46 | 15043.89 | 14561.44 |
| Pension of deceased in $t = 0$ | 12668.17 | 10407.14 | 13233.03 | 10223.97 | 12148.19 | 10546.34 |
| Observations | 94578 | | 45022 | | 49556 | |

**Notes:** The table reports summary statistics for the full balanced sample of surviving spouses. The statistics are computed on the sample of survivors whose benefit start date is within a 24-month symmetric bandwidth around September 1, 1995. Monetary quantities are expressed in 2010 prices. Labor income is unconditional on employment. Days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

TABLE 1.B3: COVARIATE BALANCING TESTS

| | Regression discontinuity | | | | | Control |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | mean |
| Female | 0.003 | 0.005 | 0.006 | -0.001 | 0.003 | 0.899 |
| | (0.004) | (0.005) | (0.006) | (0.007) | (0.007) | |
| Age in $t = 0$ | 0.070 | -0.097 | -0.160 | -0.216 | -0.075 | 46.860 |
| | (0.094) | (0.124) | (0.143) | (0.179) | (0.120) | |
| Experience in $t = -1$ | -0.001 | -0.006 | -0.418* | -0.188 | -0.289 | 14.445 |
| | (0.143) | (0.189) | (0.216) | (0.269) | (0.123) | |
| Earnings in $t = -1$ | -269.993* | -170.699 | -140.946 | -105.928 | -111.621 | 6373.42 |
| | (139.426) | (185.312) | (211.708) | (265.219) | (224.762) | |
| Prop. employed in $t = -1$ | 0.002 | -0.005 | -0.002 | -0.005 | 0.004 | 0.397 |
| | (0.006) | (0.008) | (0.010) | (0.012) | (0.011) | |
| Days worked in $t = -1$ | -0.999 | -1.181 | -3.594* | -0.214 | 0.165 | 341.026 |
| | (1.358) | (1.797) | (2.007) | (2.539) | (3.165) | |
| Daily wage in $t = -1$ | -1.282 | 1.675 | 1.189 | -0.953 | -3.376 | 47.544 |
| | (1.020) | (1.721) | (1.353) | (2.469) | (2.204) | |
| Prop. on defined benefit | -0.005 | -0.013 | -0.006 | -0.011 | -0.003 | 0.312 |
| | (0.007) | (0.009) | (0.010) | (0.012) | (0.009) | |
| Observations | 94578 | 94578 | 94578 | 94578 | 94578 | - |
| Month-of-benefit-start FE | | x | | x | | - |
| Calendar year FE | | x | | x | | - |
| Linear trend | x | x | x | x | | - |
| Quadratic trend | | | x | x | | - |
| LLR | | | | | x | - |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 for different outcome variables. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Columns (1) and (2) are based on a linear parametric specification, without and with controls respectively. Columns (3) and (4) are based on a quadratic parametric specification, without and with controls respectively. Column (5) is based on non-parametric local linear regression. Column (6) reports the mean of the outcome variable in the control group. All estimates are based on a 24-month symmetric bandwidth. Earnings are measured unconditional on employment. The number of days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

TABLE 1.B4: SUMMARY STATISTICS FOR THE SAMPLE OF SURVIVING SPOUSES WITH PREDICTED TAXABLE INCOME IN THE SECOND OR HIGHER INCOME BRACKET

| | Full sample | | Treatment group | | Control group | |
|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD |
| Female | 0.64 | 0.48 | 0.66 | 0.48 | 0.62 | 0.48 |
| Age in $t = 0$ | 43.50 | 7.49 | 43.56 | 7.31 | 43.45 | 7.65 |
| Prop. aged < 40 in $t = 0$ | 0.29 | 0.45 | 0.28 | 0.45 | 0.30 | 0.46 |
| Prop. aged 40-50 in $t = 0$ | 0.51 | 0.50 | 0.53 | 0.50 | 0.49 | 0.50 |
| Prop. aged 51-59 in $t = 0$ | 0.20 | 0.40 | 0.19 | 0.39 | 0.21 | 0.41 |
| Prop. with dependent children in $t = 0$ | 0.58 | 0.49 | 0.58 | 0.49 | 0.59 | 0.49 |
| Age of dependent children in $t = 0$ | 12.23 | 5.61 | 12.29 | 5.61 | 12.18 | 5.62 |
| Prop. ever employed in $t \leq -1$ | 1.00 | 0.05 | 1.00 | 0.04 | 1.00 | 0.06 |
| Years of experience in $t = -1$ | 20.81 | 8.85 | 20.83 | 8.75 | 20.78 | 8.94 |
| Prop. employed in $t = -1$ | 0.96 | 0.19 | 0.96 | 0.18 | 0.96 | 0.19 |
| Prop. employed in private sector in $t = -1$ | 0.61 | 0.49 | 0.60 | 0.49 | 0.62 | 0.48 |
| Prop. employed in public sector in $t = -1$ | 0.14 | 0.35 | 0.15 | 0.36 | 0.14 | 0.34 |
| Prop. employed in para-public sector in $t = -1$ | 0.06 | 0.24 | 0.06 | 0.24 | 0.06 | 0.23 |
| Prop. self-employed in $t = -1$ | 0.17 | 0.38 | 0.17 | 0.38 | 0.17 | 0.38 |
| Prop. in professional occupation in $t = -1$ | 0.01 | 0.09 | 0.01 | 0.10 | 0.01 | 0.09 |
| Labor income in $t = -1$ | 24216.42 | 12681.93 | 24096.99 | 12625.93 | 24328.48 | 12734.13 |
| Daily wage in $t = -1$ | 72.36 | 40.08 | 71.86 | 38.14 | 72.82 | 41.82 |
| Days worked in $t = -1$ | 347.55 | 53.53 | 346.83 | 55.00 | 348.22 | 52.10 |
| Benefit in $t = 0$ | 10670.52 | 9974.44 | 10437.28 | 9605.68 | 10892.00 | 10308.06 |
| Income of deceased in $t = 0$ | 21361.10 | 21933.74 | 21886.54 | 20968.13 | 20589.71 | 23261.99 |
| Pension of deceased in $t = 0$ | 14104.45 | 13660.71 | 14528.82 | 12980.38 | 13701.51 | 14265.97 |
| Observations | 13556 | | 6562 | | 6994 | |

**Notes:** The table reports summary statistics for the balanced sample of surviving spouses with predicted taxable income in the second or higher income bracket. The statistics are computed on the sample of survivors whose benefit start date is within a 24-month symmetric bandwidth around September 1, 1995. Monetary quantities are expressed in 2010 prices. Labor income is unconditional on employment. Days worked and the wage rate are conditional on employment. The wage rate is computed as annual earnings divided by the number of days worked.

TABLE 1.B5: HETEROGENEOUS EFFECTS BY GENDER AND AGE IN $t = 0$

| | Gender | | Age in $t = 0$ | | |
|---|---|---|---|---|---|
| | Female | Male | 20-40 | 41-50 | 51-55 |
| | (1) | (2) | (3) | (4) | (5) |
| Benefit | -1984.11*** | -734.445*** | -2840.77*** | -1194.09*** | -2099.00*** |
| | (208.525) | (89.437) | (174.841) | (245.582) | (294.558) |
| | [11318.84] | [7129.74] | [8842.95] | [9612.85] | [8944.35] |
| MPE | -1.325*** | -0.106 | -1.097*** | -0.999 | -0.451 |
| | (0.376) | (0.772) | (0.459) | (0.644) | (0.299) |
| Participation rate | 0.101*** | 0.045*** | 0.028** | 0.036*** | 0.051*** |
| | (0.012) | (0.017) | (0.014) | (0.014) | (0.013) |
| | [0.639] | [0.553] | [0.883] | [0.585] | [0.212] |
| Days worked | 1.045 | -0.092 | 6.307*** | -3.999 | 3.418 |
| | (2.668) | (3.855) | (2.820) | (3.534) | (5.871) |
| | [341.04] | [338.62] | [347.63] | [336.31] | [326.39] |
| Daily wage | 1.673 | -5.854** | 1.359 | 0.526 | -1.944 |
| | (1.412) | (2.487) | (1.848) | (1.871) | (3.396) |
| | [74.238] | [83.820] | [73.630] | [80.669] | [80.453] |
| Benefit-start-month FE | x | x | x | x | x |
| Calendar year FE | x | x | x | x | x |
| Linear trend | x | x | x | x | x |
| Quadratic trend | x | x | x | x | x |

**Notes:** The table reports the estimated coefficient $\eta_0$ from equation 1.3, for various outcome variables and groups of survivors, pooling event-time years from $t = 0$ to $t = 15$. The second row reports instead the estimated coefficient $\beta$ from equation 1.1 using taxable income as outcome variable. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The mean value of the outcome variable in the control group is reported in square brackets. The wage rate is computed as annual earnings divided by the number of days worked. The estimates for days worked and the wage rate are all conditional on employment and include the number of years of work experience in $t = 0$ among the individual controls.

TABLE 1.B6: IV ESTIMATE OF THE EFFECT OF THE BENEFIT ON TAXABLE INCOME AND DISPOSABLE INCOME

| | Taxable income (1) | Disposable income (2) | Taxable income (3) | Disposable income (4) |
|---|---|---|---|---|
| Benefit | -0.943** | 0.057 | -0.847** | 0.153 |
| | (0.450) | (0.450) | (0.419) | (0.419) |
| Observations | 73783 | 73783 | 73783 | 73783 |
| Benefit-start-month FE | x | x | x | x |
| Calendar year FE | x | x | x | x |
| Linear trend | x | x | x | x |
| Quadratic trend | | | x | x |

**Notes:** The table reports the IV-RD coefficient $\beta$ from estimating equation 1.1 using different outcome variables and pooling event-time years from $t = 0$ to $t = 15$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. The IV estimates in columns (1) and (2) are based on a first stage with linear parametric specification, while those in columns (3) and (4) on a first stage with quadratic parametric specification with individual controls. All estimates are based on a 24-month symmetric bandwidth. Individuals with observed taxable income in the second and third income bracket are excluded from the estimation sample.

TABLE 1.B7: PLACEBO TEST FOR THE EFFECT OF THE REFORM ON THE DEPENDENCY PERIOD

| | Number of years with dependent children |
|---|---|
| *Placebo thresholds* | |
| September 1992 | -0.404 |
| | (0.568) |
| September 1993 | 0.757 |
| | (0.423) |
| September 1994 | -1.317*** |
| | (0.413) |
| September 1995 | 1.223*** |
| | (0.415) |
| September 1996 | -0.345 |
| | (0.421) |
| September 1997 | 0.390 |
| | (0.416) |
| September 1998 | -0.502 |
| | (0.540) |
| Benefit-start-month FE | x |
| Calendar year FE | x |
| Linear trend | x |
| Quadratic trend | x |

**Notes:** The table reports the coefficient $\eta_0$ from estimating equation 1.3 using different cutoff dates $\tau$. Robust standard errors are reported in parenthesis. P-value: *** p<0.01, ** p<0.05, * p<0.1. Estimates are based on a quadratic parametric specification with individual controls and a 24-month symmetric bandwidth. The dependency period is measured as the number of years with dependent children within the household.

TABLE 1.B8: SUMMARY STATISTICS FOR THE SAMPLE OF EITC RECIPIENTS IN THE MARCH CURRENT POPULATION SURVEY (1993-1997)

| | All household heads | | Single household heads | |
|---|---|---|---|---|
| | Mean | SD | Mean | SD |
| Female | 0.53 | 0.50 | 0.85 | 0.36 |
| Age | 35.47 | 8.56 | 35.18 | 8.44 |
| Prop. aged $< 40$ | 0.68 | 0.47 | 0.69 | 0.46 |
| Prop. aged 40-50 | 0.27 | 0.44 | 0.27 | 0.44 |
| Prop. aged 51-59 | 0.05 | 0.22 | 0.04 | 0.20 |
| Prop. with dependent children | 0.93 | 0.26 | 0.92 | 0.27 |
| Age of dependent children | 7.89 | 4.70 | 8.47 | 4.68 |
| Prop. single | 0.51 | 0.50 | | |
| Prop. employed in $t = -1$ | 0.93 | 0.25 | 1.00 | 0.00 |
| Prop. employed in private sector in $t = -1$ | 0.79 | 0.41 | 0.82 | 0.38 |
| Prop. employed in public sector in $t = -1$ | 0.12 | 0.32 | 0.13 | 0.33 |
| Prop. self-employed in $t = -1$ | 0.10 | 0.30 | 0.05 | 0.22 |
| Total income (excl. unearned) | 22066.68 | 14261.54 | 23397.75 | 13192.35 |
| Estimated EITC amount | 1557.91 | 1053.41 | 1645.41 | 1045.20 |
| Observations | 28036 | | 13600 | |

**Notes:** The table reports summary statistics for the sample of EITC recipients in the March Current Population Survey (CPS) for the years 1992-1997. The sample is restricted to household heads aged 18-55 at the time of the survey. The first two columns report the mean and standard deviation of variables for all household heads, while the last two columns report the same statistics for the sample of single household heads (i.e. those for whom no cohabiting spouse is recorded in the data). Observations are weighted using the CPS individual weights. Monetary quantities have been converted in 2010 euros using the US CPI and euro/dollar purchasing-power-parity conversions.

## 1.C Additional Results

### 1.C.1 Proof of Proposition 1

This section shows that the semi-elasticity of participation to the benefit, rescaled by the semi-elasticity of labor supply to labor earnings, can be used to estimate the welfare gain of increasing survivor benefits in the widowhood state.

I develop a model in which widow(er)s choose labor supply at the extensive margin. Preferences are defined over consumption and labor. When participating in the labor market, individuals incur an additively separable utility cost $\phi$ and earn labor income $z$. Let utility be given by

$$u(c) - \mathbf{I}\{l = 1\} \cdot \phi \tag{1.C1}$$

where $u(\cdot)$ is a concave utility function, $c$ is consumption, $l \in \{0, 1\}$ a binary labor force participation decision and $\phi$ labor disutility. $\phi$ is distributed with probability density function $f(\phi)$ and cumulative distribution function $F(\phi)$. Assuming that labor force participation generates income $z$, the budget constraint is

$$c = \mathbf{I}\{l = 1\} \cdot z + B \tag{1.C2}$$

where $B$ is the survivor benefit.

Let $V(z, l, B)$ denote the indirect utility function. Individuals decide to work if and only if

$$V(z, 1, B) - V(0, 0, B) \geq \phi \tag{1.C3}$$

which is equivalent to a threshold rule whereby individuals work if and only if $\phi \leq \overline{\phi}(z, B)$. The probability of working – i.e. the labor force participation rate – is $\Phi(z, B) = F(\overline{\phi}(z, B))$.

The semi-elasticity of labor supply with respect to the benefit is

$$\frac{d\Phi}{d \log B} = f(\overline{\phi}) \cdot \frac{\partial \overline{\phi}}{\partial B} \cdot B = f(\overline{\phi}) \cdot \left[ \frac{\partial V(z, 1, B)}{\partial B} - \frac{\partial V(0, 0, B)}{\partial B} \right] \cdot B \tag{1.C4}$$

Using a first order Taylor expansion around $z = 0$, we have

$$\frac{d\Phi}{d \log B} \approx f(\overline{\phi}) \cdot \frac{\partial^2 V}{\partial z \partial B} \cdot z \cdot B \tag{1.C5}$$

Since $\dfrac{\partial V}{\partial z} = u'(c(B))$ is the marginal utility of income, we have

$$\frac{d\Phi}{d \log B} \approx f(\overline{\phi}) \cdot \frac{\partial u'(c(B))}{\partial B} \cdot z \cdot B = f(\overline{\phi}) \cdot u''(c(B)) \cdot z \cdot B \tag{1.C6}$$

Rescaling the above expression by the semi-elasticity of labor force participation to labor earnings $\varepsilon = \dfrac{d\Phi}{d\log z} = f(\overline{\phi}) \cdot u'(c(B)) \cdot z$ and applying a first order Taylor expansion around $B = 0$, we obtain

$$\frac{\left[\dfrac{d\Phi}{d\log B}\right]}{\varepsilon} \approx \frac{u''(c(B)) \cdot B}{u'(c(B))} \approx \frac{u'(c(B)) - u'(c(0))}{u'(c(B))} \tag{1.C7}$$

or equivalently, the negative of the labor supply response to $\log B$ divided by $\varepsilon$ provides a measure of the marginal benefit ($MB$) of survivor insurance:

$$MB = \frac{u'(c(0)) - u'(c(B))}{u'(c(B))} \approx -\frac{\left[\dfrac{d\Phi}{d\log B}\right]}{\varepsilon} \tag{1.C8}$$

# Chapter 2

# Subsidizing Labor Hoarding in Recessions: The Employment and Welfare Effects of Short Time Work

## 2.1 Introduction

The Great Recession has generated a significant revival of interest in policies destined at encouraging labor hoarding by firms during downturns (e.g. Yagan, 2017; Giroud and Mueller, 2017). Short time work programs (STW), which are subsidies for temporary reductions in the number of hours worked, are the most emblematic of such policies, and have been aggressively used during the Great Recession, especially in European countries. The fraction of employees on STW in 2009 reached 7% in Belgium, close to 5% in Germany and 4% in France.[1] In Italy, according to social security data, 4.6% of the workforce was in STW in 2013, for a cost of 0.5% of GDP. This revival of interest is also palpable in the U.S., where state STW programs have been actively promoted by the Job Creation Act of 2012. In 2016, more than 28 U.S. states had implemented their own STW program.[2]

But what is behind this STW craze? Do we know that it is effective in stabilizing employment? Is it helping firms hold onto their productive workers? Is it an effective way to provide insurance to workers? More effective than unemployment insurance (UI) for instance? More fundamentally, do we know anything about its welfare implications? What sources of inefficiencies are we trying to correct with STW? If we believe that hours or employment are not optimally set in the labor market, how can STW deal with these inefficiencies? Are we not creating additional inefficiencies with these programs, by keeping workers in unproductive firms, preventing efficient reallocation of labor?

[1]See Hijzen and Martin (2013), and Cahuc, Kramarz, and Nevoux (2018)

[2]U.S. Department of Labor Office, 2016.

Despite STW being a key element to the countercyclical policy toolkit, and one of the main active labor market policies during downturns, we are completely at a loss to answer these fundamental questions: we know close to nothing about the effects of STW, and its welfare consequences. This is all the more surprising given the large literature devoted to the use of other insurance programs such as UI over the business cycle (Schmieder, von Wachter, and Bender, 2012; Landais, Michaillat, and Saez, 2018a; Landais, Michaillat, and Saez, 2018b; Kekre, 2016).

There are however three simple reasons that explain the very limited knowledge that we have of the effects and desirability of STW. The first reason is a critical lack of firm or individual level administrative data on STW.[3] The literature on STW had to mainly resort to cross country analysis (e.g. Van Audenrode, 1994; Boeri and Brücker, 2011; Cahuc and Carcillo, 2011). But even in the presence of firm level data, the second issue lies in the lack of credible sources of identification of STW treatment.[4] Most papers therefore rely on the structure of calibrated models to analyze the effects of STW on workers and firms (Tilly and Niedermayer, 2016; Cooper, Meyer, and Schott, 2017). Alternatively, a few studies have tried to find instruments for the take up of STW, but their results have not enabled to reach any consensus.[5] Even if we were to have some credible estimates of the effects of STW on workers' and firms' outcomes, the third issue is the lack of a simple, tractable yet general conceptual framework to rationalize the empirical evidence and feed these estimates back into a welfare evaluation that would make transparent the trade-offs implied by STW policies.[6]

This paper contributes to our understanding of STW by addressing these three limitations. It relies on uniquely rich administrative data on STW from Italy. It uses the presence of variation in eligibility rules across firms to provide compelling evidence of the causal impact of STW on firms' and workers' outcomes. And it offers a simple and general conceptual framework that maps to our empirical results to transparently assess the welfare consequences of STW programs.

[3]As a matter of example, the German social security administration (*IAB*) does not collect data on STW. Most STW applications and reports are sent in a paper format to the Federal Employment Agency, and are not digitized. Only a sample of these reports has been digitized for the Nuremberg metropolitan area for years 2008 to 2010 and matched to *IAB* data (Tilly and Niedermayer, 2016).

[4]In most countries with large STW programs in place, like Germany or France for instance, there is no variation in a firm's eligibility to take up STW. This severely complicates identification, with no obvious method to control for the selection of firms into STW take up.

[5]Most studies instrument STW take up during the recession with the prior experience of firms with the program (e.g. Boeri and Brücker, 2011; Cahuc and Carcillo, 2011; Hijzen and Martin, 2013) and find competing results. Recently, Cahuc, Kramarz, and Nevoux (2018) offer a credible and compelling IV strategy in the French context. They instrument STW take up using the proximity of a firm to other firms having used STW before recessions. They also use as an alternative instrument response time variation in the administrative treatment of STW applications across French departments. They find, similar to our results, large and significant employment effects of STW treatment.

[6]While a small theoretical literature shows (not surprisingly) that STW may distort both hours (Burdett and Wright, 1989), and the allocation of workers across firms, thus reducing output (Cooper, Meyer, and Schott, 2017), there is no clear view of the conditions under which STW programs might be socially desirable and improve welfare.

Our data comes from the Italian social security administration (INPS) and covers the universe of Italian employer-employee matches in the private sector, and the universe of all social security and transfer payments in Italy, from 1983 to 2015. Besides granular information on firms and workers' histories, it provides detailed information on eligibility, applications and authorizations of the universe of STW episodes at both the firm and individual levels from 2005 to 2015. This data, combined with the specificities of the Italian STW program, which creates variation in eligibility across firms, allows us to provide causal evidence of the effects of STW. Identification stems from the interaction between two sources of variation in eligibility: INPS codes and firm size. First, we exploit the fact that within 5-digit industries, certain firms, defined by particular INPS codes, are eligible while others are not, because of the particular interpretation of the STW Law that was given by INPS in a circular dating back to the 1970s. While this variation in STW access across otherwise very similar firms appears exogenous to economic conditions at such fine level today, we use the additional requirement that firms must be above a certain full-time equivalent size threshold to be eligible for the program. This enables us to test and control for the possibility that differential time shocks affected eligible and non-eligible INPS codes within 5-digit industries during the recession. We further provide multiple robustness checks for the validity of our approach. In particular, we show that our approach is not confounded by manipulation of size or INPS codes, nor by any other change in regulations at the main eligibility size threshold.

Our results demonstrate that STW has large and significant effects on firms' employment at both the intensive and extensive margin. Compared to counterfactual firms, firms treated by STW experience a 40% reduction in hours worked per employee, and a similar magnitude increase in the number of employees in the firm, with no discernible effect on wage rates. Unpacking the full dynamics of treatment effects, we show that these employment effects are temporary, and immediately disappear once STW treatment stops. On the workers side, we similarly find that treatment effects are all concentrated in the short run. STW has immediate positive effects on employment probability, but negative effects on hours, and a positive effect on total earnings and transfers. But these effects disappear after treatment, so that STW provides no significant insurance to workers in the medium or long run. In fact, two years after treatment, there are no significant differences in the employment probability, earnings, and total income of workers who were treated by STW and workers who were counterfactually laid-off.

We then analyze the selection of firms into STW and the heterogeneity in the treatment effects of the program, to shed light on the mechanisms behind the temporary nature of the average estimated effects and the lack of long term insurance for workers. In particular, we show that firms that were at the bottom of the productivity distribution before the Recession are three times more likely than higher productivity firms to take up STW during the Recession and that employment effects for them are significantly smaller. This clearly suggests that STW is predominantly targeting firms that have permanently lower productivity and helps explain why keeping workers in these firms does not entail significant long term benefits. More importantly, this suggests that by keeping workers in low productivity firms, STW may have significant negative reallocation effects in the labor market.

To investigate these claims, we leverage the rich spatial variation available in Italy across more than 600 local labor markets (LLM) and estimate how an increase in the fraction of workers treated by STW in a LLM affects employment outcomes of non-treated firms. We instrument variation in the intensity of STW treatment across LLM by the average yearly fraction of eligible workers in the LLM based on the interaction between firm size and INPS codes in the pre-recession period, controlling for a rich set of firm and LLM characteristics. We provide various placebo tests confirming the validity of our IV strategy. Our results provide compelling evidence of the presence of equilibrium effects of STW within labor markets. We show that STW significantly decreases the employment growth and inflow rates of non-treated firms, and has a significant negative impact on TFP growth in the labor market.

We finally provide a tractable search and matching framework that rationalizes these empirical findings, and maps our estimates into a transparent welfare evaluation of STW. While remaining general, the model adds a series of key ingredients that prove critical to evaluate the arguments put forward in favor or against the existence of STW programs. First, workers are risk averse and are imperfectly insured so that insurance against the incidence of productivity shocks on workers' earnings is socially desirable. The model allows firms, which are subject to (idiosyncratic and/or aggregate) productivity shocks, to adjust labor inputs along both the intensive (hours) and extensive (employment) margin.[7] Wages and hours are negotiated between workers and firms, to split the surplus generated by matches in a frictional labor market. And hours adjustments are therefore constrained by the outside options of workers.[8] More generally, wage and hours schedules may not always guarantee that hours and employment are set at their socially efficient level, opening the potential need for a government intervention to stabilize employment and hours.[9] In this environment, STW policies, by affecting equilibrium in the labor market, naturally create reallocation effects between high and low productivity firms.

Many arguments have been put forward in the public debate in favor or against STW. STW provides insurance to workers, it provides insurance to firms against the costs of

[7]In that sense, our model is related to Cooper, Meyer, and Schott (2017), but with two important departures: first workers are risk averse and insurance markets are incomplete, which provides a rationale for social insurance. And second, the hours constraint, which creates potential inefficiencies in employment is endogenous rather than exogenous.

[8]As in Braun and Brügemann (2014), this means that STW may correct inefficiencies created by the unemployment insurance system.

[9]Note that we do not impose restrictions on the matching environment. Assuming directed search, as in Cahuc, Kramarz, and Nevoux (2018) for instance, would guarantee that hours and employment are always socially efficient, leaving no role for STW to correct potential inefficiencies in employment and hours over the business cycle.

replacing workers, it mitigates hours and wage rigidities that may prevent optimal labor adjustments, it stabilizes employment, it inefficiently reallocates labor towards low productivity firms, etc. Thanks to its generality, our model encompasses an array of previous frameworks used in the small theoretical literature on STW, and enables to review within one single framework most of these important arguments. We contribute by clarifying the conditions under which STW "works", i.e. induces firms hit by productivity shocks to take up the program, and increase their employment. In particular we show that wage rigidity critically amplifies the employment responses to STW.

Importantly, our model is directly related to the public finance literature on optimal policies in equilibrium models of the labor market (see for instance Landais, Michaillat, and Saez, 2018a, and Michaillat and Saez, 2017). In the spirit of this sufficient statistics literature, we use the model to provide a general formula for the optimal generosity of STW subsidies which clarifies the key welfare tradeoffs of STW programs. The main insight is that optimal STW not only balances the insurance value of the subsidy with its fiscal externality but also needs to account for two additional sources of inefficiencies: first employment may be inefficient due to the frictional nature of the labor market, and second, equilibrium hours may also not be at their socially optimal level due to the missing market for hours. STW will entail positive welfare gains when equilibrium employment is suboptimally low, and hours suboptimally high, and our formula offers a clear representation of these hours and employment inefficiency terms. The advantage of this approach is that the formula, and the key tradeoffs underpinning it, remain the same irrespective of the exact structure and primitives of the underlying model. In that sense, our formula is robust to the way wages and hours are determined in the model, to the specification of the costs of replacing or firing workers, to the presence of specific human capital, to various sources of hours or wage rigidity, to the presence of liquidity constraints, etc. Furthermore, our approach offers the possibility to conduct a local welfare calibration using our reduced form estimates, which suggests that in the current Italian context, both the fiscal externality and the insurance value of STW are high, and that the marginal welfare gains of further increases in STW are small as the employment and hours inefficiencies are equally large but of opposite sign.

Finally, we use a calibrated version of the model to run non-marginal counterfactual analysis and quantify the welfare effects of removing STW. This analysis confirms that the welfare gains of further increases in the generosity of STW are small, but the value of having STW is significantly positive. In the absence of any STW subsidy, the unemployment level would have been almost 2 percentage points higher during the recession, and total TFP about 2% higher, but at a total welfare cost of about 2%. We also use the calibrated model to explore various counterfactual scenarii and gain further insights on the effects of STW outside the specific context of Italy, where the Great Recession transformed into a long, protracted shock. We show that the immediate employment effects of STW are significantly larger (around 20% to 40%) when the aggregate shock is temporary than when it is permanent, as firms' desire for labor hoarding is much greater for temporary shocks, especially when the cost of replacing workers is high, and when the magnitude of the aggregate shock is large. This suggests that STW might have been much more effective in the German context than in Italy during the Great Recession.

The remainder of the paper is organized as follows. Section 2.2 describes the Italian STW institutions and the data. Section 2.3 presents the identification strategy and our estimates of the effects of STW on firms outcomes. We explore in Section 2.4 the corresponding effects of STW on the short run and long run outcomes of workers. Section 2.5 investigates selection into the program and heterogeneity in its treatment effects before presenting clear evidence of the spillover effects of the program on untreated firms. Section 2.6 develops the model and explores the welfare implications of our findings.

## 2.2 Institutional Background and Data

### 2.2.1 The Italian *Cassa Integrazione Guadagni* (CIG)

The Italian Cassa Integrazione Guadagni (CIG) was created in 1941. It represents, with the German *Kurzarbeit*, one of the oldest, largest and most comprehensive short-time work programs in the world. It was heavily used during the latest recession: in 2013, almost 5% of the Italian workforce was on STW, for a cost of roughly 0.5% of Italian GDP. This massive expansion of STW take-up makes Italy the perfect laboratory to analyze the employment and welfare consequences of STW during the Great Recession.

CIG is composed of three programs: Cassa Integrazione Guadagni Ordinaria (CIGO), Cassa Integrazione Guadagni Straordinaria (CIGS) and Cassa Integrazione Guadagni in Deroga (CIGD). We focus throughout the paper on the second program, CIGS, which is the main pillar of STW used in recessions.[10]

CIGS rules are quite standard among STW programs, and make it a good example of most of the programs implemented across OECD countries. CIGS targets firms experiencing economic shocks, broadly defined: it can be a demand or revenue shock, a company crisis, a need for restructuring or reorganization, an illiquidity or insolvency issue, etc. CIGS is a subsidy for partial or full-time hour reductions, replacing approximately 80% of the earnings forgone by the worker due to hours not worked.[11] The subsidy is

[10]CIGO is restricted to small transitory shocks or accidents involving forced reduction of activity (e.g. adverse weather conditions, earthquakes, power cuts). It is restricted to the manufacturing sector and has a maximum duration of 13 weeks. CIGD is a smaller additional program created in 2009, administered at the local level and granted ad-hoc on the basis of regional decrees.

[11]Hours not worked are computed against the regular hours stipulated in the labor contract. The normal weekly working hours are 40 in Italy. There is almost no variation in the replacement rate of the subsidy across workers. If a firm is eligible, all workers with at least 90 days of tenure are eligible to be put on CIGS, except for apprentices and top executives of the firm. Firms are free to decide the amount of hours reductions they request, i.e. there is no minimum or maximum amount of hours reduction in the CIGS program.

available to workers in the private sector and is administered by the Italian Social Security (INPS). The subsidy is remitted directly to the workers. Firms intending to use the program must file an application to the Social Security or the Ministry of Labor, providing a justification of economic need and a recovery plan.[12] Once authorized, the usage of CIG is subject to weak conditionality requirements for both firms and workers: there are no provisions for compulsory training nor prohibitions of dismissal by firms, and no job-search requirements for employees. The cost to firms of putting workers on CIGS is minimal: they pay a fee to INPS equal to 3 to 4.5% of the total amount of the subsidy to workers.[13] CIGS is otherwise financed via ordinary payroll contributions. The duration of the program is up to 12 months, with limited possibilities of extensions. Utilization of the program need not be on a continuous basis, but cannot exceed a maximum duration of 36 months – including extensions – over 5-year periods that are fixed and defined by the law. In practice, almost all firms use CIGS for exactly 12 months, with the median and average durations of CIGS take-up both almost equal to 52 weeks.

One of the specificities of CIGS is the presence of various provisions of the law that create quasi-exogenous variation in eligibility across firms, offering the unique possibility of identifying the causal effect of short-time work programs on firm and individual outcomes. This is remarkable as most STW programs like the German *Kurzarbeit* or the French STW, provide little to no variation in eligibility across firms, making it complicated to identify the causal effect of STW in these contexts (Cahuc, Kramarz, and Nevoux, 2018). We exploit the fact that a firm's eligibility for CIGS depends in particular on two dimensions: an INPS specific code called "contributory regime" and the size of the firm prior to filing an application.

Contributory regimes (or INPS codes) are created by combining 5-digit industry codes and 333 different "codice autorizzazione".[14] Eligibility of each INPS code to CIGS is determined by a circular of the Ministry of Labor translating the provisions of the Law on Cassa Integrazione, and made operational by INPS, dating back to the 1970s. As a consequence, within fine-grained 5-digit industry codes (594 industries), there is variation in CIGS eligibility across otherwise very similar firms, due to regulations from the Ministry of Labor that are quite plausibly exogenous to economic conditions at such fine level today. To provide just a few concrete examples: within the 5-digit industry codes 11306, 11307 and 11308, which are firms in construction specialized in the installation of electrical machinery, only those with codice autorizazzione 3N are eligible; within the

[12] Using data on CIGS applications and authorizations, we found that in practice, applications are never rejected: 99.99% of applications are authorized by the Ministry of Labor.

[13] The fee is 3% for firms with up to 50 employees and 4.5% for larger firms. In 2015, a reform introduced an experience rating component to the costs of CIGS to the employer by making the fee an increasing function of the amount of subsidized hours.

[14] The "codice autorizzazione" is an administrative code used by INPS that, in combination with the 5-digit industry code, defines the various programs and contributions a firm is eligible to or subject to. The combination of 5-digit industry codes and "codice autorizzazione" creates an INPS code that allows to univocally identify the contributory regime and CIGS eligibility of any given firm.

5-digit code 10106, which are firms that produce seeds and beans, only firms with codice autorizzazione 3A are eligible.

Besides INPS codes, a firm eligibility to CIGS depends on its size being above a certain threshold. This variation in eligibility across firms of different sizes allows to use non-eligible firms within INPS codes to test and control for differential time shocks across eligible vs non eligible INPS codes. The main size requirement is that a firm must have employed on average more than 15 employees in full-time equivalent (FTE) units in the six months prior to the application.[15] For some industries in the retail sector, the size requirement differs, and is set to 50 FTE. Note that employment legislation regulating dismissals also apply in Italy when a firm reaches 15 employees within a single establishment or municipality, or 60 employees in the firm in Italy as a whole.[16]

We explain in Section 2.3.1 how these sources of variation in eligibility across INPS codes and firm size can be combined to identify the effects of CIGS on firms and workers.

### 2.2.2 Data

We use administrative data from INPS on the universe of employer-employee matches and social security payments in the private sector in Italy from 1983 to 2015. The data includes detailed information on workers' demographics, working histories, participation in all social assistance and social insurance programs. It also provides detailed information on firm characteristics such as employment, labor-force composition and industry. Most importantly, starting from 2005, the data provides information on eligibility, applications, authorizations, duration and payments of the Italian short-time work program at the individual and firm level. We linked the administrative archives to firm-level balance-sheet data from CERVED via a unique identifier. CERVED is a firm register containing balance sheet information of all incorporated limited liability companies in Italy. The balance-sheet information covers roughly 50% of firms in the administrative records and enables to create various measures of productivity and credit constraints.

We define STW events at the firm level as any month in which a STW episode is reported in the INPS records, which is also authorized according to the authorization data. When aggregating at the annual level, an event is defined as having at least one STW episode during the year. Eligibility status is defined dynamically using INPS codes and based on the maximum 6-month average FTE firm size in each year.[17]

[15]To be precise, eligibility to CIGS, and therefore eligibility requirements, all apply at the establishment level. INPS codes are also establishment specific. When we refer to firms throughout the paper, we mean "establishments". We restrict our baseline sample to single establishment firms.

[16]In Section 2.3.3, we explain and provide multiple evidence that our approach is robust to the variation in dismissal costs at the 15 FTE threshold. We use in particular multi-establishments firms that are always subject to the dismissal cost regulation.

[17]The FTE size measure relevant for establishing CIGS eligibility is computed considering all employees, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units. Eligible firms must have employed on average at least 15 employees in FTE in the 6 months prior to their application. Firms that have less than six months of activity should

To define intensive measures of employment, we leverage detailed weekly level information on whether a worker was working full-time or part-time. When working part-time, we have information on the percentage of part-time work. We use this information to create a measure of hours worked for each worker. We assign 40 hours per week to full-time workers, and weight hours for part-time work using the percentage of part-time work, assuming a corresponding full-time contract of 40 hours.

Our main sample of analysis is a balanced panel of all ever-active private sector firms that ever reach an average 6-month full-time equivalent firm size between 5 and 25 in the period 2005 to 2014. Our sample of workers is a balanced panel of all workers ever working in these firms.[18] Appendix Table 2.A1 provides descriptive statistics on our main sample of firms in 2008, prior to the start of the Great Recession. The average firm size in our sample is close to 9 employees, with an average of 38.7 weekly hours worked per employee. The average wage bill per employee is 20.6k euros. The table also breaks down firms between eligible and non-eligible INPS codes. Despite being unequally distributed across industries, firms in eligible and non-eligible INPS codes are quite similar in terms of observable characteristics prior to the Great Recession. Firms in eligible INPS codes are slightly larger, but are quite comparable in terms of hours worked per employee, wage bill per employee, revenues, investment and liquidity. Table 2.A2 provides similar information for workers in our main sample of analysis. Workers in eligible INPS codes are more likely to be male and blue collars, and they are also slightly older than workers in non-eligible INPS codes, which reflect the fact that manufacturing is more represented in eligible INPS codes than in non-eligible INPS codes.

Appendix Figure 2.A1 reports additional information on the distribution of treatment across workers in firms experiencing STW. Panel A plots the distribution of the ratio of treated workers to eligible workers in firms currently under short time work treatment, and shows that most firms choose to put all their eligible workers in the program and therefore spread hours reductions across all eligible workers. Panel B reports the distribution of reported weekly hours reduction of workers currently experiencing STW. The graph shows a smooth distribution of hours reductions, with a mode around 0.25, and an average weekly hours reduction of a little more than 35%.[19]

consider the average number of employees (in FTE) in the month or months of activity. In order to determine whether a firm meets the size requirement, we use the exact FTE firm size measure that determines CIGS eligibility as provided by INPS (the variable is called "forza aziendale").

[18]We restrict the main analysis to the period up to 2014, as an important reform of Italian labor market regulations started being implemented in 2015, which may have interfered with the effects of STW programs.

[19]Figure 2.A1 therefore provides evidence that STW does not work like temporary layoffs, but effectively like hours reductions spread across all workers in the firm.

## 2.3 Effects of STW on Employment and Firm Outcomes

### 2.3.1 Identification

The eligibility requirements of the Italian CIGS create sharp variation in a firm's probability to use STW based on INPS codes and firm size.

Appendix Figure 2.2 provides direct evidence of this variation in access to CIGS by INPS codes and firm size. Panel A plots, among firms with eligible INPS codes in our sample, the evolution of the fraction of firms receiving CIGS in each calendar year $t$ from 2005 to 2014, for firms with a maximum 6-month average size of 15 to 25 full time equivalent employees in year $t-1$ and for firms with a maximum 6-month average size of 5 to 15 full time equivalent employees in year $t-1$. For firms with more than 15 FTE employees, CIGS take up rose sharply from less than 1% before the onset of the recession, to roughly 8% throughout the recession. While for firms with less than 15 employees, take up was essentially zero throughout the period. Panel B of Figure 2.2 replicates the same exercise for firms in non-eligible INPS codes. For both firms below and above the 15 FTE threshold, the take up is null throughout the entire period.

Our main identification strategy relies on using the interaction of being in an eligible INPS code, and having more than 15 FTE as a source of quasi-experimental variation in CIGS treatment after the onset of the recession in 2008. For each outcome $Y$, the baseline specification underlying our reduced-form graphical evidence is:

$$\begin{aligned} Y_{igst} = \; & \sum_j \gamma_1^j \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \right\} \\ & + \sum_j \sum_k \gamma_2^{jk} \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] \\ & + \sum_j \sum_k \gamma_3^{jk} \cdot \left\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] \\ & + \sum_j \sum_k \gamma_4^{jk} \cdot \left\{ \mathbb{1}[j = t] \right\} \cdot \mathbb{1}[k = s] + v_{igst} \end{aligned} \tag{2.1}$$

where $Y_{igst}$ denotes outcome $Y$ for firm $i$, belonging to INPS code group $g$, in 5-digit industry $s$ in year $t$. A firm can either be in the group of INPS codes eligible to receive CIGS ($g \in \mathcal{E}$) or in the group of non-eligible firms ($g \in \mathcal{E}^{\complement}$). $N_{i,t-1}$ is firm $i$'s full time equivalent size in calendar year $t-1$. Note that by systematically controlling for 5-digit industry fixed effects and their interactions with time and firm size, we only exploit variation in eligibility of INPS codes across firms within the same fine-level industry codes. This variation stems from the interaction between industry codes and "codice autorizazzione".[20] To restrict our attention to comparable firms in a narrow neighborhood around the 15 FTE cut-off, we estimate the above model on our baseline balanced panel of firms

[20]This approach therefore fully controls for the fact that eligible firms are not evenly distributed across 5-digit industries nor across "codice autorizazzione".

who ever reach a size between 5 and 25 FTE. Our graphical evidence consists in plotting the estimated coefficients $\hat{\gamma}_1^t$ for all years $t$, which capture the evolution over time of the relative outcomes of firms that are just above and just below the 15 full-time equivalent employee threshold in eligible INPS codes, versus firms that are just above and below the same 15 full-time equivalent employee threshold in non-eligible INPS codes, but within the same 5-digit industry. The omitted year in specification (2.1) is 2007, so results are expressed relative to levels in year 2007.

Estimates of the effect of STW treatment are obtained from running IV models where we instrument the probability of STW treatment $T$ by the triple interaction of being after the onset of the recession, being in an eligible INPS code and having more than 15 FTE employees:

$$\begin{aligned}
Y_{igst} = \quad & \beta_{IV} \cdot T_{igst} \qquad (2.2)\\
& + \sum_j \sum_k \eta_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \eta_3^{jk} \cdot \Big\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \eta_4^{jk} \cdot \Big\{ \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] + \mu_{igst} \\
T_{igst} = \quad & \kappa_1 \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t > 2008] \Big\} \qquad (2.3)\\
& + \sum_j \sum_k \kappa_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[j = t] \Big\} * \mathbb{1}[k = s] \\
& + \sum_j \sum_k \kappa_3^{jk} \cdot \Big\{ \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] \\
& + \sum_j \sum_k \kappa_4^{jk} \cdot \Big\{ \mathbb{1}[j = t] \Big\} \cdot \mathbb{1}[k = s] + \nu_{igst}
\end{aligned}$$

Note that our approach allows for fully flexible 5-digit industry specific time shocks, so that our identification is not confounded by differences in the way various industries responded to the recession. Furthermore, within industry, we allow for fully flexible INPS code time shocks. In other words, we allow for the fact that within industry, firms in eligible and non-eligible INPS codes might have fared differently during the recession. Finally, within industry, we also allow for fully flexible time shocks interacted with firm size. This controls for the fact that, in Italian Labor Laws, firms are exposed to different regimes when larger than 15 employees. Our strategy therefore allows for these differential regimes to impact differently over time, firms just below 15 employees and firms just above 15 employees, within each industry.

Given this rich set of flexible controls, our identification rests on the assumption that there are no unobservable time shocks that would be, within each industry, specific to firms that are in the set of INPS codes eligible to CIGS *and* whose size is just above the

15 FTE threshold. Or equivalently, we rely on the parallel trend assumption that size specific time shocks are common across eligible and non-eligible INPS codes within the same industry, and that "INPS code"-specific time shocks within a given industry are common across firm just above and below 15.

We explore the credibility and validity of these assumptions in a series of robustness tests in Section 2.3.3. In terms of inference, we define two groups of firm sizes: a group with FTE above 15 in $t-1$ and a group with FTE below 15 in $t-1$, and we cluster all our standard errors at the INPS code times firm size group level. We explore additional inference approaches such as permutation tests (see footnote 24).

### 2.3.2 Results

Figure 2.1 Panel A starts by providing a graphical representation of the CIGS variation used to identify the causal effects of STW. It plots the coefficients $\widehat{\gamma_1^t}$ for all years $t$ from a regression following specification (2.1), using as an outcome the probability that a firm receives CIGS treatment. It confirms the evidence from Appendix Figure 2.2 discussed above, that our instrument generates a sharp and significant first-stage. Our instrument accounts for a 5 percentage point increase in the probability of CIGS take-up by firms during the 2008 recession, starting from a baseline very close to zero for all firms prior to the onset of the crisis. Regarding the timing, the graph also shows that CIGS take-up quickly increased after the onset of the recession, and was high throughout the recession, with a peak in 2013.

Figure 2.2 displays estimates of the effect of STW on employment outcomes and wages. For each panel, we plot the coefficients $\widehat{\gamma_1^t}$ for all years from 2000 to 2014, based on a regression following specification (2.1), and we also report on the graph the estimated IV coefficient $\widehat{\beta_{IV}}$ of the effect of CIGS treatment following the IV model in specification (2.2).

First, the figure provides supporting evidence for our identifying assumption, by confirming, for each outcome, the absence of differential pre-trends between firms just below and just above the 15 FTE threshold in eligible and non-eligible INPS codes within the same industry.

The figure also suggests that STW has had large employment effects at both the intensive and extensive margin but insignificant effects on wage rates. Panel A shows that CIGS reaches its primary intent, by allowing firms to reduce employment at the intensive margin. Our estimates suggest that CIGS access enables firms to significantly reduce the number of hours worked per employee by $e^{-0.51}-1=40\%$ on average. While reducing employment at the intensive margin, CIGS treatment significantly increases employment at the extensive margin, as shown in Panel B. Firms experience a large and highly significant increase in headcount employment of $e^{0.38}-1\approx 45\%$ due to CIGS treatment. Importantly, Panel C suggests that CIGS has no statistically significant effect on wage rates, defined here as earnings per hour worked per worker. This rigidity of wages means that the wage bill per employee decreases significantly with CIGS, by about 45% as shown in Panel D, since workers work less hours for the same wage rate cost to the firm.

In Table 2.1, we provide additional results of the effects of STW treatment on various firms' outcomes. Panel B shows that the positive employment effects are driven by an increase in the relative number of employees in open-ended contracts. The estimated IV coefficient for the effect of CIGS treatment on the log number (headcount) of employees in open-ended contract is $\beta_{IV} = 0.61\ (0.043)$, but the number of employees in fixed-term contracts is negatively impacted by CIGS treatment ($\beta_{IV} = -0.40\ (0.11)$). This reallocation of employment between open-ended and fixed-term contracts reflects the duality of the Italian labor market (Boeri, 2011).

Panel C of Table 2.1 presents results on the effect of STW on balance-sheet and productivity outcomes. These results are estimated on the sample of firms that were matched to their balance sheet data from CERVED. To get a better idea of the magnitude of the effects, we report the estimated IV coefficient $\beta_{IV}$ scaled by the average value of the outcome for non-eligible firms in the post 2008 period. Our results suggest that there is a small positive (yet not significant) effect of STW on firms' total output. We measure total output by firm value added, that is, total revenues plus unsold stocks minus cost of goods and services used in production.[21] We find a small positive insignificant effect of STW of 0.09 (0.16). Value added per worker goes down significantly by roughly 50% (12%) in response to STW treatment. Interestingly, this result of a negative effect on value added per worker provides evidence that the hours and employment responses to STW are real responses, and are not simply driven by reporting behavior. One may indeed worry that collusive avoidance behavior may occur within the firm, by which firms report less hours to INPS so that workers may benefit from the STW subsidy, while real working hours remain unchanged. If it were the case though, value-added per worker would remain unchanged when measured in the CERVED data. The significant decline in value-added per worker indicates that our estimates of hours responses to STW capture real behavior rather than avoidance.

Finally we investigate the effect of STW on firms' investment and liquidity, defined as cash and cash equivalents. We do not find any effect on investment and find a positive effect (although very imprecisely estimated) on liquidity. Combined with the large employment effect of STW and with wage rigidity, the fact that a firm's liquidity reacts to STW treatment, suggests that internal funds constraints may play a role in amplifying employment responses to negative productivity shocks, as suggested by Schoefer (2015).[22]

[21] In effect, this is equivalent to defining firm output as total profits plus total capital depreciation plus total wage cost.

[22] We provide additional evidence on the role of liquidity constraints in Section 2.5.1.

### 2.3.3 Robustness

The first potential concern with our identification strategy is that firms may endogenously select into either firm size or eligible INPS code in order to benefit from STW.

In terms of firm size, treatment eligibility is determined by a firm's largest 6-month continuous FTE size in the year prior to STW application. While this may limit manipulation opportunities in practice, firms with private information about future shocks may still have the possibility to endogenously adjust their FTE size ex-ante. To assess to what extent size manipulation creates significant selection susceptible of biasing our results, we first display in Appendix Figure 2.A3 the probability density function of FTE size over our entire sample period. Size manipulation to benefit from STW treatment in response to the 15 FTE threshold should result in "bunching from below", with missing mass just below the threshold, and excess mass above. The figure displays little signs of bunching from below. To provide more formal testing for size manipulation, we report in Appendix Figure 2.A4 results from McCrary tests of the presence of a discontinuity in the probability density function (pdf) of FTE size. We report the statistic from the test and its confidence interval for each year, and separately for eligible and non eligible INPS codes. In the presence of manipulation, we would expect the presence of a significant discontinuity in the pdf for eligible INPS codes, that would be more pronounced during the Recession, if access to STW is indeed valuable during Recession. The figure shows that, for both eligible and non eligible INPS codes, no statistically significant discontinuity in the pdf of FTE can be found, and that this holds for each year from 2000 to 2014. As a final exercise to assess the robustness of our results to size manipulation, we run a "doughnut" regression, where we exclude all firms with FTE between 12 and 18. Results, displayed in Table 2.2 column (1) are almost identical to our baseline results, confirming that our estimated effects are not driven by selection due to size manipulation by firms.

Beyond their FTE size, firms may be willing to manipulate their INPS code, either through their codice autorizzazione or their industry code, in order to gain eligibility to STW. In practice, while not impossible, such manipulation is complicated, and extremely rare. Appendix Figure 2.A5 shows that less than 0.6% of firms change eligibility status due to a change in their INPS code every year in our sample, with the same fraction ($\approx 0.3\%$) of firms moving from being eligible to non eligible and moving from being non eligible to being eligible. Furthermore, these fractions are extremely stable over time. These results suggest that it is highly unlikely that firms endogenously self-select into INPS codes in order to get access to CIGS.

The identifying assumption underlying our strategy is that there is no time shock that would be specific to firms just above 15 FTE *and* eligible INPS codes within 5-digit industry codes. To assess the credibility of this assumption and the robustness of our approach, we proceed in several steps.

First, we show that there is little evidence of significant differential time shocks between eligible and non eligible INPS codes within the same industry for firms just below

15 FTE. To this effect, we directly estimate differential trends across INPS codes within 5-digit industry codes using only firms with FTE below 15 and therefore non eligible to receive STW. We estimate a model of the following form on a sample restricted to firms between 5 and 15 FTE in year $t-1$.

$$Y_{igst} = \alpha_1 \cdot \left\{ \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[t \geq 2009] \right\} + \sum_k \alpha_2^k \cdot \mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[k = s] + \sum_j \sum_k \alpha_3^{jk} \cdot \mathbb{1}[j = t] \cdot \mathbb{1}[k = s] + v_{igst} \quad (2.4)$$

We report in column (2) of Table 2.2, the estimated coefficient $\alpha_1$ of the interaction for being in eligible INPS codes after the start of the Great Recession. Results for all outcomes of interest show that differential effects of the Great Recession for eligible vs non eligible INPS codes within the same industry are either not statistically significant or of very limited magnitude for firms below 15 FTE. These results confirm that within 5-digit industry, variation in CIGS eligibility across INPS codes, which is mostly a product of regulations from the Ministry of Labor in the 1970s, is quite plausibly exogenous to economic conditions today.[23]

The previous evidence suggests that, for firms below 15 FTE, there is no evidence of time shocks that would be, within 5-digit industries, specific to eligible INPS codes. But of course finding no differential trends across eligible and non-eligible INPS codes for firms below 15 employees does not preclude the possibility that such differential trends exist for firms above 15 employees. Indeed, firms below and above the 15 FTE differ in terms of the dismissal regulations they are subject to. Heterogeneity in the treatment effects of employment regulation across INPS codes may then create differential trends across INPS codes for firms above 15 employees. We assess the robustness of our results to this potential threat in two simple ways.

First we can directly assess the extent of heterogeneity in the treatment effects of employment regulation across INPS codes by running placebo specifications across non-eligible INPS codes. We restrict the sample to non eligible INPS codes only. Among these non eligible INPS codes, we randomly select a series of INPS codes, to which we attribute a placebo "eligible" status and then run the reduced-form of our baseline IV specification (2.3). We replicate this procedure 100 times and obtain bootstrapped estimates of the placebo reduced-form coefficient for the triple interaction of being a firm above the 15 FTE threshold in (placebo) eligible INPS codes after 2008. We report the mean and standard error of the distribution of these 100 bootstrapped estimates in column (4) of Table 2.2. All estimates are statistically insignificant, very close to zero, with tight standard errors, showing no evidence for heterogenous responses to the recession across INPS codes

[23] As a consequence, this means that our baseline results do not rely much on correcting for differential trends across eligible and non eligible INPS codes within industry, using firms with less than 15 FTE. This can be clearly seen from results in column (3) of Table 2.2 which reports estimates from a specification where we focus on firms between 15 and 25 FTE only, and therefore only identify the effects of STW by comparing firms in eligible vs non eligible INPS codes, before vs after the onset of the Great Recession. Results are indeed extremely similar to our baseline results.

by firms just above 15 FTE. This evidence clearly alleviates the concern that our baseline estimates may just be picking up some idiosyncratic time shocks at the INPS code level for firms above 15 FTE during the Great Recession.[24]

Second, we use the fact that for some firms, the size thresholds that determine CIGS eligibility and employment dismissal regulation do not coincide. One reason for the two thresholds not to coincide is that employment legislation regulating dismissals apply in Italy when a firm reaches 15 employees within a single establishment, *or 60 employees in the firm in Italy as a whole*. But, as explained in footnote 15 above, eligibility to CIGS, and therefore eligibility requirements, all apply at the establishment level. In Table 2.2, we use a sample of multi-establishment firms that have more than 60 employees across Italy, and compare their establishments that are around the 15 FTE, by running on this sample our baseline IV specification (2.2). Because all these establishments are already subject to dismissal regulation, the identifying variation in CIGS eligibility cannot be confounded by potential heterogeneity in the treatment effect of employment protection laws. Results reported in column (5) of Table 2.2 are qualitatively similar to our baseline estimates, with large negative effects on employment at the intensive margin and large positive effects on employment at the extensive margin, although much less precise due to the small size of this sample. In column (6) of Table 2.2, we provide additional evidence of the robustness of our results by focusing on another small group of firms in the retail sector for which the size thresholds that determine CIGS eligibility is set at 50 FTE, and therefore does not coincide with the 15 FTE size threshold for employment dismissal regulation. We create a sample of single-establishment firms in the wholesale and retail sectors that ever reach a maximum 6-month FTE size between 25 and 75. We estimate our baseline model specification (2.2), on this sample, simply replacing the dummy variable $\mathbb{1}[N_{i,t-1} > 15]$ by a dummy for reaching a maximum 6-month firm size above 50 FTE in year $t-1$. Results reported in column (6) are again very comparable to our baseline estimates, with negative effects on hours and large positive effects on headcount employment. Although point estimates are similar to our baseline estimates, standard errors are much larger due to the small size of this sample.

Taken together, this set of results provides evidence of the credibility of our identifying assumption, and of the robustness of our baseline results.

[24]This placebo procedure naturally lends itself to a simple permutation test for the estimates obtained from our baseline specification. In other words, we can use the bootstrapped placebo estimates to determine what the likelihood would be of getting our baseline estimates if "treated" INPS codes were actually allocated randomly. We report in Appendix Figure 2.A7 the p-value from such tests for the baseline estimate of each coefficient $\hat{\gamma}_1^t$ in specification (2.1), each panel corresponding to a different firm outcome. Results show that for the outcomes (intensive and extensive employment margin, wage bill, etc) where we find large statistically significant effects in our baseline specification, the probability of finding such effects "at random" is extremely small, and always below 5%. To the contrary, for the wage rate, where we find no statistically significant effect in our baseline specification, the p-value is large, which further suggests that wage rates seem to be totally unresponsive to STW.

### 2.3.4 Dynamic Effects

As explained in Section 2.2, CIGS treatment is temporary. Firms can receive STW for a maximum of 12 months over a fixed 5-year period and, in practice, both average and median duration are very close to 52 weeks. Furthermore, INPS codes and firm size, which determine access to STW, are persistent over time. As a result, a firm that is eligible based on firm size and INPS code in year $t$ is not only more likely to receive treatment in $t$, but also more likely to have received treatment in $t-1$, $t-2$, etc. Appendix Figure 2.B1 provides direct evidence of the correlation between current eligibility and past treatment by plotting the effect of the triple interaction $\mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[N_{i,t-1} > 15] * \mathbb{1}[j = t]$ on the probability to have been receiving treatment in the past 5 years.

Our baseline estimates $\widehat{\beta_{IV}}$, which use the triple interaction $\mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[N_{i,t-1} > 15] \cdot \mathbb{1}[t > 2008]$ as an instrument, are therefore identifying the total effect of exposure to STW during the Great Recession. In other words, they capture both contemporaneous effects of STW treatment and past dynamic effects of STW treatment. One may however be interested in unpacking this sequence of dynamic effects to gain further insights on the impact of STW on firms' and workers' outcomes.

To identify the the sequence of dynamic treatment effects of STW $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$, we develop a methodology similar in spirit to the recursive identification of dynamic treatment effects in Cellini, Ferreira, and Rothstein (2010). All the details of the procedure are given in Appendix 2.B.2. The main intuition is straightforward. Take all firms that are active in 2009, and define our instrument for STW access in 2009 $Z_{2009}$ as the interaction between firm size and INPS code in 2009. The difference in outcome in 2009 of eligible firms in 2009 ($Z_{2009} = 1$) versus non-eligible firms ($Z_{2009} = 0$) only reflects the contemporaneous effect of treatment ($\beta_0^{TOT}$) in 2009. This is because there is no difference in 2009 in the probability of past treatment between eligible and non-eligible firms in 2009 as clearly shown in Appendix Figure 2.B1. Because eligible firms in 2009 are not only more likely to be treated in 2009, but also to be treated in 2010, the difference in their outcome in 2010 will reflect both the 1-year lagged effect of treatment in 2009 ($\beta_1^{TOT}$) and the contemporaneous effect of treatment ($\beta_0^{TOT}$) in 2010. And so on and so forth. That is, the difference in outcome in any year $k \geq 2009$ between firms that are eligible versus non eligible in 2009 capture the dynamic Intention-To-Treat (ITT) effect from treatment in 2009 after $k$ years, allowing for potential future treatment.

Exploiting this intuition, we show in Appendix 2.B.2 that the sequence of ITT effects are identified by the coefficients for each year ($\beta_{2009}^{RF}, \beta_{2010}^{RF}$, etc.) of the reduced form relationship between the outcome and $Z_{2009}$. We also show that ITT effects have the following recursive structure as a function of TOT effects:

$$ITT_0 = \hat{\beta}_{2009}^{RF} \quad = \quad \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{2.5}$$

$$ITT_1 = \hat{\beta}_{2010}^{RF} \quad = \quad \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}, \qquad etc. \tag{2.6}$$

Using estimates of $\hat{\beta}^{RF}_{2009}, \hat{\beta}^{RF}_{2010}$, etc., and of the first stages $\widehat{\frac{dT_{2009}}{dZ_{2009}}}, \widehat{\frac{dT_{2010}}{dZ_{2009}}}, etc.$, we can identify the sequence of dynamic TOT effects $\{\hat{\beta}^{TOT}_0, \hat{\beta}^{TOT}_1, ..., \hat{\beta}^{TOT}_4\}$.

Figure 2.3 reports the dynamic effects of STW treatment on hours per employee. Results suggest that the entire employment effects of STW are on impact. At the time of treatment, log hours per employee decrease by 0.3, but this effect disappears immediately after treatment, with no significant long term impact. Appendix Figure 2.B2 shows similar patterns for other employment outcomes. Upon treatment, log headcount employment increases by 0.2, the log wage bill decreases by 0.2, log open-ended contract increase by 0.4, but all these effects dissipate instantly as treatment disappears. In the long run, the recursive identification lacks precision, as it makes standard errors become somewhat large.[25] Yet point estimates are consistently small, and close to zero, indicating no significant long term effects of treatment. This dynamic pattern of results, with short run employment effects that quickly dissipate after treatment, is confirmed by our analysis of the dynamics of outcomes at the worker level, which we now turn to.

## 2.4 Dynamic Effects and Insurance Value of STW for Workers

The analysis so far has focused on firms, and firms' outcomes. Yet, understanding how STW policies interact with workers' employment and earnings dynamics is equally key to assess the welfare consequences of such programs.

In this section, we explore two important dimensions of the relationship between workers' labor market dynamics and STW. First, we document how the outcomes of workers on STW compare to the outcomes of other workers, and to the outcomes of the same workers prior and after being on STW. The difference in outcomes informs us about the difference in marginal utility of consumption of individuals receiving vs contributing to the STW policy, which, in the spirit of the optimal tax and social insurance literature, is "sufficient" to evaluate the marginal welfare value of the transfer operated by the policy (Chetty, 2006a).

Second, we are also interested in identifying the causal effect of receiving STW treatment on the dynamics of workers' labor market outcomes. One important rationale for STW programs is that job separations may actually destroy positive surplus created in employment relationships and can have significant short run as well as long term negative consequences for workers (Yagan, 2017). Many different mechanisms may participate in creating these negative effects of separations, from various labor market frictions, to specific human capital accumulation, experience effects or other scarring/discrimination effects. A prevalent idea in the public debate is that STW, by reducing the incidence of separations, may therefore have beneficial dynamic "insurance" value to workers by preserving the surplus created by the employment relationship. By

[25] We report bootstrapped standard errors for the TOT effects. Because of the recursive nature of identification, standard errors using the Delta-method equally suffer from this lack of precision.

estimating the causal effect of STW on the dynamics of workers' outcomes, one can directly assess the validity of these claims.

### 2.4.1 Event Studies

We start by documenting, using event studies, the dynamics of workers' outcomes around STW treatment. We create a panel of the labor market histories of all employees of firms active and with FTE firm size $\in$ $(5;25]$ at any point between 2000 and 2015. An event year is defined as the first year a firm experiences a STW spell. Treated individuals are individuals who are employed in the firm at the start of the first STW spell. We run event study regressions on this sample of treated individuals, controlling for individual and calendar year fixed effects and report in Figure 2.4 estimates for three outcomes, the probability of being employed, the total number of hours worked per year (unconditional on employment), and total earnings plus all social transfers including STW. All estimates are relative to event year -1, and scaled by the average level of the outcome among the treated in year -1. In Figure 2.4, we also report results for two comparison groups of similar workers not treated by STW. The first comparison group consists of workers with similar characteristics as treated workers pre-treatment, but who cannot access STW as they work in firms non-eligible to CIGS based on FTE size and eligibility. To create this group, we match each treated worker, using Mahalanobis nearest-neighbour matching without replacement, with a worker from the sample of non-eligible firms with FTE size $\in$ $(5;25]$ in event year -1. Matching is based on gender, age, job characteristics at event time *t-1*, employment status, annual weeks worked, earnings and firm size at *t-1*, *t-2*, *t-3* and *t-4*, and main industry at *t-1*. The second comparison group consists of workers in non-eligible firms in event year -1 who experience a mass layoff in event time 0, and is created following a similar nearest-neighbor-matching strategy using the same variables.

Results of the event study estimates for all three groups and all three outcomes are reported in Figure 2.4 and reveal interesting dynamic patterns. First, there seems to be no differential trends pre-event across the treated workers and our comparison groups, signalling little anticipation of STW treatment in terms of labor market trajectories. Second, treated STW workers experience, on impact, a sharp reduction of roughly 30% of their worked hours, a reduction very close to our IV estimate of the effects of STW on hours using firm outcomes. This sharp drop in hours translates into a milder drop of 18% in total earnings and transfers, because of the high replacement of the STW subsidy.

When comparing the labor market outcomes of treated workers to our comparison groups during the treatment period, it is interesting to note that workers experiencing STW treatment maintain a probability of being employed similar to workers in non-eligible firms, and much larger than workers in the layoff comparison group. This is indicative that STW has indeed a positive effect on employment in the short run, as shown in the previous section. But despite having a similar probability of being employed, treated workers experience a reduction in hours that make their total employment, measured by total annual hours worked, much lower ($\approx$ 25 percentage points) than workers in non-eligible firms, and only 10 to 15 percentage point larger than laid-off workers. But the high replacement rate of STW makes their total income from earnings and transfers significantly larger ($\approx$ 18%) than laid-off workers.

Labor market dynamics after treatment are also informative. After treatment is over, treated workers experience a sharp drop in labor market outcomes, confirming the reversal also observed for firms' outcomes. First, there is a sharp drop in the probability of employment and in total hours worked in the next two years following treatment.[26] There is also a significant drop in total earnings and transfers of treated workers, which, 2 years after treatment, only represent 60% of their pre-treatment level. In comparison to non-eligible workers, treated workers fare much worse in terms of all labor market outcomes in the medium and long run. But even more strikingly, two to three years after treatment, labor market outcomes of treated workers are no longer significantly different from those of non-eligible workers who were laid-off at time 0. This suggests that, while STW offers some short run insurance, in the medium run, being laid-off or being put on STW are somewhat equivalent in terms of labor market outcomes.

### 2.4.2 Identifying Causal Dynamic Effects on Workers

We want to understand to what extent the interesting dynamic patterns from the previous event studies reveal the deeper causal dynamic impact of STW treatment. Endogeneity concerns prevent interpreting the event study estimates on the treated as the causal dynamic impact of short time work. The incidence and timing of CIGS treatment across firms are indeed not random and workers within these firms may differ from other workers along various characteristics affecting their labor market dynamics. Two things can be done to tackle this issue. First, we can use event studies estimates from our comparison groups to get bounds on counterfactuals, and therefore obtain bounds on the dynamic treatment effects of STW. Second, we can also get a causal estimate of the dynamic effect of STW on workers by implementing a version of our IV recursive identification method used in Section 2.3.4, but focusing on workers outcomes instead of firms. All the details and results of these two approaches are given in Appendix 2.B.2.

Results, displayed in Appendix Figure 2.B3, confirm that STW has a positive effect on workers outcomes during treatment and therefore provides short term insurance to workers in firms exposed to shocks. Yet, these effects entirely disappear after treatment so that STW provides no longer term insurance to workers. In other words, there was no long term beneficial effect of keeping treated workers in firms treated by CIGS during

[26] The decrease in total hours worked between event year 0 and 1 is a little less severe (15 percentage points) than that of the probability of employment (around 20 percentage points), and reflects the fact that hours conditional on employment increase post treatment, a result similar to what was observed in firm level outcomes.

the Great Recession in Italy. This also suggests that there is limited scope for experience effects in the CIGS context, which confirms a stream of evidence on the absence of significant returns to experience for workers treated by active labor market programs (Card and Hyslop, 2005). To understand why STW provides no long term insurance to workers, the next section investigates patterns of selection of firms into STW treatment, and its implications for reallocation of workers in the labor market.

## 2.5 Selection and Reallocation effects

### 2.5.1 Selection into STW

Can the selection of firms into STW take-up explain the limited insurance value of STW treatment for workers? To answer this, we analyze selection patterns and heterogeneity in treatment effects along two important dimensions: firms' productivity levels pre-recession, and firms' likelihood of mass separations during the recession.

We start by ranking firms in quartiles of the distribution of their average yearly productivity over the 2007-2008 period. We use two different measures: labor productivity, defined as firm value-added divided by total number of hours worked in the firm, and TFP, defined as in Section 2.3.2 above. We then run the first-stage regression (2.3) separately for firms in each quartile of the distribution to investigate how pre-recession productivity levels differentially affect take-up of STW. Results of the estimated coefficients $\hat{\kappa}_1$, reported in Figure 2.5 Panel A, indicate that firms that had very low productivity prior to the recession are significantly more likely to take up STW conditional on eligibility. Firms in the bottom quartile of pre-recession TFP are almost 7 percentage point more likely to take-up STW than firms in the top quartile, conditional on eligibility. Do these firms benefit more from access to STW? In Panels B and C of Figure 2.5 we report estimates of $\hat{\beta}_{IV}$, from the 2SLS-IV model (2.2), again estimated separately for each quartile of the pre-recession productivity distribution. Panel B shows that low productivity firms tend to reduce hours more in response to STW treatment, but Panel C shows that this comes with limited total effects on employment. To the contrary, firms that were experiencing high productivity levels pre-recession seem to exhibit a much larger positive treatment effect of STW treatment on employment.

Turning to the targeting efficiency of STW, we also investigate whether firms that have a higher likelihood to separate workers are more likely to take-up STW. To investigate this effect, we start by building a prediction model of the probability of mass layoff during the recession using a rich set of regressors including balance-sheet information and Bartik-style instruments.[27] We estimate this model using LASSO on the sample of

[27] A mass layoff is a layoff of at least 5 workers over a time period of 120 days. We define an indicator for mass layoff taking value 1 in each year in which we observe at least 5 layoffs occurring over a 4-month period. The regressors included in the prediction model are: a Bartik-style index for employment shocks at the 2-digit industry level and provincial level, labor productivity, a Whited-Wu index of credit constraints, net revenues per employee, profits per employee, liquidity over total assets, cash flows over total assets,

non-eligible firms with more than 15 FTE. We then use the model to predict the incidence of mass layoff during the recession among eligible firms, and rank firms in quartiles of the distribution of the prediction score. Finally, we replicate the selection and heterogeneity analysis of Figure 2.5 across firms in the different quartiles of the mass layoff score. Results in Panel A of Figure 2.5 show that firms that would have been highly likely to layoff workers in the absence of STW are 80% more likely to select into treatment, conditional on eligibility. Interestingly, low risk firms still do take up significantly. Panel B indicates that firms with higher mass layoff risk scores reduce hours more when treated by STW but Panel C shows no significant heterogeneity in treatment effects on employment.

Overall, results of Figure 2.5 contribute to explaining the dynamic patterns of the treatment effects of STW observed for firms and workers. Firms taking up STW exhibit low productivity to begin with, and are likely to layoff workers in a downturn. When the aggregate shock to the economy is quite persistent, as was the case during the Great Recession in Italy, STW can only be a short term fix, which explains why the positive employment gains of STW immediately disappear after treatment. The results suggest that STW may be more effective at preserving employment in high productivity firms experiencing temporary shocks. But in practice, STW subsidizes mostly firms that have permanently lower levels of productivity, with more limited effects on employment.

### 2.5.2 Reallocation effects

It is often argued that labor market programs hinder the "cleansing" effect of recessions. By keeping workers in low productivity firms, which are much more likely to take up the program, STW is indeed susceptible of affecting the reallocation of workers towards more productive employment relationships. To investigate such claims, we leverage the rich spatial variation available in Italy across more than 600 local labor markets (LLM) defined by the Italian statistical agency (ISTAT) and estimate how an increase in the fraction of workers treated by STW in a LLM affects employment outcomes of non-treated firms.[28] In each LLM, we define the fraction of treated workers as the total numbers of workers on STW divided by the total number of employed workers observed from INPS records.[29] Appendix Figure 2.C1 shows the large amount of variation in the intensity of STW treatment across LLM during the recession. Importantly, this spatial variation arises mostly within rather than between Italian regions. Yet, variation in the intensity of STW treatment across LLM will be of course endogenous to local economic and labor market conditions during the Great Recession, which might affect employment outcomes of non-treated firms. To account for this threat, we instrument the fraction of workers treated by STW during the recession by the average yearly fraction of eligible workers in the LLM

tangible and intangible assets over total assets. All regressors enter the model in levels, one-year lags and first differences.

[28] We use the ISTAT 2011 classification of municipalities into 611 local labor markets.

[29] For employed workers, we use information about the address of the place of work available in the INPS individual records.

based on the interaction between firm size and INPS codes in the pre-recession period, in the years 2005 to 2008. We identify the reallocation effects of STW on non-treated firms at the LLM level based on the following model:

$$\underset{t,t'}{\Delta} Y_{ij} \quad = \quad \alpha + \beta_{IV}^{R} \underset{t,t'}{\Delta} T_j + X_j' \gamma_0 + W_i' \gamma_1 + \varepsilon_{ij} \tag{2.7}$$

The model is estimated on the sample of all firms $i$ that are non-eligible to STW based on their characteristics in 2008. $\underset{t,t'}{\Delta} Y_{ij}$ are long differences in average yearly employment outcomes of firm $i$ in LLM $j$ between the recession period $t'$ and the pre-recession period $t$.[30] $\underset{t,t'}{\Delta} T_j$ is the long difference in the average yearly fraction of workers treated by STW in LLM $j$ between period $t$ and $t'$. The long difference in the fraction of workers treated by STW in LLM $j$ is instrumented by the average yearly fraction $Z_j$ of workers of LLM $j$ that are eligible to STW during the pre-recession period based on the interaction between their firm size and INPS code in the pre-recession period. We control for a rich vector $W_i$ of firm characteristics, correlated with CIGS take-up, and likely to affect firm employment outcomes during the recession. The vector is composed of 5-digit industry fixed effects and codice autorizzazione fixed-effects, as well as bins of firm size in 2008. We also control for LLM characteristics that could be correlated with the fraction of treated workers and likely to affect employment outcomes during the recession, such as the industry and firm size composition of the LLM and the initial unemployment rate in the LLM prior to the recession. Identification therefore comes from comparing LLM with similar characteristics, including firm size composition and industry composition, but with different allocations of workers within firm size times INPS codes bins during the pre-recession period. We propose various tests for the validity of our exclusion restriction below. Standard errors are clustered at the LLM level. Appendix Figure 2.C2 provides evidence of the strong first-stage relationship between the fraction of eligible workers in a LLM during the pre-recession years 2005-2008 and the fraction of workers on STW during the recession conditional on controls for firm and LLM characteristics.

Panel A of Figure 2.6 provides striking evidence of the presence of significant reallocation effects of STW within LLMs. The graph is a bin-scatter plot of the reduced-form of the IV model (2.7), that is, the relationship between the instrument $Z$ (the fraction of eligible workers in the pre-recession period in a LLM based on the interaction of firm size and INPS codes) and the long difference in log employment of non-eligible firms. The reduced-form relationship is strongly negative, indicating that in LLMs with a larger fraction of eligible workers in the pre-recession period, employment growth of non-eligible firms was significantly worse during the recession. The corresponding IV estimate is $\beta_{IV}^{R}$=-0.94 (0.22), which means that a 1 percentage point increase in the fraction of treated

[30] In our baseline estimation of model (2.7), we compare the recession years 2010-2013 to the pre-recession years 2005 to 2008. Results are robust to the precise definition of the pre and post recession periods.

workers in a LLM reduces employment of non-eligible firms by 0.94%. Another way of assessing the magnitude of these spillover effects on non-treated firms is to ask the following question: what is the impact of preserving one employment relationship in a firm treated by STW on the number of jobs in non-treated firms. Given our estimates of the effect of STW treatment on employment of treated firms, our $\beta_{IV}^{R}$ estimates imply that for one job "saved" by STW in a treated firm, employment in non-treated firms decreases by 0.03 job. Table 2.3 summarizes the results, and also shows that the employment effects are driven by a significant decline in inflows in non-eligible firms (measured as the number of new hires) as the fraction of workers treated by STW increases in the LLM.

By keeping more workers in low productivity firms, and by reducing the number of workers reallocating to non-treated firms, which have higher productivity than treated firms on average, STW is likely to affect overall productivity within the LLM. We explore this possibility by computing LLM level measures of TFP and running IV model similar to (2.7) with LLM TFP long differences as an outcome.[31] The IV results, displayed in Table 2.3, confirm that STW has a significant negative impact on overall TFP within LLM, with a one percentage increase in the fraction of workers treated by STW translating into a roughly 2% decrease in TFP.

One may worry about the validity of the exclusion restriction underpinning the IV estimates. This restriction may be violated if the fraction of workers eligible to CIGS in the pre-recession period based on the interaction of firm size and INPS code is correlated with other unobserved characteristics of the LLM affecting employment and TFP growth. To assess the credibility of our strategy we run placebo models similar to (2.7) where we now compare long differences between 2000-2005 and 2005-2008, and use as a placebo instrument the fraction of eligible workers in the LLM based on the interaction between firm size and INPS codes in the 2000-2005 period. Because there is no take-up of CIGS during the 2005-2008 period, there is no first stage in this model, so that our placebo instrument will only pick up an effect if the exclusion restriction does not hold, and the instrument is correlated with other determinants of employment and TFP growth within a LLM. The reduced-form relationship of the placebo model for employment growth of non-eligible firms in the LLM are reported in Panel B Figure 2.6. We clearly see no significant relationship between the placebo instrument and the outcomes, which provides comforting evidence for the validity of our exclusion restriction. We report similar placebo models for TFP growth in Table 2.3 and find no significant relationship between our instrument and TFP growth in the LLM in the pre-recession period.

Overall, by leveraging the rich spatial variation across LLM in Italy, and the variation in STW treatment created by the interaction of firm size and INPS codes, these results provide compelling evidence that STW has significant equilibrium effects within labor

[31] We define TFP as TFP $= \text{VA}/(L^{\alpha}K^{\beta})$, but we now aggregate all variables (VA, L and K) at the LLM level.

markets. STW creates significant spillover effects on non-treated firms through reallocation of workers. Non-treated firms are less able to grow and hire new workers as a result. And by tilting the allocation of workers towards less productive firms, STW has a significant negative impact on TFP growth in the labor market.

## 2.6 Welfare Implications for STW Programs

To understand the welfare implications of our empirical results, this section develops a simple search and matching framework of labor market equilibrium, allowing for labor adjustments both at the intensive and extensive margin. The model serves three important purposes. First, it offers a general tractable model in which one can rationalize the empirical evidence of Sections (2.3) to (2.5). In particular, we clarify the conditions under which general search and matching models can generate the observed employment responses to STW treatment. Second, we derive a general formula for the optimal subsidy rate of STW policies and clarify the welfare tradeoffs inherent to STW policies. Finally, we calibrate a version of the model based on our reduced-form evidence to provide estimates of the welfare effects of STW and conduct counterfactual policy analysis.

### 2.6.1 A General Model with Intensive and Extensive Labor Adjustments

We consider a unit mass of risk averse workers in a frictional labor market where firms are exposed to both idiosyncratic and aggregate productivity shocks. For simplicity, we assume that for each level of the aggregate shock, there are two levels of idiosyncratic productivity (high $\epsilon_H$ and low $\epsilon_L$) and we denote by $\rho$ the (endogenous) fraction of high productivity employment in stationary equilibrium.

In each period $t$, $u_t$ unemployed workers meet firms with a vacancy at a rate described by a constant returns to scale matching technology function $M(u_t, v_t)$, increasing and concave in both arguments. We define labor market tightness $\theta_t \equiv \frac{v_t}{u_t}$ as the ratio of vacancies to unemployment, which is, given $M$, a sufficient statistics for both the vacancy filling probability $q(\theta_t)$ and the job finding probability $\phi(\theta_t)$. Each period, a fraction $\delta$ of existing employment relationships is destroyed exogenously.

We assume random matching between workers and firms irrespective of their productivity, that is, search is not directed across separate search markets for high and low productivity firms. While some papers have explored STW in the context of directed search models, there are a few important reasons why sticking to the more general case of random matching might be preferable.[32] First, random matching proves critical in generating transparently the spillover effects that we observe in the data. Second, a key feature of directed search models is that equilibrium employment will be socially efficient absent STW. Consequently, there is no room for correcting potentially inefficient employment levels over the business cycle, which makes one of the main argument in

[32] See Cahuc, Kramarz, and Nevoux (2018) for a static version of a directed search model with STW.

favor of the existence of STW irrelevant. Finally, a corollary is that in directed search models, ex-ante utility is always equalized across workers searching for high and low productivity employment contracts. Which means that STW transfers towards low productivity employment contracts have no insurance value. This is because the ability to direct search across search markets for low or high productivity employment contracts already provides insurance for workers against firms' idiosyncratic shocks. With random matching, workers cannot insure themselves against the risk of being matched with a low productivity firm, which provides an additional insurance argument in favor of STW subsidies.

**Workers** Workers are identical. They value consumption and have disutility in hours worked, according to a general utility function $u(c,h)$, $u'_c > 0, u'_h < 0$. Workers are risk-averse in consumption, $u''_c < 0$ and discount the future at rate $\beta$. There is no storage technology, agents consume all they earn every period. Workers therefore value insurance against income fluctuations provided by the government, which takes two forms. First, unemployment insurance benefits $b$ (extensive margin insurance) are given to unemployed workers. Second, intensive margin insurance is provided in the form of a STW subsidy of rate $\tau$ given against earnings losses for hours reductions below a threshold level $\bar{h}$ for workers in low productivity firms. The total amount of STW benefits for a worker in the program is therefore $b^{STW} = \tau w(\bar{h} - h)$. Both UI and STW benefits are funded by a lump sum tax $t$ levied on all workers.

The value function of a worker when unemployed, $W^u$ is:

$$W^u = u(b,0) + \beta(\phi W^e + (1-\phi)W^u) \tag{2.8}$$

The value function of a worker when employed by firm of productivity $\epsilon_k \in \{\epsilon_H, \epsilon_L\}$, is $W^e_k$

$$W^e_k = u(c_k, h_k) + \beta(\delta W^u + (1-\delta)W^e_k) \tag{2.9}$$

Workers can endogenously quit their job every period. They will choose not to do so whenever the employment relationship entails a positive surplus, which means that the continuation value of being employed in a firm of productivity $\epsilon_k$ is at least equal to the value of being unemployed $W^e_k - W^u \geq 0$. The zero surplus condition $W^e_k - W^u = 0$ implicitly defines the reservation values of wage and hours that a worker is willing to accept for any employment relationship. Note that these reservations values will be functions of the UI benefits and STW subsidy. In particular, the lower bound on hours that workers are willing to accept decreases with STW, *ceteris paribus*. In other words, STW relaxes the constraint on offering lower hours contracts.

**Firms** Firms produce an homogeneous consumption good using labor inputs according to the technology $\epsilon_t F(h_t, n_t)$. We keep the production function general and allow the marginal product of labor to potentially differ at the intensive (hours worked per employee $h$) vs extensive margin (number of employees $n$). This captures the simple fact that the return to adjusting labor may differ when increasing hours per employee or when hiring a new employee.

Firms determine every period the number of vacancies to be posted $v_t$ to maximize profits:

$$\Pi(\epsilon_t, n_{t-1}) = \max_{v_t}\{\epsilon_t F(h_t, n_t) - wh_t n_t - cv_t + \beta\mathbb{E}_t[\Pi(\epsilon_{t+1}, n_t)]\} \tag{2.10}$$

subject to the law of motion of employment:

$$n_t = (1-\delta)\cdot n_{t-1} + q(\theta_t)\cdot v_t \tag{2.11}$$

The first order condition of profit maximization implicitly determines the demand for employment $n_t = n(\theta_t, h_t, w)$ of the firm.

Workers and firms negotiate hours and wages to split the surplus created by realized matches, which translates into an hours schedule and a wage schedule. Note that there are multiple hours and wage schedules that are compatible with equilibrium. At this point, we allow for general hours and wage schedules: $h(w, \theta, \epsilon, n, b, \tau, t)$ and $w(h, \theta, \epsilon, n, b, \tau, t)$.

**Hours vs Employment Responses & STW policy** In Sections 2.3 to 2.5, we showed three important sets of empirical results regarding the hour and employment responses of firms to STW policy. First, we showed that hours decrease strongly with STW. Second, we showed that this hours decrease was met by a large positive employment response. Finally, we showed that low productivity firms are more likely to take up STW.

In Appendix 2.D.2, we characterize the hours schedule and the firm's hours and employment responses to variation in productivity and variation in STW generosity, *conditional on the wage schedule*. This characterization of hours and employment responses to STW enables to transparently understand the conditions under which our general model delivers the observed reduced form results of Sections 2.3 to 2.5. This exercise highlights the critical role of rigid wage schedules in amplifying the employment responses to STW. In particular, we show that hours decrease strongly and employment increases strongly with STW, and low productivity firms select more into STW, when four conditions are met: (i) the marginal utility of employment for workers is strongly decreasing in hours; (ii) the wage schedule is relatively rigid; (iii) technology is relatively linear in employment $n$ but (iv) relatively concave in hours $h$.

The intuition for (i) is quite straightforward. When the return to an additional hours in terms of utility decreases strongly, which can be due to large risk aversion in consumption or a very convex disutility of work, STW subsidies reduce more drastically workers' reservation hours, making the hours schedule more responsive to the introduction of

STW. Conditions (ii), (iii) and (iv) also have an intuitive interpretation. When productivity goes down, firms want to reduce more their labor inputs, especially when wages are rigid. The more concave technology is in hours relative to employment, the more firms want to "hoard labor" and reduce hours relative to employment. Without STW, outside option of workers reduces the extent to which firms can reduce hours, and they will reduce employment instead. With STW, firms can offer lower hours, and increase employment instead. Rigid wages magnify again this employment response: when hours decrease in response to STW, the net profit of a filled job increases more when wages are more rigid, driving a larger positive employment response.

Interestingly, we showed clear empirical evidence that wages exhibit a significant level of rigidity in our context. This means that condition (ii) is likely to be met, and helps explaining why we find such strong and significant effects of STW on firms' hours and employment.

**Equilibrium & Spillover Effects** A steady state equilibrium consists in a set of: (i) hours schedules $h$ and wage schedules $w$ that split the surplus in high and in low productivity firms subject to the incentive constraint that $W_k^e - W^u \geq 0$; (ii) labor demand functions $n^d$ in high and in low productivity firms that maximizes firms' profits and (iii) a labor market tightness $\theta$ that clears the labor market subject to the steady state equality of flows in and out of employment.

To understand the logic of the equilibrium effects of STW in the labor market, we borrow the equilibrium representation of Michaillat (2012). This representation allows for a transparent representation of the effects of various labor market policies on equilibrium (e.g. Landais, Michaillat, and Saez, 2018a) and of the mechanisms underlying equilibrium spillover effects across workers or firms (Lalive, Landais, and Zweimüller, 2015).

In this representation, the steady state equality of flows in and out of employment characterizes a labor supply $n^s(\theta, \delta)$, which is an increasing function of $\theta$ in the $\{n, \theta\}$ space. The profit maximization of firms determines a labor demand demand $n^d(\theta)$, which will be a decreasing function of $\theta$ when the marginal product of $n$ is decreasing, and horizontal otherwise (i.e. if technology is linear in $n$). With random matching, aggregate labor demand is simply the weighted sum of demands of high and low productivity firms. Equilibrium tightness and equilibrium employment are determined at the intersection of aggregate demand and supply. When STW is introduced, labor demand of low productivity firms increases, especially so when tightness is low (and hiring is therefore cheap). This in turn increases aggregate demand, and equilibrium tightness. This increase in equilibrium tightness is the force driving our observed spillover effects in the data. It makes hiring more costly for all firms, and therefore reduces employment of firms non treated by STW. This equilibrium mechanism captures the negative reallocation effects of

STW which distorts employment towards low productivity firms rather than high productivity firms. Again, this effect will be stronger the more horizontal labor demands are (that is, the more linear technology is in $n$). A graphical illustration of these equilibrium mechanisms, using the calibrated version of our model, is offered in Appendix Figure 2.D1.

### 2.6.2 Optimal STW Subsidy

We now use the general model presented above to characterize the optimal STW subsidy rate and clarify the welfare tradeoffs implied in STW policies.

**Planner's problem** The planner maximizes a utilitarian social welfare function $\Gamma(W^e_H, W^e_L, W^u, n, \rho) = n \cdot [\rho \cdot W^e_H + (1-\rho) \cdot W^e_L] + (1-n) \cdot W^u$, using government policies $\mathbb{T} = \{\tau, b, t\}$ where $\tau$ is the STW subsidy rate, $b$ is the UI benefit level given to the unemployed, and $t$ is a lump sum tax financing both STW and UI. The planner maximizes social welfare subject to three set of constraints. First, the government budget needs to balanced. Second the government needs to account for the optimal behavior of workers and firms that are function of the policy instruments. Finally, the labor market must be in equilibrium. We further assume that profits are fully taxed and redistributed lump sum to all workers.

**Proposition 2.** *Assuming differentiability, an interior optimal subsidy rate $\tau$ balances the transfer value of the policy with the fiscal externality, the employment externality and the hour externality of the policy and needs to satisfy:*

$$1 + \underbrace{\left\{ \varepsilon_{n,\tau} \frac{(1-\rho)b^{STW} - b}{b^{STW}} - \varepsilon_{h_L,\tau} \frac{h_L}{\bar{h} - h_L} \right\}}_{\text{Fiscal Externalities}} = \overbrace{\mathbb{G}_L}^{\text{Value of Transfer}} + \varepsilon_{\theta,\tau} \frac{\theta}{E} \cdot \overbrace{\left\{ \Phi'(\theta)\Delta + q'(\theta)\mathcal{C} \right\}}^{\text{Employment Externalities}}$$
$$+ \varepsilon_{h_L,\tau} \frac{n_L \cdot h_L}{E} \cdot \underbrace{\left\{ [F^h_L - w] + [w(1-\tau) - MRS^L_{c,h}] \cdot \mathbb{G}_L \right\}}_{\text{Hours Externalities}}, \tag{2.12}$$

*where $\varepsilon_{Y,X} = \frac{dY}{dX}\frac{X}{Y}$ denote the elasticity of $Y$ w.r.t. $X$. $n_L = (1-\rho)n$ is employment in low productivity firms and $h_L$ is contracted hours in these firms. $F^h_L = \frac{\varepsilon_L}{n_L} \cdot \frac{\partial F(h_L, n_L)}{\partial h}$ is the marginal product of an increase in hours in low productivity firms. $E = \tau \cdot n_L \cdot w \cdot (1-h_L)$ is total expenditures on the STW policy. $\mathbb{G}_L$ is the social welfare weight on workers currently in low productivity firms. $\Delta$ is the weighted wedge in utility between being employed and unemployed and $\mathcal{C} = c \cdot v$ is total recruiting costs. $MRS^L_{c,h} = \frac{u'_c(c_L, h_L)}{u'_h(c_L, h_L)}$ is the marginal rate of substitution between consumption and hours for workers in low productivity firms.*

**Proof.** *See Appendix 2.E.*■

The first two terms correspond to the traditional public finance formula for optimal tax/transfer, which simply states that, absent pre-existing distortions, the optimal transfer balances the value of the transfer with its fiscal externality at the margin. In our context, two additional sources of potential inefficiencies arise, that the planner needs to account for when setting the subsidy $\tau$. First labor market frictions do not ensure that employment is at a socially efficient level to start with. Second, there is no market for hours.

To better understand and interpret the formula, we now turn to each of its terms separately, and provide a local calibration of the welfare effects of a marginal change in the subsidy $\tau$ from its current level, based on our reduced-form empirical estimates of Sections 2.3 to 2.5.[33]

**Value of STW Transfer** The social value of transferring one euro from all taxpayers to workers currently in low productivity firms is captured by $\mathbb{G}_L = \frac{\Omega_L}{n_L}\frac{u'_c(c_L,h_L)}{\mu}$ where $\mu$ is the Lagrange multiplier for the budget constraint in the planner's problem, which captures the social value of one euro redistributed lump sum to all workers. $\Omega_L$ is a weight that captures the expected relative time that workers currently in a low productivity firm will spend in low vs high productivity firms or in unemployment.[34] The value of STW transfers depends critically on the marginal utility of consumption of treated workers ($u'_c(c_L, h_L)$) relative to that of the whole population of workers $\mu$. Figure 2.4 Panel C shows that treated workers have total earnings and transfers that are significantly below ($\approx$ 18% lower) that of their matched non-treated workers. This indicates that their marginal utility of consumption might be significantly higher than that of the average worker. In Appendix 2.E.2, using these estimates and a coefficient of relative risk aversion of 2.5, we calibrate $\mathbb{G}_L$ and find that the value of transfer is relatively large: $\mathbb{G}_L \approx 1.45$.

**Fiscal externality** Transferring one euro to workers in STW programs costs more than 1 euro, because workers and firms do not internalize the effect of their change in behaviors on the government budget constraint, which creates a fiscal externality. This fiscal externality is captured by two terms. The first term, $\varepsilon_{n,\tau}\frac{(1-\rho)b^{STW}-b}{b^{STW}}$ captures the cost to the government of employment responses. When employment increases in response to an increase in $\tau$, ($\varepsilon_{n,\tau} > 0$), this moves individuals out of unemployment, which saves the government on the unemployment benefits $b$ they collected. But it also increases the number of individuals receiving STW benefits $b^{STW}$. The relative generosity of STW vs UI benefits and the fraction of workers in low productivity firms therefore determine the sign of this fiscal externality. When UI benefits are very generous and the fraction of workers in low productivity firms receiving STW is small, the employment responses may have a positive effect on the government's budget. The second term captures the

[33]All the details of the local calibration are provided in Appendix 2.E.2.

[34]If on average more time is spent in, say, the low productivity state, then the social planner places greater weight on welfare in this state. The precise definition of the weights is provided in Appendix 2.E.

hours response to the program: $-\varepsilon_{h_L,\tau}\frac{h_L}{\bar{h}-h_L}$. Because of the large negative responses of hours to the policy $\varepsilon_{h_L,\tau} \approx -0.4$, this term increases the fiscal externality of STW significantly. Using our estimates, we show in Appendix 2.E.2, that the total estimated fiscal externality is quite large, and equal to 0.37. Interestingly, it is quite close to the calibrated value of the transfer. This means that the welfare value of a marginal increase in the subsidy rate above its current level would be quite small, unless the hours and employment externality terms are very large. To investigate this, we now turn to these two extra externality terms.

**Employment Externalities** Our empirical evidence shows that STW affects equilibrium employment, creating employment spillovers on untreated firms. As shown in Landais, Michaillat, and Saez (2018a), in frictional labor markets, such equilibrium effects have potential welfare consequences, and the design of labor market policies need to account for them. The reason is that in frictional labor market, market tightness $\theta$, and, as a consequence equilibrium employment, need not be at their socially efficient level, which is defined by the Hosios condition (Hosios, 1990). Here, the Hosios condition will hold when the term $\left\{\Phi'(\theta)\Delta + q'(\theta)\mathcal{C}\right\}$ is equal to zero. This term captures the competing search externalities created by a change in equilibrium tightness. On the one hand, an increase in $\theta$ will increase the probability for workers to find jobs ($\Phi'(\theta) > 0$), and being employed gives them an increase in utility equal to the wedge between the average utility of being employed vs unemployed $\Delta$. On the other hand, an increase in $\theta$ will decrease the probability that vacancies are matched ($q'(\theta) < 0$), increasing the overall cost $\mathcal{C}$ of replacing workers for firms.[35] In specific models (directed search for instance), these two opposite externalities may exactly cancel out. But in general, there is no particular reason for the Hosios condition to hold. If we believe that equilibrium employment is suboptimally low in Recessions, then any policy increasing labor market tightness like STW may have positive employment externalities that are socially desirable.

While it is tricky to calibrate the Hosios term, in Appendix 2.E.2 we use our reduced-form estimates to provide evidence that (i) the curvature of the matching function is large, and that (ii) the utility wedge between employment and unemployment is large. These two facts indicate that the employment externality is likely to be positive, suggesting that employment is indeed suboptimally low during recessions.

**Hours Externalities** Incorporating the intensive margin of hours in the model has important welfare implications as the missing market for hours creates an additional source of externalities that the planner needs to account for. While hours and wages are set to

[35]Note that $\Delta$ will be larger in the presence of specific human capital, or experience effects. But the cost of replacing a worker with a similarly productive one $\mathcal{C}$, will also be larger when it is hard to find similar workers due to specific human capital, experience effects. It is therefore unclear how the presence of experience effects or specific human capital affects the socially efficient level of tightness and the optimal level of the subsidy $\tau$.

split the surplus between workers and firms, there is no reason for hours to be set at the socially efficient level. As is clear from our formula, hours are at the first best optimum when the marginal rate of substitution $MRS^L_{c,h}$ is equal to the marginal rate of transformation (the marginal product of an hour $F^h_L$) and equal to the wage rate $w$. In some models, such as directed search, hours will be optimally set at the first best. But this is not always the case in our more general framework. If equilibrium hours deviate from this level, the large negative effect of STW on equilibrium hours ($\varepsilon_{h_L,\tau} < 0$) entails welfare effects.

As we discuss in Appendix 2.E.2, signing and calibrating the hours externality term remains tricky in practice. On the one hand, the large employment responses to the reduction in hours in firms treated by STW indicates that $[F^h_L - w] < 0$. On the other hand, there is ample evidence that the fraction of workers reporting that they are willing to work more hours increases drastically during recessions (e.g. Canon, Kudlyak, and Reed, 2014). This would indicate that $w(1-\tau) - MRS^L_{c,h} \geq 0$. In which case decreasing equilibrium hours has a negative externality on workers.[36]

### 2.6.3 Calibration and Counterfactual Policy Analysis

The previous characterization of the optimal STW subsidy is useful to clarify the tradeoffs involved in STW policies in the general class of search-and-matching models we presented, and get a sense of the welfare consequences of local deviations from the existing policy. We now turn to a structural calibration of our model. While this calibration comes at the cost of putting more assumptions on the structure of the model, it delivers the additional benefits of enabling the exact computation of the externality terms in formula (2.12), which can be hard to measure empirically. More importantly, it allows the counterfactual explorations of non-local policy changes, such as removing STW.

When specifying the model for the purpose of calibration, we make a series of assumptions. In particular, we assume that in low productivity firms, all the bargaining power is on the firm's side, so that all the surplus goes to the firms and workers are kept at their outside option. Besides greatly simplifying the computation of the hours schedule, a useful by-product of this modelling feature is that it generates quite large variations in bargained hours in response to STW. For wages, we assume that they are a somewhat rigid function of productivity, and, following our empirical evidence, that they do not respond to the STW policy.[37]

All further details on functional form specifications and parameter calibrations are given in Appendix 2.F. Importantly, we explain in this section how our reduced-form empirical evidence, using quasi-experimental variation, allows us to calibrate most of the key parameters of the model. In particular, parameters of the demand function can be identified by the reduced form evidence of the hours and employment responses of firms

[36] This effect on welfare is weighted by the social marginal welfare weight of workers in low productivity firms experiencing this decline.

[37] As in Hall (2005) and Landais, Michaillat, and Saez (2018b), we assume a wage schedule of the following form $w(\epsilon) = w_s \epsilon^{w_a}$, with $w_a < 1$.

to STW treatment. Second, our reduced form evidence on spillover effects identifies key parameters of the matching function. Final parameters of model, that cannot be identified from quasi-experimental variation, nor directly calibrated from external sources, are estimated using GMM to match a set of key moments of firms above 15 FTE eligible to STW during the Great Recession. In effect, our calibration relies on the thought experiment that we have a version of the Italian economy where all firms correspond to firms above 15 FTE, and are eligible to STW. Furthermore, we treat the overall period 2008-2014 of the Great Recession as a steady state equilibrium.

In Figure 2.7, we display results of a counterfactual analysis of this steady state equilibrium during the recession, for various levels of the STW subsidy $\tau$. Panel A shows that a higher STW subsidy significantly decreases the level of unemployment. In particular, in the absence of any STW subsidy ($\tau = 0$), the unemployment level would have been almost 2 percentage point higher during the recession. As shown in Panel C, this comes at the cost of a significant decline in total TFP of about 2%. Yet, overall, Panel D shows that the total welfare effect of having STW is positive: compared to a situation without STW, welfare was about 2% higher during the recession. Results also confirm that the marginal welfare effect of increasing or decreasing the subsidy is close to zero. The reason is that the subsidy is already large enough that workers are willing to accept extremely low hours: Panel B shows that, at $\tau = 0.8$, the hours constraint on low productivity firms does not bite any longer, so that any further increase in the subsidy does not affect the hours and employment allocation any more.

The previous calibration considers the Great Recession in Italy as a steady state, and asks what the value is, in such a steady state, of having STW subsidies target firms with negative idiosyncratic shocks. But the nature of shocks, whether they are permanent or transitory, aggregate or idiosyncratic, may matter as well in assessing the effects of STW policies. Firms may be more willing to hoard labor when they expect a shock to be temporary, and therefore relaxing constraints to labor hoarding may be more effective for temporary shocks.[38] To get further insights on this, in Appendix 2.F.3, we use our calibrated model and simulate the effects of STW under two different scenarii of aggregate shocks: a permanent shock and a transitory shock.

Results, reported in Appendix Figure 2.F1 show that hoarding is indeed more valuable when the shock is transitory than permanent, and that labor hoarding is significantly larger when the cost to firms of replacing their workers increases. As a consequence, the employment effects of having STW also differ according to the permanence of the aggregate shock. The employment effects of STW on impact are significantly larger (around 20% to 40%) when the shock is temporary than when it is permanent. This, again, is especially true when the cost of replacing workers is high, and when the magnitude of the aggregate shock is large.

[38]Our previous analysis in Figure 2.5 indeed indicates that employment effects of STW are larger for firms that were high productivity prior to the recession, suggesting that STW may be more effective for high productivity firms experiencing a transitory negative shock than for permanently low productivity firms.

These counterfactual simulations help put into perspective our empirical results, and gauge their external validity outside the Italian context of the Great Recession. In particular, they help explain why the effectiveness of STW may have proved very different in Italy compared to other countries such as Germany during the Great Recession. First, the recession was a very transitory and very large shock in Germany (due to the collapse of world trade in 2009) and a much longer and protracted shock in Italy (due to the European debt crisis that followed). Second it is mostly high productivity exporting manufacturers that were affected in Germany, with high skilled workers that are very costly to replace, while, as we showed, it is mostly low productivity firms with lower skilled workers that were affected in Italy. This suggests that STW might have been much more effective in the German context than in Italy during the Great Recession.

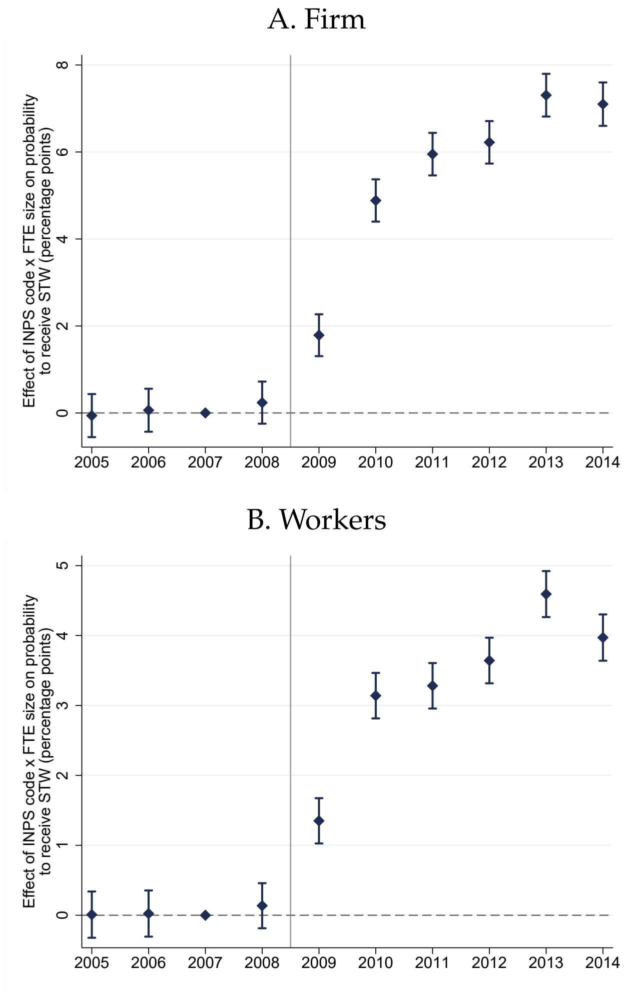
## 2.7 Concluding Remarks

STW programs have attracted a lot of attention as a tool to subsidize labor hoarding, and have been aggressively used during the Great Recession. Yet, very little is known on their effects and welfare consequences. This paper contributes by providing new high quality administrative data, a compelling quasi-experimental setting and a general framework to interpret our results. We show that STW has large, but temporary effects on labor inputs, and no significant long run insurance value to workers. We provide evidence that the dynamics of these effects has to do with the particular selection of firms into STW and the nature of the shock they face. Furthermore, we show that STW does significantly affect reallocation in the labor market.

Our framework then enables to use this empirical evidence to characterize the welfare consequences of STW. We derive a formula for the optimal STW subsidy in a general class of search and matching models. The fundamental insight is that, above the traditional trade-off between the value of transfers and fiscal externalities, STW will entail positive welfare gains when equilibrium employment is suboptimally low, and hours suboptimally high. Importantly, our formula offers a clear representation of these hours and employment inefficiency terms that connects to the data. The advantage of this approach is that the formula, and the key tradeoffs underpinning it, remain the same irrespective of the exact structure and primitives of the underlying model. In that sense, our formula is robust to the way wages and hours are determined in the model, to the specification of the costs of replacing or firing workers, to the presence of specific human capital, to various sources of hours or wage rigidity, to the presence of liquidity constraints, etc. Based on our empirical evidence, we show that STW has positive, albeit small, welfare effects. While more work needs to be done to better understand STW programs, the calibrated version of our model already enables to explore the validity of our findings outside the Italian context, and suggests that STW will be significantly more effective for large but transitory shocks in contexts where labor hoarding is constrained by wage rigidities, hours rigidities or financial rigidities.

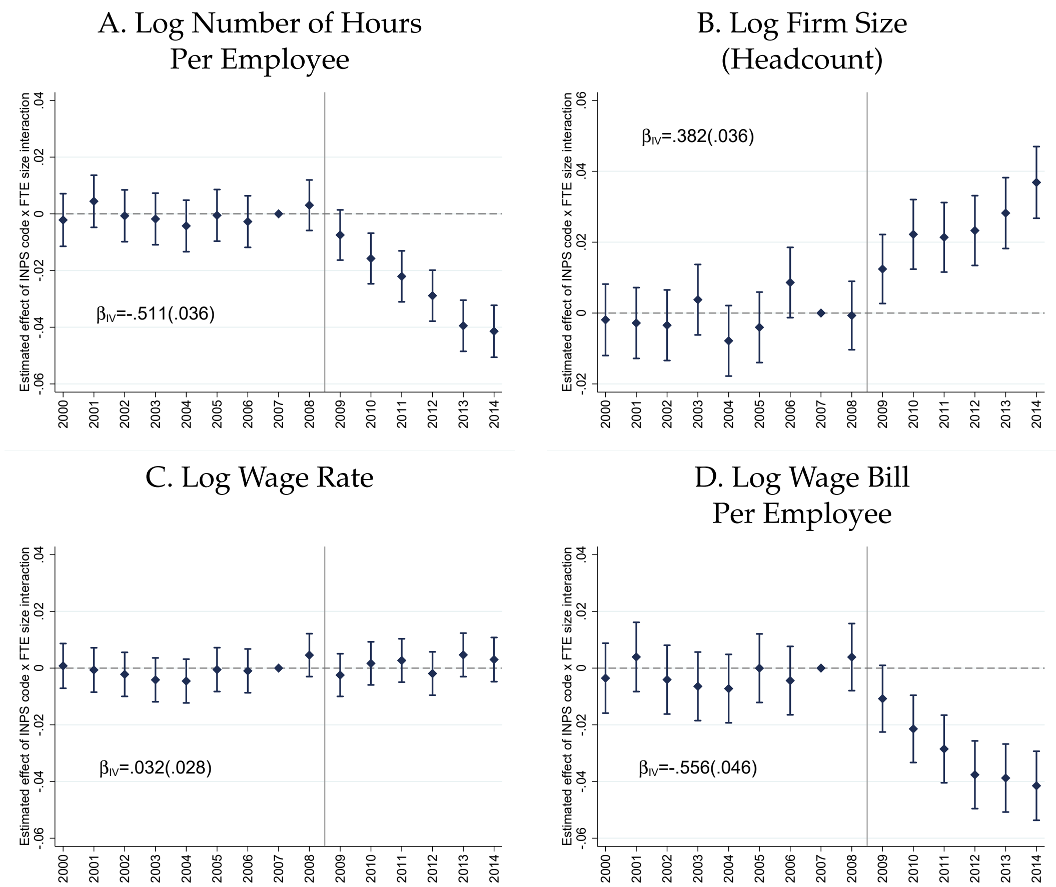
## 2.8 Figures

FIGURE 2.1: FIRMS' AND WORKERS' PROBABILITY OF RECEIVING SHORT TIME WORK TREATMENT BY FIRM SIZE AND SECTOR



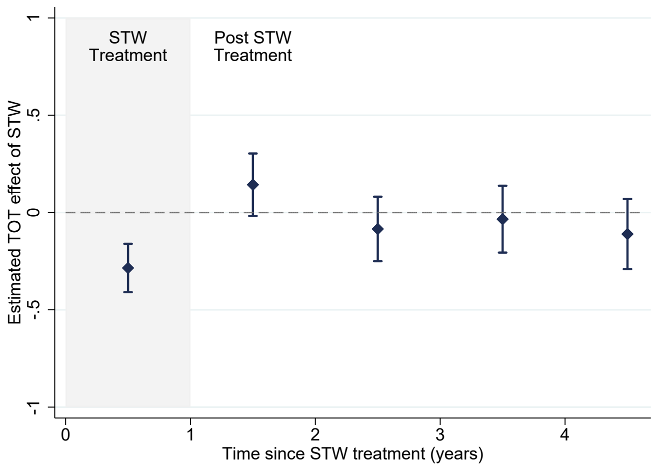

**Notes:** The graphs show the coefficients $\hat{\gamma}_1^t$ estimated from equation (2.1) for all years $t \in [2005, 2014]$ using the probability of STW receipt as outcome. This coefficient captures the triple interaction between being a firm with an INPS code eligible to STW, having had a firm size above the eligibility threshold in the 6 month prior and being in year $t$. The omitted year is 2007, so all results are relative to 2007. Panels A and B plot the estimated coefficients for the probability of STW receipt at firm level and at worker level respectively. We cluster standard errors at the INPS code times firm size group level. The vertical bars indicate 95% confidence intervals. See text for details.

FIGURE 2.2: ESTIMATES OF THE EFFECTS OF SHORT TIME WORK ON FIRMS' OUTCOMES




**Notes:** The graphs show the coefficients $\hat{\gamma}_1^t$ estimated from equation (2.1) for all years $t \in [2000, 2014]$ for different firm-level outcomes. The omitted year is 2007, so all results are relative to 2007. We cluster standard errors at the INPS code times firm size group level. The vertical bars indicate 95% confidence intervals based on cluster-robust standard errors. Each graph also reports the coefficient $\hat{\beta}_{IV}$ estimated from equation 2.2 and its associated standard error. The wage rate is defined as earnings per hour worked per employee.

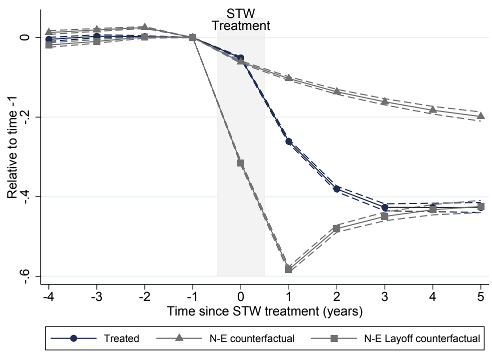
FIGURE 2.3: TOT ESTIMATES OF THE DYNAMIC EFFECTS OF SHORT TIME WORK: LOG NUMBER OF HOURS



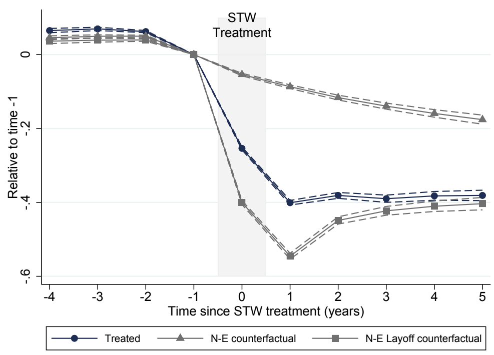

**Notes:** The graph reports the coefficients $\hat{\beta_k^{TOT}}$ for $k \in [0, ..., 4]$ for the dynamic effects of STW treatment on hours worked per employee. These effects are estimated recursively as illustrated in Appendix 2.B. The $\beta_k^{TOT}$ coefficients identify the dynamic treatment effects of STW receipt in year $k = 0$ on outcomes in years $k \in [0, ..., 4]$. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. The graph indicates that the effects of STW treatment are only felt on impact, and disappear immediately after treatment. There are no significant long term treatment effects of STW on firms' employment outcomes. See text for details.

FIGURE 2.4: DYNAMIC EFFECTS OF CIGS TREATMENT ON WORKERS' OUTCOMES

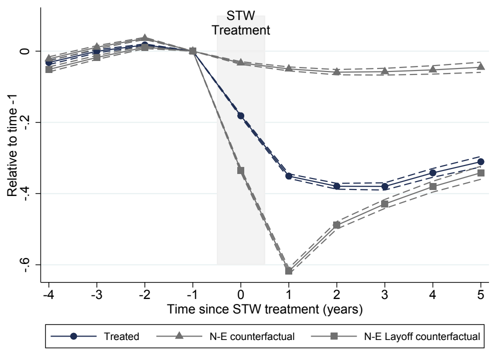
A. Probability of Employment




B. Number of Hours Worked

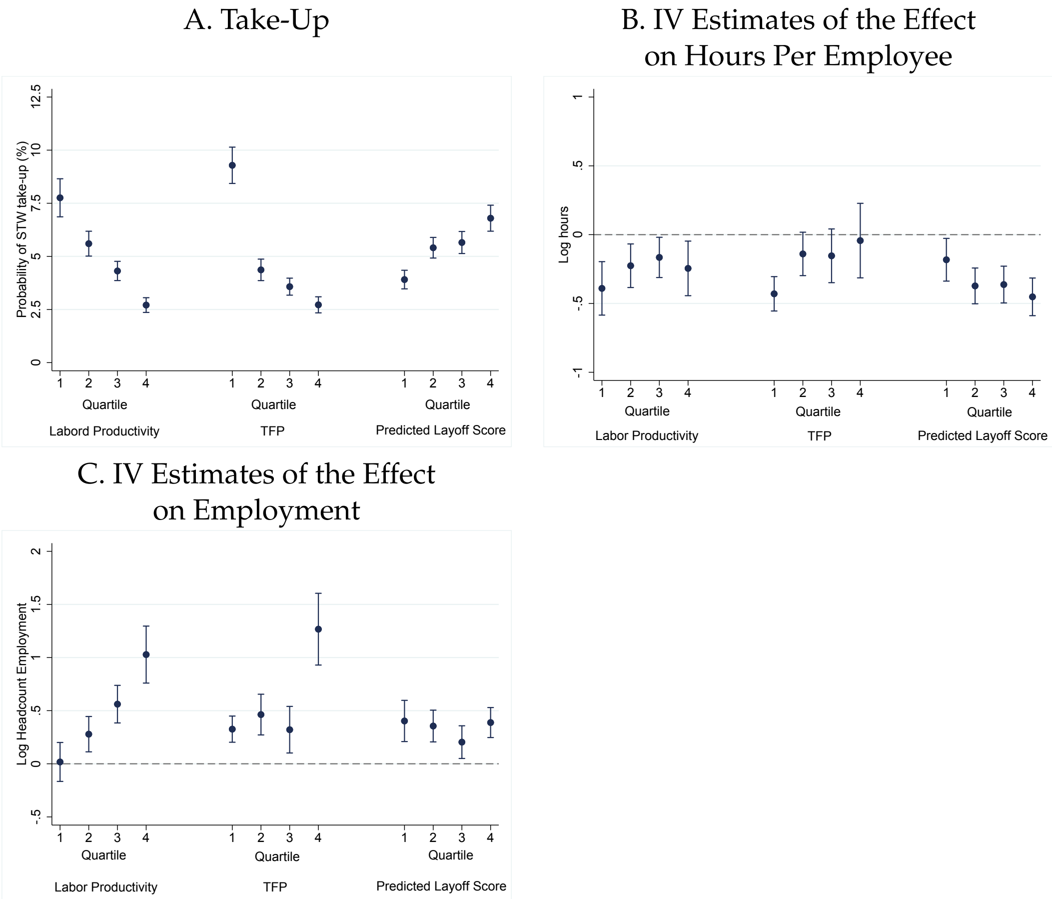



C. Earnings + CIGS/Transfers




**Notes:** The graphs report the estimated coefficients of event study regressions for different outcomes and different event-year definitions at the worker level. All estimates are relative to event-year -1 and are scaled by the average level of the outcome in that year. Individual and calendar year fixed effects are included in the event-time specification. The dashed lines around the estimates indicate 95% confidence intervals based on robust standard errors clustered at the individual level. For the treatment group (indicated by solid circles), an event year is defined as the first year in which the worker experiences a STW event, conditional on the worker being in a firm with 6-month average FTE size $\in (15;25]$ at event time -1. The first comparison group (indicated by solid triangles) consist of workers employed at firms with 6-month average FTE size $\in (5;25]$ at event time -1, which are not eligible for STW due to either their INPS code or FTE size. The second comparison group (indicated by solid squares) consist of workers employed at non-eligible firms with 6-month average FTE size $\in (5;25]$ at event time -1 and who experience a mass layoff in event time 0. Individuals in the two comparison groups are matched to individuals in the treatment group using Mahalanobis nearest-neighbor matching without replacement based on gender, age, job characteristics at event time -1, employment status, annual weeks worked, earnings and firm size at event times -1, -2, -3 and -4, and main industry at event time-1. Total hours worked and total earnings are unconditional on employment. In Panel C, we report the evolution of all earnings, and all transfers received (including STW or any other social assistance program or tax transfer).

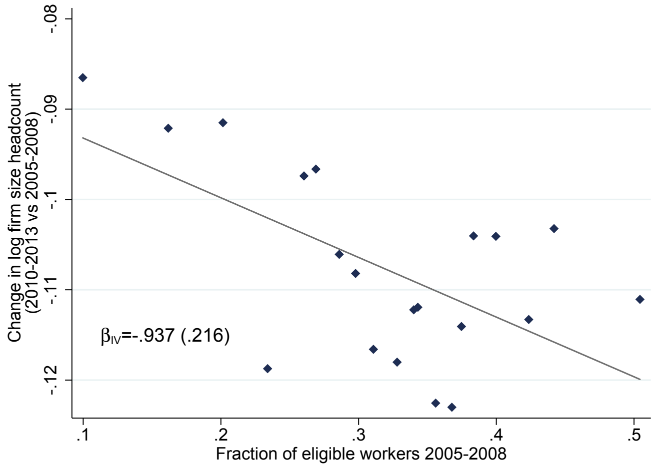
FIGURE 2.5: SELECTION OF FIRMS INTO STW AND HETEROGENEOUS TREATMENT EFFECTS



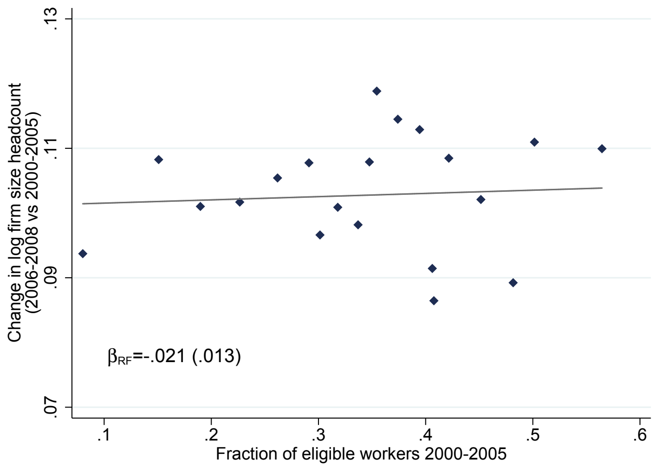

**Notes:** The graphs show heterogeneity in STW take-up and treatment effects across different firm characteristics. Panel A displays the estimated coefficient $\hat{\kappa}_1$ from specification (2.3) for the probability of STW take-up for groups of firms with different levels of productivity and with different predicted likelihood of mass layoffs. For productivity, firms are ranked into four quartiles of the distribution of average yearly productivity in 2007-2008. Productivity is measured as labor productivity (defined as value added per week worked) or total factor productivity (defined as described in Section 2.3.2). Firms are also ranked into quartiles of the distribution of their predicted probability of mass layoff, as described in Section 2.5.1. The vertical bars indicate 95% confidence intervals based on cluster-robust standard errors. Panels B and C report the estimated $\hat{\beta}_{IV}$ from specification (2.2) for the log of hours worked per employee and the log of total employment headcount. The two panels are constructed in the same way as Panel A. See text for details.

FIGURE 2.6: REALLOCATION EFFECTS: EMPLOYMENT GROWTH OF NON-ELIGIBLE FIRMS AS A FUNCTION OF STW TREATMENT IN THE LOCAL LABOR MARKET

A. Employment Growth
2005-2008 to 2010-2013

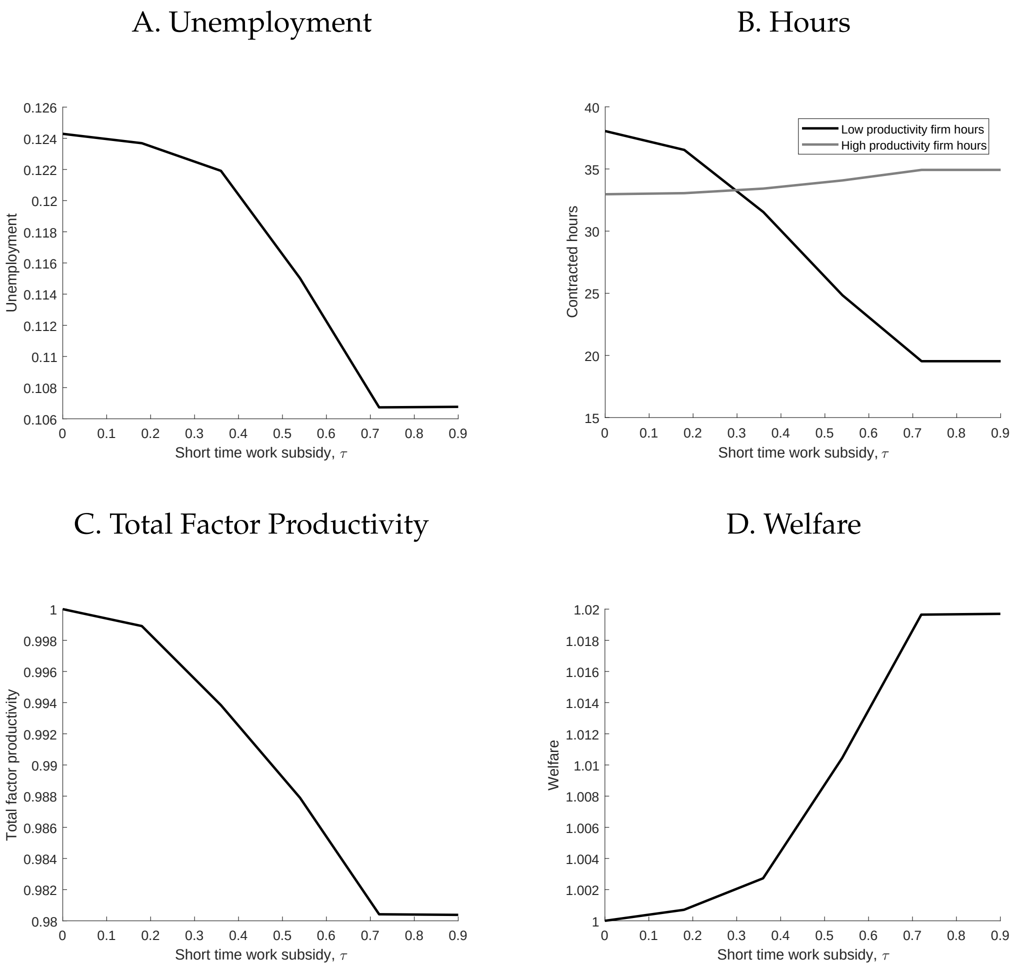



B. Placebo: Employment Growth
2000-2005 to 2005-2008




**Notes:** The graphs show binned scatter plots of the reduced form of equation (2.7). Panel A plots the reduced form relationship between the change in average log firm size headcount of firms non-eligible to STW in a local labor market (LLM) between 2005-2008 and 2010-2013, and the fraction of eligible workers in 2005-2008 in the LLM based on the interaction between firm size and INPS codes. Both variables are residualized on firm level and LLM level controls. Panel A also reports the $\hat{\beta}_{IV}$ coefficient from equation 2.7 and its associated robust standard error clustered at the LLM level. Panel B is constructed in the same way as Panel A and shows the placebo relationship between the change in average log firm size headcount of firms non-eligible to STW in a LLM between 2000-2005 and 2005-2008, and the fraction of eligible workers in 2000-2005 in the LLM. Panel B also reports the reduced-form $\hat{\beta}_{RF}$ coefficient from equation (2.7) and its associated robust standard error clustered at the LLM level.

FIGURE 2.7: COUNTERFACTUAL SIMULATIONS: EFFECTS OF CHANGING STW GENEROSITY $\tau$




**Notes:** The figure displays results of a counterfactual analysis of steady state equilibria of the Italian economy during the Great Recession using our calibrated model and varying the level of the STW subsidy $\tau$. Panel A shows counterfactual values of the equilibrium unemployment rate, Panel B displays counterfactual values of the hours per employee for low and high productivity firms. Panel C shows the counterfactual values of total factor productivity. Panel D shows counterfactual values of total welfare (i.e. including firms profits which are rebated lump sum to workers). For Panels C and D, results are normalized to the level of the outcome in the steady state equilibrium without STW ($\tau$=0). All details of the calibration of the model are given in Appendix 2.F.

## 2.9 Tables

TABLE 2.1: EFFECTS OF STW TREATMENT ON FIRMS' AND WORKERS' OUTCOMES

| | IV Estimate | Std Error | N |
|---|---|---|---|
| | **A. First Stage** | | |
| Proba. of CIGS Take-Up | .05 | (.002) | 3029855 |
| | **B. Employment Outcomes** | | |
| Log Number of Hours Per Employee | -.511 | (.036) | 2843205 |
| Log Number of Full-Time Weeks Per Employee | -.461 | (.034) | 2843205 |
| Log Firm Size (Headcount) | .382 | (.036) | 2843205 |
| Log Wage Rate | .032 | (.028) | 2843205 |
| Log Wage Bill Per Employee | -.556 | (.046) | 2843205 |
| Log Number of Open-Ended Contracts | .432 | (.047) | 2843205 |
| Log Number of Fixed-Term Contracts | -.367 | (0.128) | 2843205 |
| Firm Survival Probability (in $t+1$) | -.014 | (.009) | 2570917 |
| | **C. Balance-Sheet & Productivity Outcomes** | | |
| Firm Value-Added | .095 | (.159) | 873839 |
| Value-Added Per Worker | -.508 | (.120) | 873839 |
| Tangible Investment | -.003 | (.672) | 873839 |
| Liquidity | .939 | (.461) | 873839 |

**Notes:** Panel A reports the estimates of the coefficient $\hat{\kappa}_1$ from specification (2.3) and its associated cluster-robust standard error in parenthesis. Panels B and C report the $\hat{\beta}_{IV}$ coefficients estimated from equation (2.2) and their associated cluster-robust standard errors in parenthesis for a set of different firm-level outcomes. The wage rate is defined as total earnings per hours worked per employee. For survival probability, the reported coefficient is the IV estimate scaled by average survival probability in $t+1$: $\hat{\beta}_{IV}/\bar{Y}$. Value added is defined as total revenues plus unsold stocks minus cost of goods and services used in production, or equivalently total profits plus total capital depreciation and total wage costs. Liquidity is defined as cash and cash equivalents.

TABLE 2.2: ROBUSTNESS OF BASELINE EFFECTS

| | (1) "Doughnut" Regression | (2) Only ≤ 15 FTE (Placebo) | (3) Only >15 FTE | (4) Permutation Test (Placebo) | (5) No Dismissal Rule Change >60FTE Across Italy | (6) No Dismissal Rule Change 50FTE threshold |
|---|---|---|---|---|---|---|
| | | | First Stage | | | |
| Proba. of CIGS Take-Up | .053 | .002 | .051 | .000 | .055 | .041 |
| | (.002) | (.000) | (.002) | (.000) | (.005) | (.004) |
| | | | Outcomes | | | |
| | IV | RF | IV | RF | IV | IV |
| Log Hrs per wker | -.449 | -.011 | -.602 | .000 | -.670 | -.156 |
| | (.037) | (.020) | (.081) | (.010) | (.230) | (.132) |
| Log Empl. | .284 | -.020 | .306 | -.001 | .848 | .338 |
| | (.032) | (.030) | (.099) | (.009) | (.297) | (.258) |
| Log Wage Bill | -.544 | -.026 | -.498 | .000 | -.568 | -.390 |
| | (.049) | (.030) | (.155) | (.013) | (.297) | (.709) |
| N | 2686140 | 2608383 | 429490 | 2978239 | 152753 | 44793 |

**Notes:** The upper panel of the table reports the estimated coefficient $\hat{\kappa}_1$ from specification (2.3). Cluster-robust standard errors are reported in parenthesis below each coefficient. The lower panel reports either reduced form or IV coefficients for different firm-level outcomes. Column (1) reports the coefficients of a doughnut version of specification (2.2) excluding firms with 6-month average FTE size $\in (12, 18]$. Column (2) reports the reduced-form coefficient $\hat{\alpha}_1$ for specification (2.4) restricting the sample to firms with 6-month average FTE size $\in (5, 15]$. It shows little evidence of differential trends during the recession between eligible and ineligible INPS codes. Column (3) reports the IV coefficients for specification (2.4) restricting the sample to firms with 6-month average FTE size $\in (15, 25]$ and instrumenting STW take-up with $\{\mathbb{1}[g \in \mathcal{E}] \cdot \mathbb{1}[t \geq 2009]\}$. Column (4) reports reduced-form coefficients for a placebo-version of specification (2.2) in which the sample is restricted to firms with non-eligible INPS codes and placebo "eligibility" status is assigned to a randomly chosen subgroup of INPS codes. Column (5) reports the estimated IV coefficients for specification (2.2) for a sample of establishments with 6-month FTE size $\in (0, 40]$ that belong to multi-establishment firms with FTE size $> 60$. For this group of firms, employment protection legislation does not apply differentially for firms above and below the 15 threshold. Column (6) reports the estimated IV coefficients for specification (2.2) for a sample of firms with INPS codes in the retail sectors and with 6-month FTE size $\in (25, 75]$. For this small group of firms, the size threshold that determines eligibility is set at 50 and employment protection legislation does not apply differentially above and below the threshold.

TABLE 2.3: EQUILIBRIUM EFFECTS OF STW ON NON-TREATED FIRM OUTCOMES

| | **Reallocation Effects** | | | **Placebo Estimates** | | |
|---|---|---|---|---|---|---|
| | IV | IV | IV | RF | RF | RF |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | A. Employment Spillovers on Non-Eligible Firms | | | | | |
| Log Employment | -0.492 | -0.918 | -0.937 | -0.021 | -0.021 | -0.021 |
| | (0.137) | (0.216) | (0.216) | (0.013) | (0.013) | (0.013) |
| Log Inflows | -3.594 | -4.406 | -3.176 | -0.047 | -0.046 | -0.030 |
| | (1.947) | (2.380) | (1.440) | (0.112) | (0.113) | (0.107) |
| LLM Controls | | × | × | | × | × |
| Firm-level Controls | | | × | | | × |
| N | | 3023166 | | | 2784567 | |
| | B. Labor Market Effects on Productivity | | | | | |
| Log TFP | -2.307 | -2.093 | | -0.161 | -0.161 | |
| | (0.593) | (0.606) | | (0.129) | (0.129) | |
| LLM Controls | | × | | | × | |
| N | | 1222 | | | 1222 | |

**Notes:** Columns (1)-(3) of the table report the $\hat{\beta}^{R}_{IV}$ estimated from equation (2.7) and its associated robust standard errors clustered at the LLM level in parenthesis. Columns (4)-(6) report reduced-form placebo estimates of equation 2.7 comparing outcome growth during a placebo pre-recession periods (2000-2005) vs (2005-2008). LLM controls include the unemployment rate and the firm size and industrial composition of employment (employment shares by industry) in the LLM in the pre-recession period. Firm-level controls are the probability of STW take-up, firm size in 2008, a dummy for whether the firm ever has an eligible INPS code and 5-digit industry dummies. In Panel B, we estimate IV model similar to (2.7) but where the outcome is long differences of TFP, at the LLM level. We define TFP as TFP $= \text{VA}/(L^{\alpha}K^{\beta})$, where we aggregate all variables (VA, L and K) at the LLM level.

## 2.A Appendix A: Additional Figures and Tables

### 2.A.1 Descriptive Statistics

TABLE 2.A1: DISTRIBUTION OF FIRMS' CHARACTERISTICS IN MAIN SAMPLE, BROKEN DOWN ACROSS ELIGIBLE AND NON-ELIGIBLE INPS CODES (2008)

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | All INPS codes | | Eligible INPS codes | | Non-eligible INPS codes | |
| | mean | sd | mean | sd | mean | sd |
| Employees (headcount) | 8.72 | 5.16 | 9.78 | 5.55 | 8.22 | 4.90 |
| Employees (FTE) | 8.04 | 4.78 | 9.35 | 5.38 | 7.42 | 4.33 |
| Employees on open-ended contracts | 7.80 | 4.91 | 8.96 | 5.35 | 7.25 | 4.60 |
| Employees on fixed-term contracts | 0.92 | 2.11 | 0.81 | 1.78 | 0.98 | 2.25 |
| Annual hours worked per employee | 2015.26 | 1008.70 | 2043.69 | 980.97 | 2001.86 | 1021.24 |
| Annual wage bill per employee (000) | 20.66 | 12.38 | 22.49 | 13.22 | 19.80 | 11.86 |
| Net revenue per week worked (000) | 6.22 | 49.55 | 5.94 | 52.77 | 6.48 | 46.31 |
| Value added per week worked (000) | 1.11 | 11.36 | 1.22 | 14.41 | 1.01 | 7.42 |
| Liquidity | 0.11 | 0.14 | 0.09 | 0.13 | 0.12 | 0.15 |
| Investment in tangible assets | 0.07 | 0.11 | 0.07 | 0.10 | 0.07 | 0.11 |
| Investment in intangible assets | 0.02 | 0.05 | 0.01 | 0.04 | 0.02 | 0.06 |
| North-West | 0.29 | 0.46 | 0.30 | 0.46 | 0.29 | 0.46 |
| North-East | 0.25 | 0.43 | 0.20 | 0.40 | 0.27 | 0.44 |
| Center | 0.21 | 0.40 | 0.20 | 0.40 | 0.21 | 0.41 |
| South | 0.25 | 0.43 | 0.30 | 0.46 | 0.23 | 0.42 |
| Observations | 321580 | | 102757 | | 218823 | |

**Notes:** The table reports the mean and standard deviation of a set of firm-level variables for firms in our sample as of 2008. The summary statistics refer to year 2008. Column (1) refers to both firms with eligible and non-eligible INPS codes. Column (2) restricts the sample to firms with eligible codes and column (3) to firms with non-eligible codes. Revenue, value-added, liquidity and investments come from the CERVED data which covers approximately 50% of firms in our sample. Value added is defined as total revenues plus unsold stocks minus cost of goods and services used in production, or equivalently total profits plus total capital depreciation and total wage costs. Liquidity is defined as cash and cash equivalents. All monetary figures are expressed in 2008 euros. North-West, North-East, Center and South are dummies for the geographic region of location of the firm within Italy.

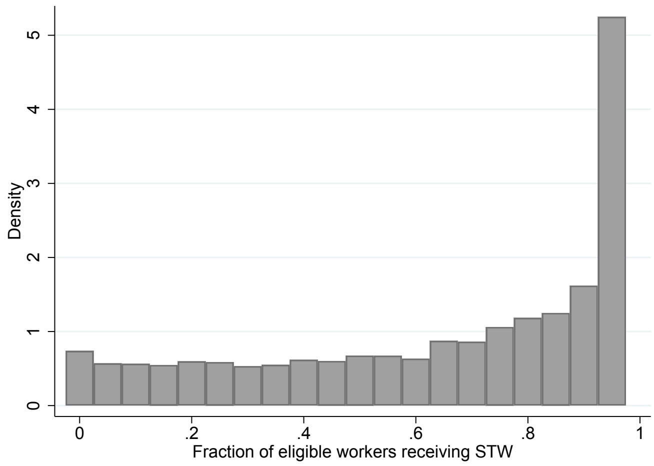
TABLE 2.A2: DISTRIBUTION OF WORKERS' CHARACTERISTICS IN MAIN SAMPLE, BROKEN DOWN ACROSS ELIGIBLE AND NON-ELIGIBLE INPS CODES (2008)

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | All INPS codes | | Eligible INPS codes | | Non-eligible INPS codes | |
| | mean | sd | mean | sd | mean | sd |
| Proportion female | 0.38 | 0.48 | 0.24 | 0.43 | 0.45 | 0.50 |
| Age | 36.89 | 10.72 | 38.53 | 10.51 | 36.04 | 10.72 |
| Proportion aged <40 | 0.57 | 0.49 | 0.51 | 0.50 | 0.60 | 0.49 |
| Proportion aged 40-54 | 0.35 | 0.48 | 0.40 | 0.49 | 0.33 | 0.47 |
| Proportion aged 55+ | 0.08 | 0.26 | 0.09 | 0.29 | 0.07 | 0.25 |
| Experience (years) | 14.23 | 10.58 | 16.04 | 10.81 | 13.30 | 10.34 |
| Tenure (months) | 59.49 | 71.52 | 66.72 | 76.83 | 55.75 | 68.31 |
| Proportion on full-time contract | 0.82 | 0.38 | 0.90 | 0.30 | 0.78 | 0.42 |
| Proportion on open-ended contract | 0.83 | 0.37 | 0.88 | 0.32 | 0.81 | 0.40 |
| Proportion on fixed-term contract | 0.15 | 0.36 | 0.12 | 0.32 | 0.17 | 0.38 |
| Proportion on seasonal contract | 0.02 | 0.13 | 0.00 | 0.05 | 0.02 | 0.15 |
| Proportion blue collar | 0.64 | 0.48 | 0.69 | 0.46 | 0.61 | 0.49 |
| Proportion white collar | 0.27 | 0.44 | 0.24 | 0.43 | 0.28 | 0.45 |
| Proportion manager | 0.00 | 0.05 | 0.00 | 0.06 | 0.00 | 0.05 |
| Proportion apprentice | 0.07 | 0.26 | 0.05 | 0.22 | 0.09 | 0.28 |
| Proportion native born | 0.84 | 0.36 | 0.85 | 0.36 | 0.84 | 0.37 |
| Observations | 3350203 | | 1140981 | | 2209222 | |

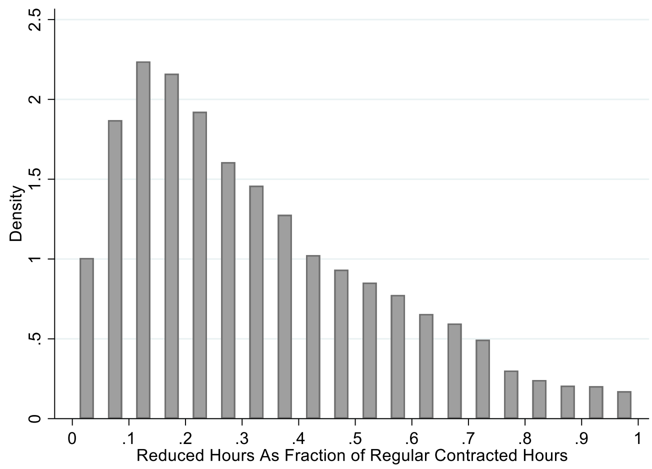
**Notes:** The table reports the mean and standard deviation of a set of worker-level variables for workers who are employed at firms in our sample at some point during year 2008. The summary statistics refer to year 2008. Column (1) refers to workers in both firms with eligible and non-eligible INPS codes. Column (2) restricts the sample to workers in firms with eligible codes and column (3) to workers in firms with non-eligible codes.

FIGURE 2.A1: DISTRIBUTION OF STW TREATMENT ACROSS WORKERS IN FIRMS EXPERIENCING STW

A. Distribution of Fraction of Eligible Workers Put on STW in Treated Firms



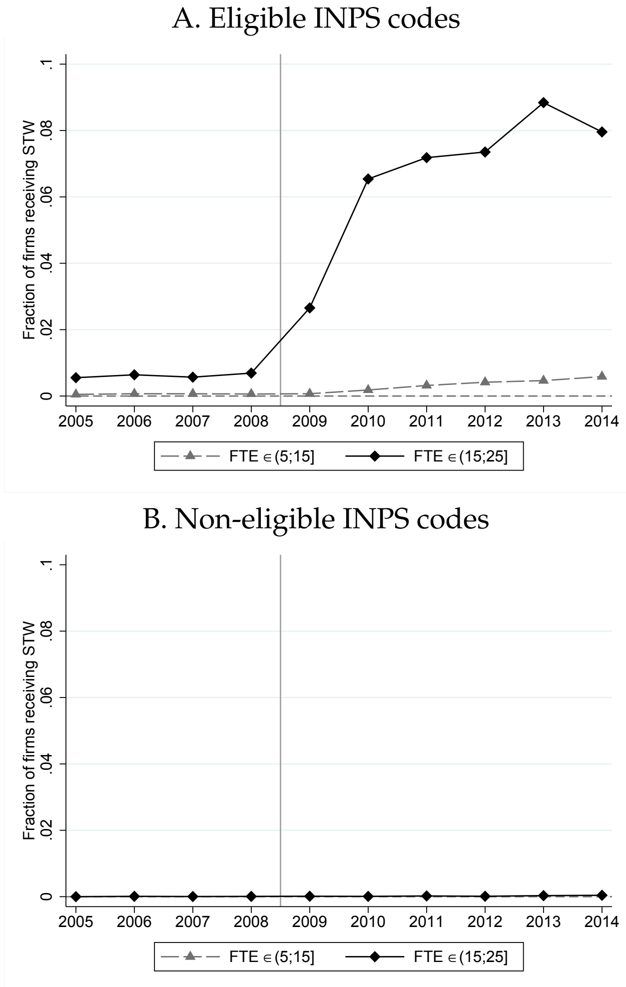

B. Distribution of Reported Weekly Hours Reductions Across Treated Workers




**Notes:** The figure reports descriptive statistics on the distribution of treatment across workers in firms experiencing STW. Panel A plots the distribution of the ratio of treated workers to eligible workers in firms currently under short time work treatment. Note that apprentices and top executives are not eligible for STW. But there are no other differential incentives to put workers on STW across workers in the Italian system. Panel A shows that most firms choose to put all their eligible workers in the STW program and therefore spread hours reductions across all eligible workers. Panel B reports the distribution of reported weekly hours reduction of workers currently experiencing STW. The graph shows a smooth distribution of hours reductions, with a mode around .25, and an average weekly hours reduction of a little more than 35%.

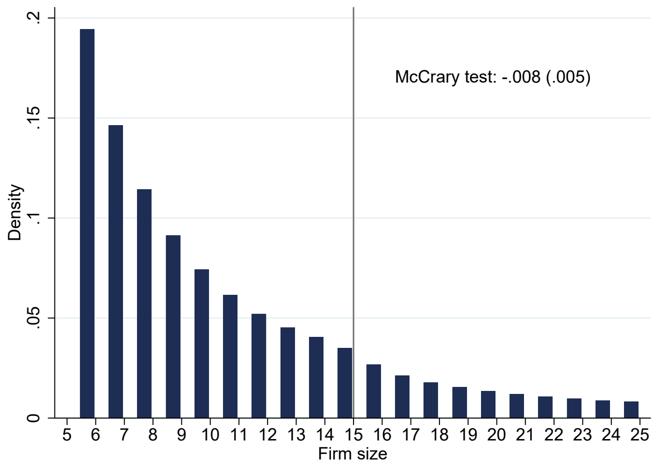
## 2.A.2 Identification and Robustness: Additional Evidence

FIGURE 2.A2: FRACTION OF FIRM'S RECEIVING STW TREATMENT BY FIRM SIZE AND INPS CODE




**Notes:** The graphs show the fraction of firms receiving STW in each calendar year $t \in [2005, 2014]$ by eligibility status and maximum 6-month average FTE firm size. Panel A plots, among firms with eligible INPS codes in our sample, the evolution of the fraction of firms receiving STW in each calendar year $t$ from 2005 to 2014, for firms with a maximum 6-month average FTE size $\in (15, 25]$ in year $t$ and for firms with a maximum 6-month average FTE size $\in (5, 15]$ in year $t$. Panel B replicates Panel A for firms in non-eligible INPS codes.

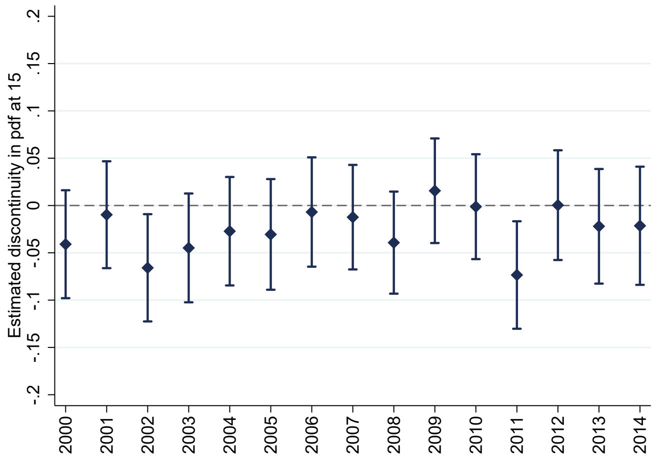
FIGURE 2.A3: DISTRIBUTION OF FIRMS' FTE SIZE (2000-2015)



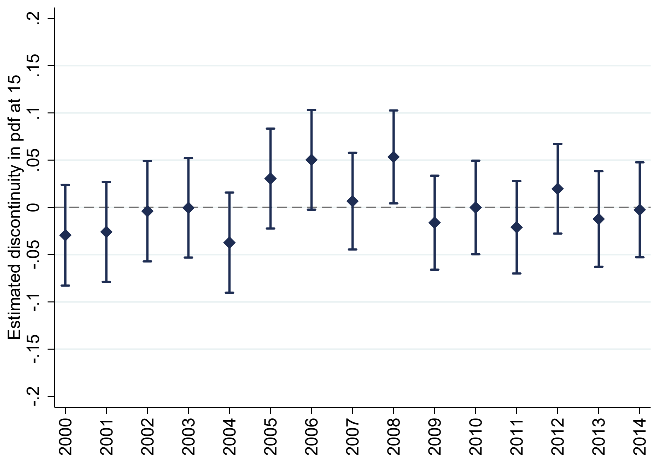

**Notes:** The graph shows the probability density function of FTE firm size by 1-unit bins for the years 2000-2015. The graph also reports the McCrary test statistics for the presence of a discontinuity in the probability density function of FTE size at 15 and its standard error. FTE firm size is defined as the full-time equivalent of all employees in the firm, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units.

FIGURE 2.A4: MCCRARY TEST STATISTIC OF DISCONTINUITY IN FIRMS' SIZE DISTRIBUTION

A. Eligible INPS codes

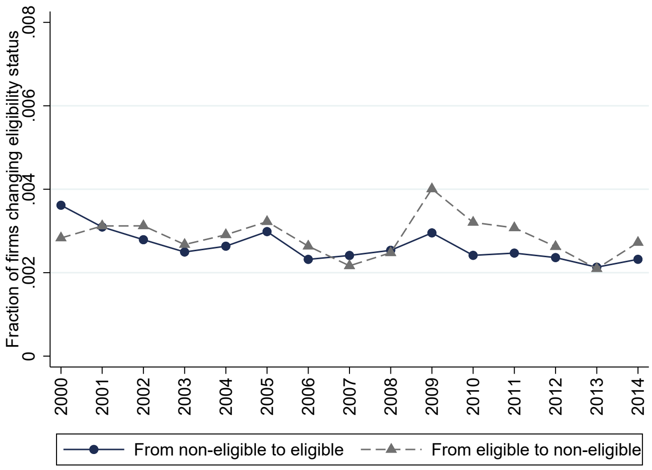



B. Non-eligible INPS codes



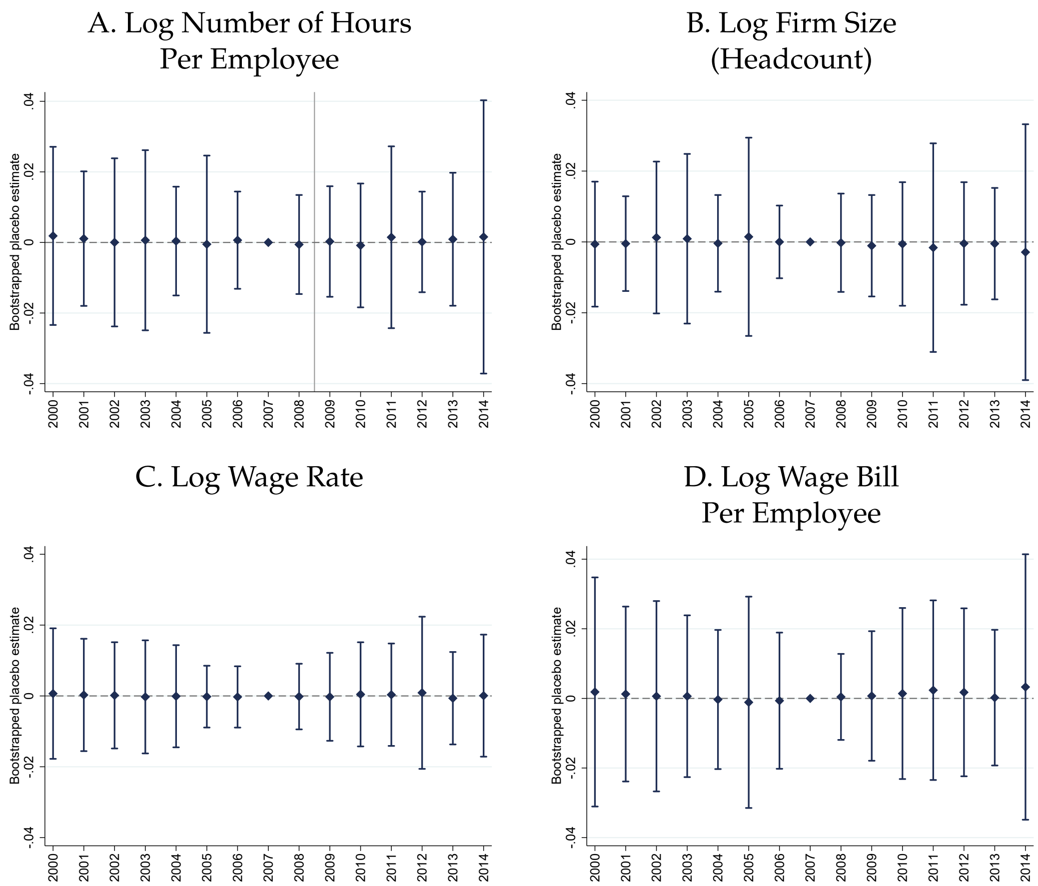

**Notes:** The graphs show the McCrary test statistic for the presence of a discontinuity in the probability density function of FTE size at 15 and its confidence interval for each year $t \in [2000, 2014]$, and for eligible and non-eligible INPS codes separately. The vertical bars indicate 95% confidence intervals. FTE firm size is defined as the full-time equivalent of all employees in the firm, including those who are not eligible for CIGS (managers, apprentices and work-from-home employees) and those who are currently on unpaid leave (unless the firm has hired a replacement). Part-time workers are counted in full-time equivalent units.

FIGURE 2.A5: FRACTION OF FIRMS CHANGING ELIGIBILITY STATUS DUE TO INPS CODE CHANGES (2000-2014)



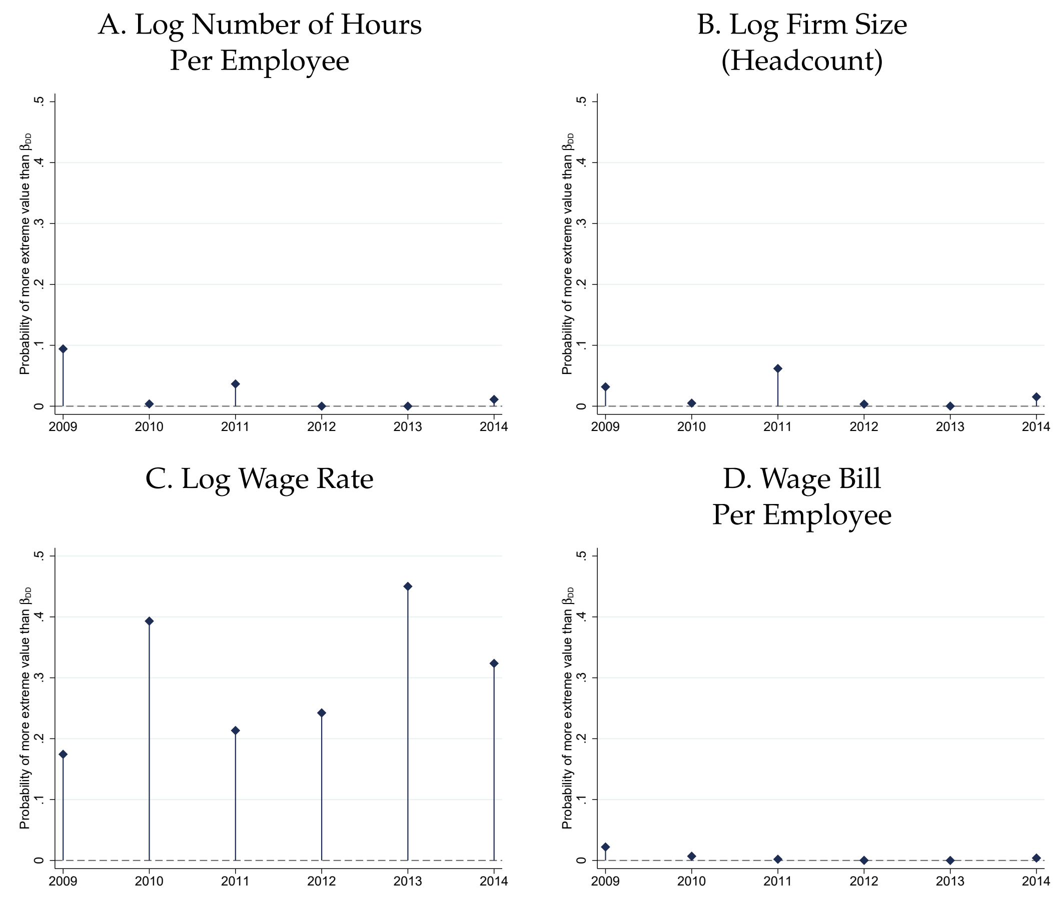

**Notes:** The graphs shows the fraction of firms that change eligibility status due to a change in their INPS code for each year $t \in [2000, 2014]$, and separately for firms changing their status from eligible to non-eligible and vice versa.

FIGURE 2.A6: PLACEBO ESTIMATES OF THE EFFECTS OF SHORT TIME WORK ON FIRMS' OUTCOMES




**Notes:**These graphs show the coefficients $\hat{\gamma}_1^t$ estimated from a placebo version of equation 2.1 for all years $t \in [2000, 2014]$ for different firm-level outcomes. Restricting the sample to non-eligible INPS codes, we select a random series of INPS codes to which we assign a placebo "eligible" status. On this sample we run specification 2.1. The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors from 100 replications of the placebo estimation. The wage rate is defined as total earnings per week worked per employee.

FIGURE 2.A7: P-VALUES OF PERMUTATION TEST ON BASELINE ESTIMATES USING BOOTSTRAPPED PLACEBO ESTIMATES




**Notes:**These graphs report the p-values of a test of equality of the baseline reduced-form estimates of model 2.1 reported in Figure 2.2 and the bootstrapped placebo estimates reported in Figure 2.A6 for the years 2009 to 2014. The p-values indicate the probability of randomly estimating an effect at least as large as our baseline estimates. The wage rate is defined as total earnings per week worked per employee.

## 2.B Appendix B: Dynamic Treatment Effects

### 2.B.1 Recursive Identification of Dynamic Treatment Effects for Firms' Outcomes

To identify the full sequence of dynamic effects of STW treatment, we develop a methodology similar in spirit to the recursive identification of dynamic treatment effects in Cellini, Ferreira, and Rothstein (2010). We would like to identify the sequence of dynamic treatment effects $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$ which capture the effect of receiving STW treatment on outcome in the year of treatment ($\beta_0^{TOT}$), one year after treatment ($\beta_1^{TOT}$), etc., up to $k$ years after treatment ($\beta_k^{TOT}$). We focus our sample on all firms that are active in 2009, and with FTE firm size between 5 and 25 workers in 2008. We create the instrumental variable $Z_{2009}$, equal to one if a firm is eligible to STW in 2009, that is equal to the triple interaction of being above the 15 FTE firm size threshold in 2008 and being in an eligible INPS code in 2009. We know that this variable will be correlated with the probability of STW treatment in 2009 ($T_{2009}$), but also with the probability of treatment in 2010 ($T_{2010}$), in 2011 ($T_{2010}$), etc. We also know from Appendix Figure 2.B3 that $Z_{2009}$ is not correlated with treatment in the past ($T_{2008}$, $T_{2007}$, etc.). If we now run on this sample the following reduced-form regression:

$$\begin{aligned} Y_{igst} = \; & \sum_j \beta_j^{RF} \cdot Z_{2009} \cdot \mathbb{1}[j=t] \\ & + \sum_j \sum_k \gamma_2^{jk} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] * \mathbb{1}[j=t] \Big\} \\ & + \sum_j \sum_k \gamma_3^{jk} \cdot \Big\{ \mathbb{1}[k=s] * \mathbb{1}[N_{i,t-1} > 15] * \mathbb{1}[j=t] \Big\} \\ & + \sum_j \sum_k \gamma_4^{jk} \cdot \Big\{ \mathbb{1}[k=s] * \mathbb{1}[j=t] \Big\} \\ & + \sum_k \gamma_5^{k} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] * \mathbb{1}[N_{i,t-1} > 15] \Big\} \\ & + \sum_k \gamma_6^{k} \cdot \Big\{ \mathbb{1}[g \in \mathcal{E}] * \mathbb{1}[k=s] \Big\} + v_{igst} \end{aligned} \tag{2.B1}$$

of the baseline IV model (see equation (2.2)) using $Z_{2009}$ as an instrument, the estimated coefficients of the reduced form for each year 2009, 2010, etc. ($\beta_{2009}^{RF}$, $\beta_{2010}^{RF}$, etc.) capture the dynamic Intention-To-Treat (ITT) effects from in 2009, letting potential future treatment occur.

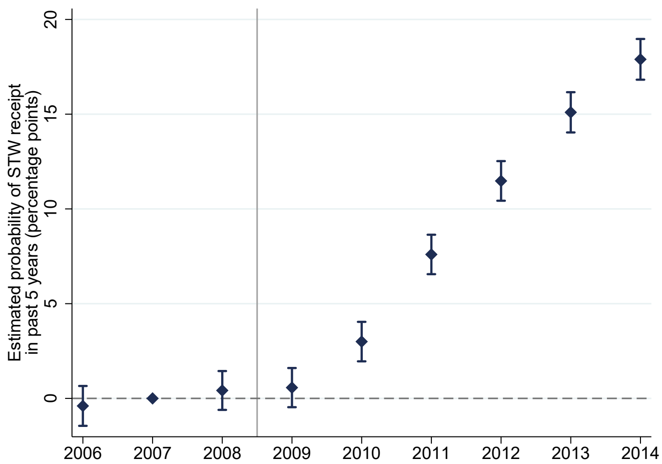
$$\beta_{2009}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{2.B2}$$

$$\beta_{2010}^{RF} = \beta_0^{TOT} \cdot \frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}} + \beta_1^{TOT} \cdot \frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}} \tag{2.B3}$$

The first stage regressions of $T_{igst}$ on $Z_{2009}$ enable us to identify $\frac{\mathrm{d}T_{2009}}{\mathrm{d}Z_{2009}}$, $\frac{\mathrm{d}T_{2010}}{\mathrm{d}Z_{2009}}$, etc. Using these estimates, the estimates of the ITT effects $\hat{\beta}_t^{RF}$ and the recursive structure of equations (2.B2), (2.B3), etc., we can identify the sequence of dynamic treatment effects $\{\beta_0^{TOT}, \beta_1^{TOT}, ..., \beta_k^{TOT}\}$.

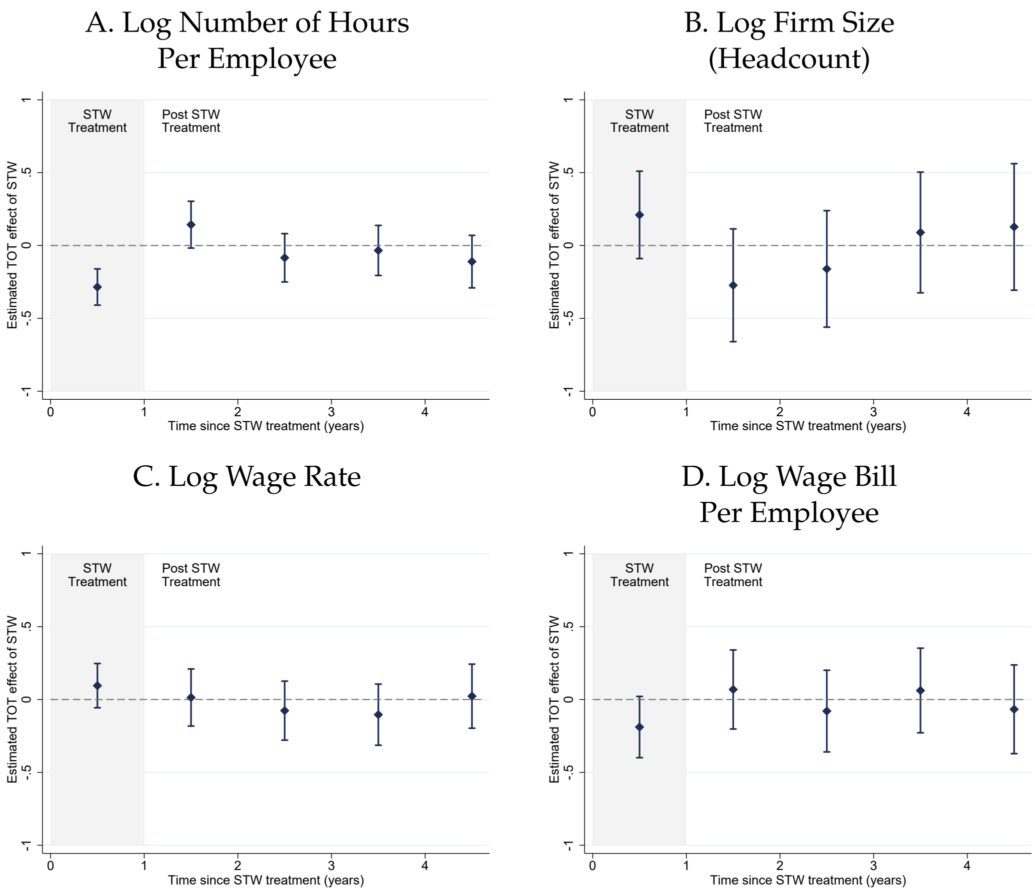
We display in Figure 2.B2 the results of these dynamic TOT effects, for various outcomes. The results suggest that the effects are large on impact, but disappear immediately once treatment stops.

FIGURE 2.B1: EFFECT OF INPS CODE AND FIRM SIZE ELIGIBILITY INTERACTION ON THE PROBABILITY OF HAVING RECEIVED STW TREATMENT IN THE PAST 5 YEARS




**Notes:** The graph shows the coefficients $\hat{\gamma}_1^t$ estimated from equation 2.1 for all years $t \in [2006, 2014]$ using as an outcome the probability of having received STW in the previous five years. The probability of STW receipt in the previous 5 years is at the firm level. The omitted year is 2007, so all results are relative to 2007. The vertical bars indicate 95% confidence intervals based on cluster-robust standard errors.

FIGURE 2.B2: TOT ESTIMATES OF THE DYNAMIC EFFECTS OF SHORT TIME WORK




**Notes:** The graphs report the coefficients $\hat{\beta_k^{TOT}}$ for $k \in [0, ..., 4]$ estimated recursively as illustrated in Appendix 2.B. The $\beta_k^{TOT}$ identify dynamic treatment effects of STW receipt at time $k = 0$ on outcomes at time $k \in [0, ..., 4]$. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. The wage rate is defined as total earnings per hour worked per employee.

### 2.B.2 Identification of Dynamic Treatment Effects for Workers

We want to understand to what extent the interesting dynamic patterns from the previous event studies reveal the deeper causal dynamic impact of STW treatment. Endogeneity concerns prevent interpreting the event study estimates on the treated as the causal dynamic impact of short time work. The incidence and timing of CIGS treatment across firms are indeed not random and workers within these firms may differ from other workers along various characteristics affecting their labor market dynamics. We start by explaining these issues, and show how two things can be done to tackle this issue.

**Model** We start by formulating a general statistical model of the dynamics of workers outcomes:

$$Y_{i,j,t+k} = \eta_i + X'_{it}\alpha_k + \beta_k \mathbb{1}[T_{jt} = 1] + \varepsilon_{j,t+k} + \mu_{i,t+k}$$

where $Y_{i,j,t+k}$ is the outcome of worker $i$ in year $t+k$, given the worker was in firm $j$ at time $t$. This outcome depends on some observed and unobserved individual characteristics $\eta_i$ and $X_{it}$, on having received STW treatment or not at time $t$. This outcome also depends on the dynamics of two types of unobserved shocks: firm level shocks $\varepsilon_{j,t+k}$ and individual level shocks $\mu_{i,t+k}$.

To identify the sequence of dynamic effects of STW $\beta_k$, we first need to control for individual fixed effects $\eta_i$: this is easily done using individual fixed effect panel models. Second, we need to control for individual level characteristics of workers $X$, as they may affect dynamics of labor market: this is done creating proper control groups using nearest-neighbor matching.

The next important concern is that firms who select into STW in $t$ are subject to (unobservable) bad shocks in $t$ ($\varepsilon_{j,t}$), that are possibly quite time persistent, creating a correlation between STW treatment and $\varepsilon_{j,t+k}$. In other words, workers treated by STW will do bad because the firms that trigger STW experience bad shocks. A final issue is the potential correlation between $\mathbb{1}[T_{jt} = 1]$ and $\mu_{i,t+k}$.

A first simple way to address these two concerns is to create counterfactual event studies that put bounds on the values of these firm and individual shocks, and therefore bounds on the treatment effects of STW. Another approach is to use our instrument for STW treatment $T_{jt}$, based on the interaction between firm size and INPS codes.

**Bounds on Dynamic Treatment Effects Using Counterfactual Event Studies** The idea here is to use comparison groups as bounds on the distribution of the unobserved shocks, and therefore bound causal effect of STW.

Intuitively, treated workers at time $t$ are selected on the basis that the firm in which they are employed experiences a negative (unobservable) shock in $t$.

**Counterfactual 1:** Similar worker at time $t-1$ from any non eligible firms due to firm size and INPS code. Because only the worse shocks select into STW, the outcomes for workers in this comparison group can be thought of as an upper bound counterfactual for what would have happened to treated workers in the absence of the program. And the comparison between the event study estimates for treated workers and workers of this first comparison group provide therefore a lower bound estimate on the dynamic treatment effect of STW.

**Counterfactual 2:** Similar worker at time $t-1$ from non eligible firms due to firm size and INPS code that experience mass layoff in $t$. We assume that the shock triggering mass layoff is at least as bad as STW shock and that the firms would have used STW instead if they were eligible. As we show in Section 2.5.1, not all firms who take up STW would have been laying off workers. In that sense, the layoff comparison group is clearly more negatively selected than our treated group. Under this assumption, workers in this mass layoff comparison group can be thought as a lower bound counterfactual for what would have happened to treated workers absent STW. And the comparison between the event study estimates for treated workers and workers of this second comparison group provides an upper bound estimate of the effect of STW.

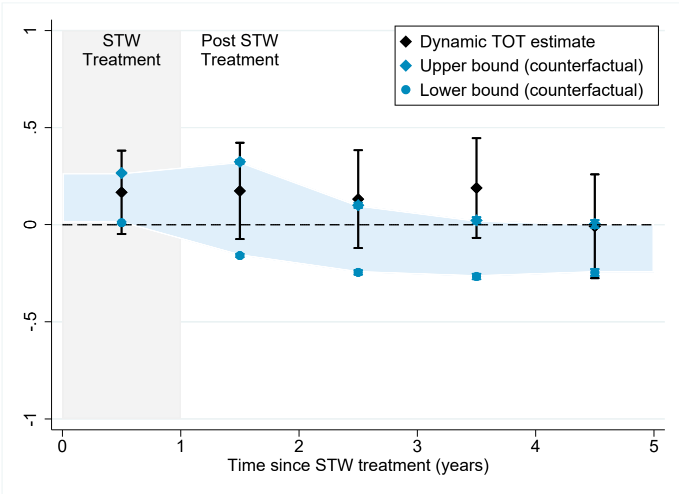
**IV-based Recursive Identification** We can also get a causal estimate of the dynamic effect of STW on workers by implementing a version of our IV recursive identification method used in Section 2.3.4, but focusing on workers outcomes instead of firms. The methodology relies on using the interaction between firm size and INPS code of the firm in which the worker is working in 2008 as an instrument for STW treatment (using the the same controls as in our baseline IV for firms). The intuition for identification is similar to the one in Section 2.3.4. Working in an eligible firm in 2008 ($Z_{2008}$) is an instrument for being treated in 2009. The reduced form regression of outcomes in 2009 on the instrument identifies the contemporaneous effect of STW treatment in 2009. But our instrument $Z_{2008}$ is also correlated with treatment in 2010. The reduced form regression of outcomes in 2010 on the instrument identifies both the one year lagged effect of treatment in 2009 and the contemporaneous effect of treatment in 2010. And so on and so forth. Exploiting this recursive structure, we can back out the dynamic TOT effects of STW on workers outcomes. While this approach has the advantage of identifying the causal dynamic effect of STW on workers, one drawback is that, given the recursive structure of estimates, standard errors are quite large.

**Results** In Figure 2.B3, we overlay the upper bound and lower bound estimates from the event study approach with the IV-based TOT estimate of the dynamic effect of STW treatment. In Panel A, we show the effect for employment, and in Panel B the effect on worker's total gross earnings plus transfers. The graph shows that in both cases, the upper bound estimate, which compares treated workers to their layoff counterfactual, is positive at the time of treatment (event year 0), but quickly converges to being close to zero, as suggested by the event studies in Figure 2.4. The point estimates for the TOT effects are interestingly extremely close to these upper bound estimates, although more imprecisely estimated.

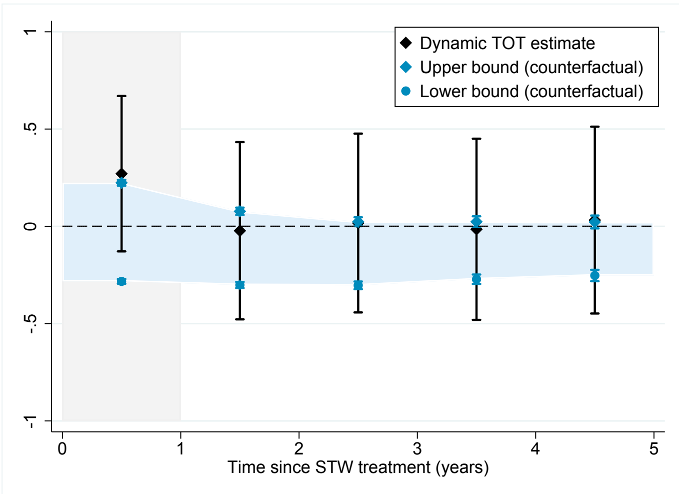
Overall, these results confirm that STW has a positive effect on workers outcomes during treatment and therefore provides short term insurance to workers in firms exposed to shocks. Yet, these effects entirely disappear after treatment when looking at total earnings and transfers, so that STW provides no longer term insurance to workers. In other words, there was no long term beneficial effect of keeping treated workers in firms treated by CIGS during the Great Recession in Italy. This also suggests that there is limited scope for experience effects in the CIGS context, which confirms a stream of evidence on the absence of significant returns to experience for workers treated by active labor market programs.

FIGURE 2.B3: DYNAMIC EFFECTS OF STW ON WORKERS' OUTCOMES:

A. Probability of Employment



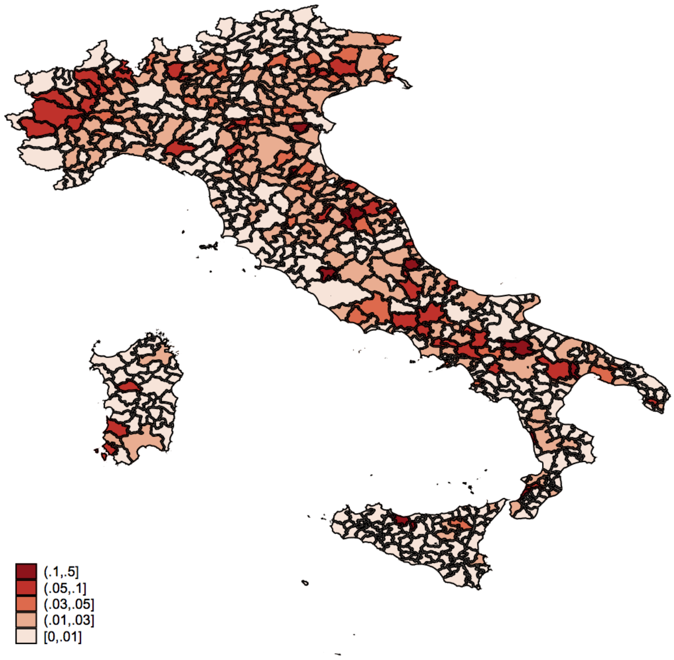

B. Earnings + CIGS/Transfers




**Notes:** The graphs report TOT estimates of the dynamic treatment effect of STW receipt on workers' employment probability and total earnings including social insurance transfers and STW. The coefficients $\hat{\beta}_k^{TOT}$ (indicated by darker solid diamonds) for $k \in [0, ..., 4]$ and are estimated recursively as illustrated in Appendix 2.B. The $\beta_k^{TOT}$ identify dynamic treatment effects of STW receipt at time $k = 0$ on outcomes at time $k \in [0, ..., 4]$. The vertical bars indicate 95% confidence intervals based on bootstrapped standard errors. The shaded area shows upper- and lower-bound estimates of the dynamic effect from the event study graphs reported in Figure 2.4. The upper bound (indicated by lighter solid diamonds) compares treated individuals with the layoff counterfactual. The lower bound (indicated by lighter circles) compares treated workers with workers in non-eligible firms.

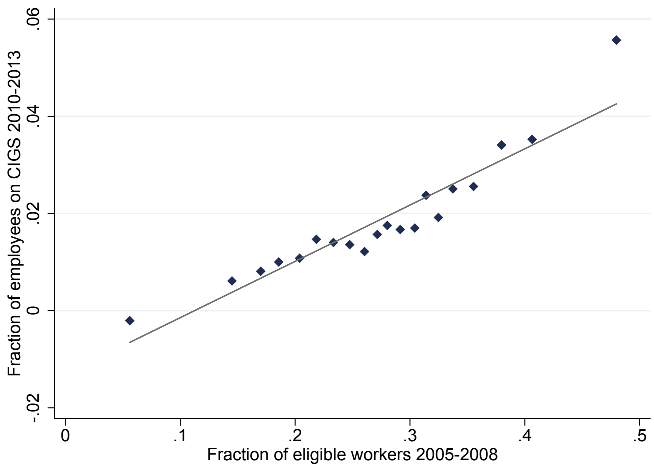
## 2.C Appendix C: Selection and Spillover Effects - Additional Evidence

FIGURE 2.C1: FRACTION OF WORKERS TREATED BY CIGS ACROSS ITALIAN LOCAL LABOR MARKETS (2010-2013)




**Notes:** The graph shows a map of the Italian territory subdivided into 611 local labor markets (LLM), as defined by the Italian Statistical Institute (ISTAT). The graph reports the fraction of workers treated by CIGS in the years 2010 to 2013 by LLM. The fraction of treated workers is defined as the number of workers with at least one STW spell divided by the total number of employees in the LLM.

FIGURE 2.C2: FRACTION OF WORKERS ELIGIBLE TO CIGS IN A LLM BASED ON FIRM SIZE AND INPS CODES DURING THE PRE-RECESSION PERIOD VS FRACTION OF WORKERS ON CIGS DURING THE RECESSION




**Notes:** The graph reports a binned scatter plot of the relationship between the fraction of employees on STW in 2010-2013 and the fraction of workers eligible to STW in 2005-2008 at the local labor market level based on the interaction between firm size and INPS codes. Both variables are residualized on firm level and LLM level controls (see text for details). This relationship corresponds to the first stage of the IV model in equation 2.7.

## 2.D Appendix D: Model - Details and Further Results

### 2.D.1 Firm's Problem

Firms faces productivity shocks $\epsilon_t$. The firm receives profits, $\Pi$, given their production function $F$ and productivity, less labour and hiring costs. There is exogenous separation at rate $\delta$, and hiring costs $c$. The firms vacancy filling probability is $q(\theta_t)$. Firms discount the future at the same rate $\beta$ as workers.

Firms determine every period the number of vacancies to be posted $v_t$ to maximize profits:

$$\Pi(\epsilon_t, n_{t-1}) = \max_{v_t}\{\epsilon_t F(h_t, n_t) - wh_t n_t - cv_t + \beta \mathbb{E}_t[\Pi(\epsilon_{t+1}, n_t)]\} \tag{2.D1}$$

subject to the law of motion of employment:

$$n_t = (1-\delta)\cdot n_{t-1} + q(\theta_t)\cdot v_t \tag{2.D2}$$

The first order condition of profit maximization implicitly determines the demand for employment $n_t = n(\theta_t, h_t, w)$ of the firm.

$$\{\mathbf{n}\}\ \epsilon_t F_n'(h_t, n_t) = wh_t + \frac{c}{q(\theta_t)} - \beta E_t(\Pi_n(\epsilon_{t+1}, n_t)) \tag{2.D3}$$

Given the firm's envelope condition:

$$\Pi_n'(\epsilon_{t+1}, n_t) = (1-\delta)\frac{c}{q(\theta_{t+1})} \tag{2.D4}$$

Therefore the FOC with respect to n, equation (2.D3), becomes:

$$\epsilon_t F_n'(h_t, n_t) = wh_t + \frac{c}{q(\theta_t)} - \beta(1-\delta)\mathbb{E}_t(\frac{c}{q(\theta_{t+1})}) \tag{2.D5}$$

In a stationary equilibrium, $\theta_t = \theta_{t+1} = \theta$, equation (2.D5) reduces to:

$$\epsilon_t F_n'(h_t, n_t) = wh_t + (1-\beta(1-\delta))\frac{c}{q(\theta)} \tag{2.D6}$$

### 2.D.2 Characterization of Hours Schedule and Employment Responses to STW

Optimal employment is determined by FOC (2.D6) above of maximization of total profits w.r.t. to vacancies. This determines employment as a function of hours, wages, and tightness.

Workers and firms bargain over the surplus created by realized matches, which translates into an hours schedule and a wage schedule. When firms and workers bargain over hours and wages there are a large number of general hours and wage schedules: $h(w, \theta, \epsilon, n, b, \tau, t)$ and $w(h, \theta, \epsilon, n, b, \tau, t)$ that offer potential solutions to the bargaining problem, because hours and wages are imperfect substitutes in both sides' objective functions. But since each side receives a share of the overall surplus, given an agreed wage, the level of agreed hours must maximize the shared surplus. Note that this does not coincide with Nash's Pareto efficiency axiom for a bargaining solution because it may be possible to make both sides better off with a different wage and hours schedule. But if wages are determined by some form of rigid rule, which corresponds to most wage bargaining setting, then such an hours solution seems plausible. In practice, in Italy, wages are negotiated by industry-wide bargaining over fixed periods of time (of approximately three years).

Given a particular fixed wage rule $w(h, \theta, \epsilon, n, b, \tau, t)$ we can now characterize hours schedule conditional on this particular wage schedule. To see how the implied hours schedule behaves when it is determined in this manner, we first reanalyze the worker continuation values

$$
\begin{aligned}
W_H =& u(c_H, h_H) + \beta[\delta W_U + (1-\delta) W_H], \\
W_L =& u(c_L, h_L) + \beta[\delta W_U + (1-\delta) W_L], \\
W_U =& u(c_U, 0) + \beta[(1-\phi(\theta)) W_U + \phi(\theta)\{\rho W_H + (1-\rho) W_L\}]
\end{aligned}
$$

Referring to $W_H, W_L$, and $W_U$ as unknowns; we have three equations and three unknowns. Let us solve out for the unknowns

$$
\begin{aligned}
W_H =& \frac{F(\theta)}{1-\beta(1-\delta)}\left[\beta\delta u(c_U, h_U) + \left\{1 + \frac{\beta^2\delta\phi(\theta)\rho}{1-\beta(1-\delta)}\right\} u(c_H, h_H) + \frac{\beta^2\delta\phi(\theta)(1-\rho)}{1-\beta(1-\delta)} u(c_L, h_L)\right], \\
W_L =& \frac{F(\theta)}{1-\beta(1-\delta)}\left[\beta\delta u(c_U, h_U) + \frac{\beta^2\delta\phi(\theta)\rho}{1-\beta(1-\delta)} u(c_H, h_H) + \left\{1 + \frac{\beta^2\delta\phi(\theta)(1-\rho)}{1-\beta(1-\delta)}\right\} u(c_L, h_L)\right], \\
W_U =& F(\theta)\left[u(c_U, h_U) + \frac{\beta\phi(\theta)\rho}{1-\beta(1-\delta)} u(c_H, h_H) + \frac{\beta\phi(\theta)(1-\rho)}{1-\beta(1-\delta)} u(c_L, h_L)\right]
\end{aligned}
$$

where $F(\theta) = \frac{1-\beta(1-\delta)}{(1-\beta(1-\phi(\theta))(1-\beta(1-\delta)) - \beta^2\delta\phi(\theta)}$. So the continuation values are a convex combination of instantaneous utility from different states, where the weighting depends on an endogenous variables - labor market tightness. Using previous notation, we can represent this in matrix form

$$
W = \Lambda' U,
$$

where $W = [W_H, W_L, W_U]'$ and $U = [u(c_H, h_H), u(c_L, h_L), u(c_U, h_U)]'$. From this, we can see that

$$\begin{aligned}
W_i - W_U =& \frac{(1-\beta\phi(\theta))u(c_i,h_i) - (1-\beta)u(c_u,0)}{1-\beta(1-\delta-\phi(\theta))}, \\
\Rightarrow \frac{d(W_i - W_U)}{dh_i} =& (1-\beta(1-\delta-\phi(\theta)))^{-2}\Big\{[1-\beta(1-\delta-\phi(\theta))]\dots \\
& \dots\left[(1-\beta\phi(\theta))(w_i(1-\tau_i)u_c(c_i,h_i) + u_h(c_i,h_i)) - \beta u(c_i,h_i)\frac{d\phi(\theta)}{dh_i}\right]\dots \\
& \dots - [(1-\beta\phi(\theta))u(c_i,h_i) - (1-\beta)u(c_u,0)]\beta\frac{d\phi(\theta)}{dh_i}\Big\}
\end{aligned}$$

Since the agents considering the bargaining process are atomistic relative to the total size of the economy, we can ignore general equilibrium effects on market tightness

$$\begin{aligned}
\frac{d(W_i - W_U)}{dh_i} =& \eta(\theta)[w_i(1-\tau_i)u_c(c_i,h_i) + u_h(c_i,h_i)], \\
\Rightarrow \frac{d^2(W_i - W_U)}{dh_i^2} =& \eta(\theta)[w_i^2(1-\tau_i)^2 u_{cc}(c_i,h_i) + u_{hh}(c_i,h_i)], \\
\Rightarrow \frac{d^2(W_L - W_U)}{dh_L d\tau} =& \eta(\theta)\left[-w_L u_c(c_L,h_L) - w_L^2(1-\tau_L)h_L u_{cc}(c_L,h_L) + \frac{dh_L}{d\tau_L}u_{hh}(c_L,h_L)]\right], \\
\Rightarrow \frac{d^2(W_H - W_U)}{dh_H d\tau} =& 0, \\
\Rightarrow \frac{d^2(W_i - W_U)}{dh_i d\varepsilon_i} =& \eta(\theta)\left[\frac{dw_i}{d\varepsilon_i}(1-\tau_i)u_c(c_i,h_i)\dots\right. \\
& \dots + w_i(1-\tau_i)\left[\frac{dw_i}{d\varepsilon_i}(1-\tau_i)h + w(1-\tau)\frac{dh_i}{d\varepsilon_i}\right]u_{cc}(c_i,h_i) \\
& \left. + \frac{dh_i}{d\varepsilon_i}u_{hh}(c_i,h_i)]\right]
\end{aligned}$$

where $\eta(\theta) = \frac{1-\beta\phi(\theta)}{1-\beta(1-\delta-\phi(\theta))}$ and we assume separability in hours and consumption. Therefore, assuming as a benchmark that a worker is on their neoclassical intratemporal first order condition, the difference $W_i - W_U$ increases with $h_i$ if the probability of finding a job decreases in $h_i$.

If we assume that productivity follows a martingale process or that we are in a stationary environment, the surplus for a match between a worker and a level $i$ productivity firm is

$$S_i = W_i - W_U + \varepsilon_i F_n(h_i, n_i) - w_i h_i$$

As explained above, for a given wage schedule this should be maximized with respect to $h_i$

$$\frac{dS_i}{dh_i} = \frac{d(W_i - W_U)}{dh_i} + \varepsilon_i F_{nh}(h_i, n_i) + \varepsilon_i F_{nn}(h_i, n_i)\frac{dn_i}{dh_i} - w_i - h_i\frac{dw_i}{dh_i} = 0$$

This equation implicitly determines a level of hours, so given a unique solution we can do comparative statics. Firstly, looking at the STW the $\tau$ parameter, and assuming that third derivatives of the production function are small, we have,

$$\frac{dh_i}{d\tau} = \frac{-\left[\frac{d^2(W_i - W_U)}{dh_i d\tau} + \varepsilon_i F_{nn}(h_i, n_i)\frac{d^2 n_i}{dh_i d\tau} - \frac{dw_i}{d\tau} - \frac{d^2 w_i}{dh_i d\tau}h_i\right]}{\frac{d^2(W_i - W_U)}{dh_i^2} + \varepsilon_i F_{nn}(h_i, n_i)\frac{d^2 n_i}{dh_i^2} - \frac{d^2 w_i}{dh_i^2}h_i - 2\frac{dw_i}{dh_i}}$$

Now looking at the productivity parameter $\varepsilon_i$, we find:

$$\frac{dh_i}{d\varepsilon_i} = \frac{-\left[\frac{d^2(W_i - W_U)}{dh_i d\varepsilon_i} + F_{nh}(h_i, n_i) + F_{nn}(h_i, n_i)\frac{dn_i}{dh_i} + \varepsilon_i F_{nn}(h_i, n_i)\frac{d^2 n_i}{dh_i d\varepsilon_i} - \frac{dw_i}{d\varepsilon_i} - \frac{d^2 w_i}{dh_i d\varepsilon_i}h_i\right]}{\frac{d^2(W_i - W_U)}{dh_i^2} + \varepsilon_i F_{nn}(h_i, n_i)\frac{d^2 n_i}{dh_i^2} - \frac{d^2 w_i}{dh_i^2}h_i - 2\frac{dw_i}{dh_i}}$$

So hours will react more strongly to the subsidy rate and firms experiencing productivity drops $d\varepsilon_i$ will want to reduce hours and take up the program more when the following conditions are met: (i) utility gain from employment $W_i - W_U$ is more strongly decreasing in hours; (ii) wages are relatively rigid w.r.t the subsidy rate, and (iii) technology closer to linear in headcount employment but more concave in hours.

Note that we can also look at the reaction of employment to different variables by taking the firms' first order condition for employment in a stationary environment:

$$A_i[\varepsilon_i F_n(h_i, n_i) - w_i h_i] = \frac{1}{q(\theta)}$$

where $A_H = \frac{\rho(1-\beta(1-\delta))}{c_v}$ and $A_L = \frac{(1-\rho)(1-\beta(1-\delta))}{c_v}$. So total differentiation gives us

$$\frac{dn_i}{dh_i} = \frac{w_i + h_i\frac{dw_i}{dh_i} - \varepsilon_i F_{nh}(h_i, n_i)}{\varepsilon_i F_{nn}(h_i, n_i)}$$

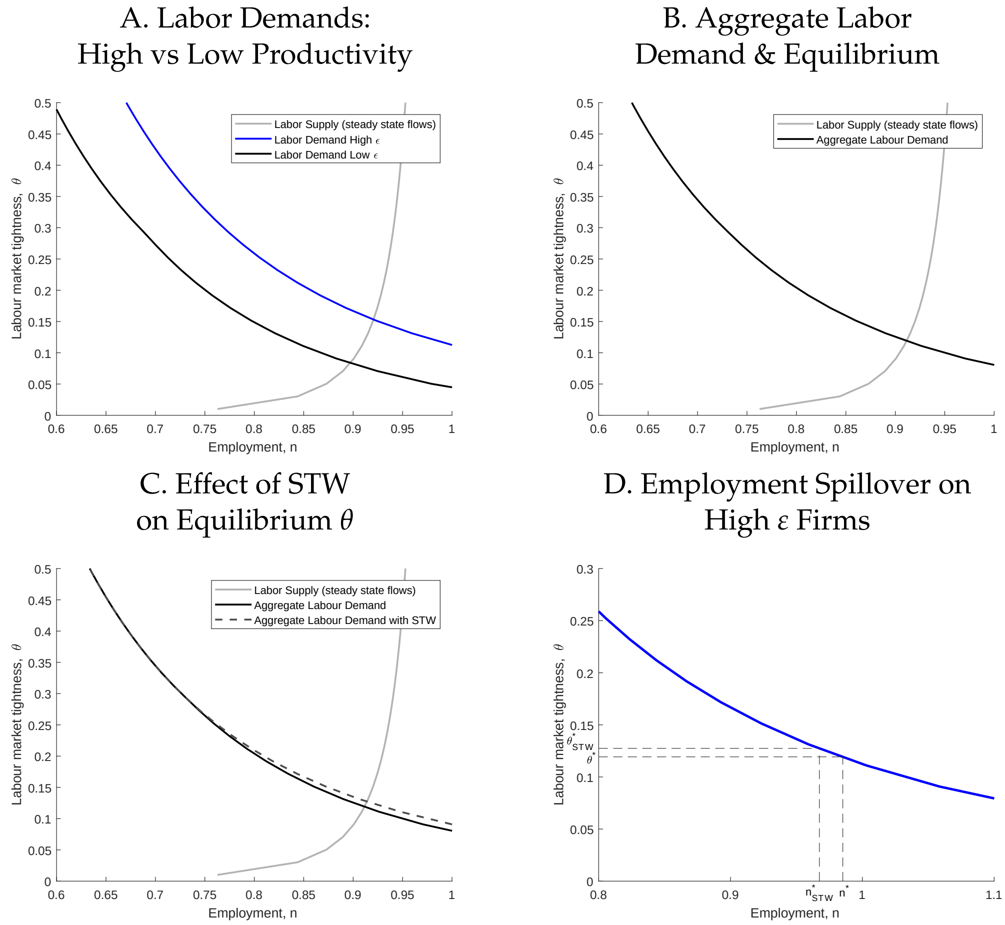
where we use the fact that firms do not internalize their impact on market tightness. This implies that positive employment responses to the STW-induced reductions in hours will occur when wages are above the marginal product of an additional input of hour. Additionally, these responses will be larger the more rigid wages are as a function of hours.

### 2.D.3 Equilibrium and Spillover Effects

A steady state equilibrium consists in a set of: (i) hours schedules $h$ and wage schedules $w$ that split the surplus in high and in low productivity firms subject to the incentive constraint that $W_k^e - W^u \geq 0$; (ii) labor demand functions $n^d$ in high and in low productivity firms that maximizes firms' profits and (iii) a labor market tightness $\theta$ that clears the labor market subject to the steady state equality of flows in and out of employment. We borrow the equilibrium representation of Michaillat (2012). A graphical illustration, using the calibrated version of our model, is presented in Figure 2.D1 below.

In this representation, the steady state equality of flows in and out of employment characterizes a labor supply $n^s(\theta, \delta)$, which is an increasing function of $\theta$ in the $\{n, \theta\}$ space. The profit maximization of firms determines a labor demand supply $n^d(\theta)$, which will be a decreasing function of $\theta$ when the marginal product of $n$ is decreasing, and horizontal otherwise (Panel A). With random matching, aggregate labor demand is simply the weighted sum of demands of high and low productivity firms. Equilibrium tightness and equilibrium employment are determined at the intersection of aggregate demand and supply (Panel B). When STW is introduced, labor demand of low productivity firms increases, especially so when tightness is low (and hiring is therefore cheap). This in turn increases aggregate demand, and equilibrium tightness (Panel C). This increase in equilibrium tightness is the force driving our observed spillover effects in the data. It makes hiring more costly for all firms, and therefore reduces employment of firms non treated by STW. This equilibrium mechanism captures the negative reallocation effects of STW which distorts employment towards low productivity firms rather than high productivity firms. Again, this effect will be stronger the more horizontal labor demands are (that is, the more linear technology is in $n$) (Panel D).

FIGURE 2.D1: EQUILIBRIUM REPRESENTATION AND SPILLOVER EFFECTS OF STW




**Notes:** The figure offers a graphical illustration of labor market equilibrium using the calibrated version of our model. In this representation, the steady state equality of flows in and out of employment characterizes a labor supply $n^s(\theta, \delta)$, which is an increasing function of $\theta$ in the $\{n, \theta\}$ space. The profit maximization of firms determines a labor demand supply $n^d(\theta)$, which will be a decreasing function of $\theta$ when the marginal product of $n$ is decreasing, and horizontal otherwise (Panel A). With random matching, aggregate labor demand is simply the weighted sum of demands of high and low productivity firms. Equilibrium tightness and equilibrium employment are determined at the intersection of aggregate demand and supply (Panel B). When STW is introduced, labor demand of low productivity firms increases, especially so when tightness is low (and hiring is therefore cheap). This in turn increases aggregate demand, and equilibrium tightness (Panel C). This increase in equilibrium tightness is the force driving our observed spillover effects in the data. It makes hiring more costly for all firms, and therefore reduces employment of firms non treated by STW. This equilibrium mechanism captures the negative reallocation effects of STW which distorts employment towards low productivity firms rather than high productivity firms. This effect will be stronger the more horizontal labor demands are (that is, the more linear technology is in $n$) (Panel D).

## 2.E Appendix E: Optimal STW Formula - Derivation and Calibration

### 2.E.1 Derivation of Proposition 1

The social planner problem is

$$\begin{aligned}\max_{\mathbb{T}}\ \mathcal{L}(\mathbb{T}) &= t+\pi_H+\pi_L-Ub-\tau n_L w(1-h_L)+\frac{1}{\mu}\Gamma(W_H,W_L,W_U,n_H,n_L)\\ \text{s.t (1) } &(1-\beta(1-\delta))[\epsilon_H F_n(h_H,n_H)-wh_H]=\frac{c_v}{\rho q(\theta)},\\ \text{(2) } &(1-\beta(1-\delta))[\epsilon_L F_n(h_L,n_L)-wh_L]=\frac{c_v}{(1-\rho)q(\theta)},\\ \text{(3) } &n=\frac{\phi(\theta)}{\delta+\phi(\theta)}\end{aligned}$$

where $n=n_H+n_L$ and $U=1-n$.

Firstly, we focus on the social welfare effects,

$$\begin{aligned}\frac{1}{\mu n_L w(1-h_L)}\left\{\frac{d\Gamma(W_H,W_L,W_U,n_H,n_L)}{d\tau}\right\}=&\frac{1}{\mu n_L w(1-h_L)}\left\{n_H\frac{dW_H}{d\tau}+n_L\frac{dW_L}{d\tau}\right.\\ &+(1-n)\frac{dW_U}{d\tau}\\ &\left.+\frac{dn_H}{d\tau}[W_H-W_U]+\frac{dn_L}{d\tau}[W_L-W_U]\right\}\end{aligned}$$

In a steady state equilibrium, $n_H=\rho n$ and $n_L=(1-\rho)n$ so we can rewrite the above

$$\begin{aligned}\frac{1}{\mu n_L w(1-h_L)}\left\{\frac{d\Gamma(W_H,W_L,W_U,n_H,n_L)}{d\tau}\right\}=&\frac{1}{\mu n_L w(1-h_L)}\left\{n\left[\rho\frac{dW_H}{d\tau}+(1-\rho)\frac{dW_L}{d\tau}\right]\right.\\ &+(1-n)\frac{dW_U}{d\tau}\\ &\left.+\frac{dn}{d\tau}[\rho W_H+(1-\rho)W_L-W_U]\right\}\end{aligned}$$

In order to expand and interpret this equation we define a matrix

$$\begin{aligned}\Lambda=&\begin{bmatrix}\lambda_{H|H} & \lambda_{L|H} & \lambda_{U|H}\\ \lambda_{H|L} & \lambda_{L|L} & \lambda_{U|L}\\ \lambda_{H|U} & \lambda_{L|U} & \lambda_{U|U}\end{bmatrix}=[\Lambda_H:\Lambda_L:\Lambda_U]\\ =&[1-\beta(1-\delta-\phi(\theta))]^{-1}\begin{bmatrix}1-\beta(1-\rho\phi(\theta)) & \beta(1-\rho)\phi(\theta) & \beta\delta\\ \beta\rho\phi(\theta) & 1-\beta(1-(1-\rho)\phi(\theta)) & \beta\delta\\ \beta\phi(\theta)\rho & \beta\phi(\theta)(1-\rho) & 1-\beta(1-\delta)\end{bmatrix}\end{aligned}$$

These new objects have a simple interpretation, $\lambda_{H|L}$ is the proportion of time spent employed in the high productivity firm over the lifetime of a worker who is currently employed in the low productivity firm. Similarly, $\lambda_{U|H}$ is the proportion of time spent unemployed over the lifetime of a worker who is currently employed in the low productivity firm, and so on.

We define another vector to account for the proportions of employed - in the high and low productivity firms - and unemployed workers

$$\mathcal{N} = \frac{1}{1-\beta}\begin{bmatrix} \rho n \\ (1-\rho)n \\ 1-n \end{bmatrix}$$

where the scalar multiplication will account for the fact that we are dealing with infinite streams of utility.

Lastly, we define the following scalars

$$\begin{aligned}
\Omega_i &= \mathcal{N}'\Lambda_i \qquad \text{for } i = H, L \\
\Delta &= \frac{\rho u(c_H, h_H) + (1-\rho)u(c_L, h_L) - u(c_U, 0)}{1-\beta(1-\delta-\phi(\theta))}\frac{1}{\mu}, \\
\Phi'(\theta) &= \phi'(\theta)\left[\frac{U}{\delta+\phi(\theta)} + \Omega_U\right], \\
E &= \tau n_L w(1-h_L), \\
u_i^c &= \frac{du(c_i, h_i)}{dc} \qquad \text{for } i = H, L, \\
\mathbb{G}_i &= \frac{\Omega_i}{n_i}\frac{u_i^c}{\mu} \qquad \text{for } i = H, L
\end{aligned}$$

where $E$ is total expenditure on the policy. $\Delta$ is the weighted wedge in utility between being employed and unemployed. $\mathbb{G}_i$ is the adjusted marginal social welfare weights on either the employed in the low or high productivity firm. The adjustment comes from the $\frac{\Omega_i}{n_i}$ term, which accounts for the fact that individuals spend different amounts of time in the high or low productivity firm. If on average more time is spent in, say, the low productivity firm, then the social planner places greater weight on welfare in this state.

Now we can rewrite the original expression

$$\begin{aligned}
\frac{1}{\mu n_L w(1-h_L)}\left\{\frac{d\Gamma(W_H, W_L, W_U, n_H, n_L)}{d\tau}\right\} = &\varepsilon_{h_H,\tau}\frac{n_H \cdot h_H\left[w - MRS_{c,h}^H\right]}{E}\mathbb{G}_H\frac{1-\rho}{\rho} + \\
&\varepsilon_{n_L \cdot h_L,\tau}\frac{h_L\left[w(1-\tau) - MRS_{c,h}^L\right]}{E}\mathbb{G}_L + \\
&\varepsilon_{\theta,\tau}\frac{\theta\Phi'(\theta)\Delta}{E} + \mathbb{G}_L
\end{aligned}$$

where $MRS_{c,h}^i = \frac{u_c'(c_i,h_i)}{u_h'(c_i,h_i)}$ is the marginal rate of substitution between consumption and hours for workers in firms of productivity level $i$. Let $\varepsilon_{Y,X} = \frac{dY}{dX}\frac{X}{Y}$ denote the elasticity of

$Y$ w.r.t. $X$. Therefore, the overall optimality condition is

$$\begin{aligned}1-\left[\frac{\varepsilon_{\pi_H,\tau}\pi_H+\varepsilon_{\pi_L,\tau}\pi_L}{E}+\varepsilon_{h_L,\tau}\frac{h_L}{1-h_L}+\varepsilon_{n,\tau}\frac{b-\tau(1-\rho)w(1-h_L)}{\tau w(1-h_L)}\right]=&\\ \varepsilon_{h_H,\tau}\frac{n_H\cdot h_H\left[w-MRS^H_{c,h}\right]}{E}\mathbb{G}_H\frac{1-\rho}{\rho}&\\ +\varepsilon_{h_L,\tau}\frac{h_L\left[w(1-\tau)-MRS^L_{c,h}\right]}{E}g_L&\\ +\varepsilon_{\theta,\tau}\frac{\theta\Phi'(\theta)\Delta}{E}+\mathbb{G}_L&\end{aligned}$$

We assume, based on empirical evidence, that $\varepsilon_{h_H,\tau}\approx 0$. Using this and rewriting the effect on profits into the externalities component yields

$$\begin{aligned}1+\left\{\varepsilon_{n,\tau}\frac{(1-\rho)b^{STW}-b}{b^{STW}}-\varepsilon_{h_L,\tau}\frac{h_L}{\bar{h}-h_L}\right\}=&\varepsilon_{h_L,\tau}\frac{n_L\cdot h_L}{E}\\ &\cdot\left\{[w(1-\tau)-MRS^L_{c,h}]\mathbb{G}_L+(F^h_L-n_L)\right\}\\ &+\varepsilon_{\theta,\tau}\frac{\theta}{E}\left\{\Phi'(\theta)\Delta+q'(\theta)\mathcal{C}\right\}+\mathbb{G}_L\end{aligned}$$

where $b^{STW}=\tau w(\bar{h}-h)$ is the total amount of STW benefits for a worker in the program. $F^h_L=\frac{\epsilon_L}{n_L}\cdot\frac{\partial F(h_L,n_L)}{\partial h}$ is the marginal product of an increase in hours in low productivity firms and $\mathcal{C}=c\cdot v$ is total recruiting costs.

### 2.E.2 Local Calibration of Optimal STW Using Reduced-Form Estimates

**Value of Transfer** To calibrate the social value of transfer $\mathbb{G}_L=\frac{\Omega_L}{n_L}\frac{u'_c(c_L,h_L)}{\mu}$, we first focus on the term $\frac{u'_c(c_L,h_L)}{\mu}$. This term refers to the marginal utility of consumption of treated workers ($u'_c(c_L,h_L)$) relative to that of the whole population of workers $\mu=\mathbb{E}[u'_c(c,h)]$. To calibrate this, we use our event studies estimates of Figure 2.4 Panel C. They show that treated workers have total earnings and transfers that are significantly below ($\approx$ 18% lower) that of their matched non-treated workers from non eligible firms. This difference between treated workers and the matched non-treated workers from non eligible firms represent the difference in total earnings and transfer between treated workers and counterfactual average workers in the economy. To translate this difference in earnings and transfer into a difference in marginal utility of consumption, we further assume that workers do not have access to additional self insurance, and that utility is separable between hours and consumption. Using a simple first-order Taylor expansion, we have that $\frac{u'_c(c_L,h_L)}{\mu}=\frac{u'_c(c_L,h_L)}{\mathbb{E}[u'_c(c,h)]}\approx 1+\frac{u'_c(c_L,h_L)-u'_c(\bar{c},h)}{u'_c(\bar{c},h)}\approx 1+\sigma_c\frac{\bar{c}-c_L}{\bar{c}}$. Using our estimates, this suggests that the wedge in marginal utility is approximately equal to $1+\sigma_c\cdot 0.18$, where $\sigma_c$ is the coefficient of relative risk aversion. The value of STW transfers is therefore potentially large. Assuming a coefficient of risk aversion of 2.5, (which is somewhat of the mid point of the accepted range of estimates in the literature), we get that $\frac{u'_c(c_L,h_L)}{\mu} \approx 1.45$ . To calibrate $\frac{\Omega_L}{n_L}$ we use the definition of $\Omega_L$ and the observed values of time spent in the various states conditional on today's state and find $\frac{\Omega_L}{n_L} \approx 1$. As a result, our local calibration delivers $\mathbb{G}_L \approx 1.45$.

**Fiscal Externality** We start with calibrating the first term of the fiscal externality, capturing the fiscal cost of employment responses: $\varepsilon_{n,\tau}\frac{(1-\rho)b^{STW}-b}{b^{STW}}$.

This fiscal cost depends on the relative generosity of UI vs STW benefits: $\frac{(1-\rho)b^{STW}-b}{b^{STW}}$ To calibrate this, we set the unemployment benefit, $b$ to match the net replacement rate for the average worker in Italy in 2008, which is around 70%. For our purposes, this is 70% of the wage obtained if working the full hours endowment $\bar{h} = 40$, i.e. $b = 0.7 \cdot w \cdot \bar{h}$. We set $\tau$ at its current level of 80%. $\rho$ is the fraction of total employment from low productivity firms. To define low productivity firms, we use the fraction of eligible firms taking up STW during the Great Recession from 2009 to 2014= 13%. $h = 20$ is set to the average level of hours observed in these low productivity firms during the Recession. Using these values, we have that $\frac{(1-\rho)b^{STW}-b}{b^{STW}} = -1.62$.

We now calibrate the employment elasticity $\varepsilon_{n,\tau}$. We can decompose total employment response between low and high productivity employment responses $\frac{dn/n}{d\tau/\tau} = (1-\rho)\frac{dn_L/n_L}{d\tau/\tau} + \rho\frac{dn_H/n_H}{d\tau/\tau}$ To calibrate $\frac{dn_L/n_L}{d\tau/\tau}$ we use our estimate of the effect of STW on employment= $e^{.38} - 1$=.45 To calibrate $\frac{dn_H/n_H}{d\tau/\tau}$ we use our spillover estimates: we know that employment decreases by 0.93% when the fraction treated increases by 1%. We also know that the availability of STW policy increases the fraction treated by 5% (our first stage estimate from Table 2.1). This means that $\frac{dn_H/n_H}{d\tau/\tau} = 0.0093 \cdot 5$.

Using again the fact that the fraction of low productivity firm is $1-\rho$ =13% (which corresponds to the fraction of eligible firms ever taking up STW during recession), we have that the total employment response to the policy is $\varepsilon_{n,\tau} = 0.13 \cdot 0.45 - 0.87 \cdot (0.0093 \cdot 5) = 1.8\%$.

The second term of the fiscal externality captures the hours response to the program: $-\varepsilon_{h_L,\tau}\frac{h_L}{\bar{h}-h_L}$. We can easily calibrate this term using the large estimated negative responses of hours to the policy from Section 2.3.2: $\varepsilon_{h_L,\tau} = e^{-0.51} - 1 = 0.4$. Using again $\bar{h} = 40$ and $h$=20, we find that the fiscal externality created by the hours response to the program is large and equal to 0.4, which trumps the small fiscal externality from employment responses.

The total fiscal externality, combining the employment and hours response is: $0.018 \cdot (-1.62) + 0.4 = 0.37$.

**Employment externality** To get a sense of the sign and magnitude of the employment externality, we proceed in two steps.

First, we calibrate the parameters of a Cobb-Douglas matching function from our reduced-form evidence to get a sense of the relative magnitude of $\Phi'(\theta)$ and $q'(\theta)$. All the details of this calibration exercise are reported below in Appendix 2.F. Our estimate of the curvature of the matching function is 0.53. At low levels of tightness, such as during a recession, this curvature parameter implies that $\Phi'(\theta) > q'(\theta)$.

Second, we turn to our event study estimates to get an idea of $\Delta$, the lifetime utility gain from employment vs unemployment today. We use an approach similar to Shimer and Werning (2007) and look at the average drop in wage rate at reemployment vs prior to unemployment for unemployed workers. This information tells us the willingness-to-pay to be employed vs unemployed, and provides an estimate of $\Delta$. In our context, this drop is relatively large with an average wage rate drop around 8% at reemployment compared to pre-unemployment wage rate. How does $\Delta$ compare to $\mathcal{C}$, the total recruitment cost of an additional worker? We have very little evidence on $\mathcal{C}$. The most recent evidence from Mühlemann and Strupler Leiser (2015) using Swiss data, suggests recruiting costs vary between less than 2% of wages for large firms to around 20% for very small firms, and are sensitive to the business cycle. Overall, if we assume that $\mathcal{C}$ is actually close to $\Delta$, the employment externality remains positive to the extent that $\Phi'(\theta) > q'(\theta)$.

**Hours Externality** Signing and calibrating the hours externality term remains tricky in practice.

We proceed in two steps. First, we examine the term $[F_L^h - w]$. By implicitly differentiating the FOC of firms profit maximization w.r.t employment, we get that the employment response of low productivity firms to their change in hours with STW is an increasing function of the wedge between wages and the marginal product of hours: $\frac{dn_L}{dh_L} = f(w - F_L^h)$. In other words, the large observed employment responses to STW indicate that $[F_L^h - w] < 0$ and that the wedge is potentially large in magnitude.

Second, we examine the second term $w(1-\tau) - MRS_{c,h}^L$. When workers freely choose hours in a complete market for hours, their optimal choice of hours is such that the MRS is equal to the net-of-tax wage rate. Interestingly, there is ample evidence that the fraction of workers reporting that they are willing to work more hours increases drastically during recessions (e.g. Canon, Kudlyak, and Reed, 2014). This would indicate that recessions are actually characterized by $w(1-\tau) - MRS_{c,h}^L \geq 0$. In which case decreasing equilibrium hours has a negative externality on workers.

## 2.F Appendix F: Model Calibration and Counterfactual Analysis

The following appendix describes the details of the calibration of the model: the choice of functional form specifications, the calibration of the various parameters using quasi-experimental evidence, the GMM estimation of the parameters that could not be directly calibrated from reduced-form evidence, and the details of the counterfactual exercises.

### 2.F.1 Exogenous Parameters

| Parameter | Description | Calibrated value |
|---|---|---|
| $\beta$ | Discount factor | 0.935 |
| $\alpha$ | Hour share | 0.6 |
| $\eta$ | Labour share | 0.7 |
| $\gamma$ | Matching function curvature | 0.53 |
| $w_a$ | Wage function curvature | 0.2 |
| $\bar{h}$ | Total hours endowment | 40 |
| $\delta$ | Separation rate | 0.05 |
| $b$ | Unemployment benefit | $0.7 \cdot \bar{h} \cdot w_s$ |
| $\tau$ | STW replacement rate | 0.8 |
| $\sigma_c$ | Coefficient of risk aversion | 2.5 |
| $\sigma_h$ | Inverse of Frisch elasticity of labour supply | 3.5 |
| **ß** | Markov transition matrix of firm productivity | [0.88 0.12; 0.88 0.12] |
| $\epsilon$ | Productivity values | [1 1.62] |

#### Matching Function, $\gamma$

We consider the Cobb-Douglas matching function:

$$M(u_t, v_t) = \mu u_t^{\gamma} v_t^{1-\gamma} \tag{2.F1}$$

The vacancy filling probability $q(\theta)$ is therefore, as above:

$$q(\theta_t) = \frac{M(u_t, v_t)}{v_t} = \mu\left(\frac{u_t}{v_t}\right)^{\gamma} = \mu\theta_t^{-\gamma} \tag{2.F2}$$

Log linearizing the above equation yields:

$$\ln(\frac{M}{v_t}) = \ln(\mu) - \gamma ln(\theta) \tag{2.F3}$$

To obtain information on the measures of hires per vacancy, $M/v_t$, and labor market tightness at the local labor market level, $\theta$ we use the RIL 2007, 2010 and 2015 surveys from INAPP. Using question C7 (and question C8 for 2015) we can compute $v_{j,t}^{RIL}$ the total number of vacancies (number of individuals the firm seeks to hire) in the RIL data at time $t$ in labor market $j$.

To scale the vacancies in the RIL data to the whole local labor market level, we use the ratio of total employment of firms in the RIL data at time $t$ in labor market $j$ to total employment at time $t$ in labor market $j$ computed from the INPS administrative data, that is we have:

$$v_{j,t} = \frac{n_{j,t}}{n_{j,t}^{RIL}} \cdot v_{j,t}^{RIL} \tag{2.F4}$$

Once a measure of vacancies $v_{j,t}$ is obtained, this is combined with measures of matches $M_{j,t}$ and of unemployment $u_{j,t}$ from the ISTAT data to create $q_{j,t}$ and $\theta_{j,t}$. For $M_{j,t}$ we compute the total number of new hires (inflows) in firms of LLM $j$ in year $t$ from the INPS data, and for $u_{j,t}$ we compute the total number of unemployed in LLM $j$ at time $t$ from the INPS data on paid unemployment.

We therefore can run the following specification:

$$\log q_{j,t} = a + b \log(\theta_{j,t}) + c_j + \zeta_t + \nu_{j,t} \tag{2.F5}$$

For $b$ to identify $-\gamma$, exogenous variation in $\theta_{j,t}$ is required. We use exposure to CIG treatment as an instrument. Intuitively, the intensity of CIG treatment offers an exogenous shock to labor demand in the LLM as depicted in Figure 2.D1 Panel C. This shock allows us to move along the "supply curve" of steady state equality of flows in the labor market, and therefore identify the curvature of the matching function. We use again the interaction between firm size and INPS codes in the pre-recession period as an instrument for the change in the number of unemployed (and therefore for the change in tightness) during the recession. Therefore, we obtain the 2SLS model:

$$\begin{aligned} \Delta \log q_{j,t} &= b\Delta \log(\hat{\theta}_{j,t}) + W_j' \mu_1 + \zeta_t + \nu_{j,t} \\ \Delta \log(\theta_{j,t}) &= Z_j^{2005-2008} + W_j' \mu_0 + \mu_{j,t} \end{aligned} \tag{2.F6}$$

where $\Delta$ is the difference operator between pre vs post 2008.[39] $Z_j$ is the average yearly fraction of workers of LLM $j$ that are eligible to STW during the pre-recession period based on the interaction between their firm size and INPS code in the pre-recession period. $W_j$ is a vector of LLM characteristics that could be correlated with the fraction of treated workers and likely to affect equilibrium labor market outcomes during the recession, such as the industry and firm size composition of the LLM and the initial unemployment rate in the LLM prior to the recession. Identification therefore comes from comparing LLM with similar characteristics, including firm size composition and industry composition, but with different allocations of workers within firm size times INPS codes bins during the pre-recession period. From this specification, we obtain $\gamma = 0.53$.

[39]Because only three waves of the survey are available (2007, 2010 and 2015), the pre 2008 data are observations for 2007, and post 2008 data are an average of the 2010 and 2015 observations.

**Production Function, $\alpha$ and $\eta$**

We assume that the production function of the firm is of the form:

$$F(h_t, n_t) = h_t^{\alpha} n_t^{\eta} \tag{2.F7}$$

Log-linearization of the first order condition of the firm's profit maximization with respect to employment gives:

$$\log n = \frac{\alpha}{1-\eta} \log h - \frac{1}{1-\eta} \log(wh) - \frac{1-\beta(1-\delta)}{1-\eta} \frac{c}{whq(\theta)} + \frac{1}{1-\eta} \log(\varepsilon\eta) \tag{2.F8}$$

Letting $\nu = \frac{1}{1-\eta} \log(\varepsilon\eta)$, and re-arranging we obtain:

$$\log n = \frac{\alpha-1}{1-\eta} \log h - \frac{1}{1-\eta} \log w - \frac{1-\beta(1-\delta)}{1-\eta} \frac{c}{whq(\theta)} + \nu \tag{2.F9}$$

A third specification can be obtained through consolidating the whole wage bill as follows: $W = w\bar{h} + (h^{max} - \bar{h})\tau_f w$. Before 2015, the experience rating of the STW program was almost zero: $\tau_f \approx 0$ so $W = wh$ but after 2015, the introduction of $\tau_f > 0$ for firms on CIG introduces some exogenous variation in the wage bill. The new specification becomes:

$$\log n = \frac{\alpha}{1-\eta} \log h - \frac{1}{1-\eta} \log W - \frac{1-\beta(1-\delta)}{1-\eta} \frac{c}{W \cdot q(\theta)} + \nu \tag{2.F10}$$

The previous log-linearization suggests the following estimation model:

$$\log n_{i,j,t} = \gamma_i + \zeta_j + \mu_t + \alpha_1 \log h_{i,j,t} + \alpha_2 \log W_{i,j,t} + \alpha_3 \underbrace{\frac{1}{W_{i,j,t} q(\theta_{j,t})}}_{X_{i,j,t}} + \nu_{i,j,t}$$

where $i$ indexes firms, and $j$ indexes LLM. Structurally, the coefficients from this regression $\alpha_1$ and $\alpha_2$ and $\alpha_3$ identify the key parameters of the demand function. We estimate the previous specification instrumenting the change in hours by STW treatment and the change in the wage bill by the interaction of STW treatment and being after 2015, when the reform introduced some positive experience rating $\tau_f > 0$. Solving for these parameters gives $\alpha = 0.6, \eta = 0.7$.

**Utility Function**

We use the following isoelastic, additively separable utility function:

$$u(c, h) = \frac{c^{1-\sigma_c} - 1}{1-\sigma_c} - \varphi \frac{h^{1+\sigma_h}}{1+\sigma_h} \tag{2.F11}$$

$\sigma_c$, the coefficient of risk aversion is set to 2.5. The parameter $\sigma_h$ can be interpreted as the inverse of the Frisch labour supply elasticity. We set this parameter to $\sigma_h = 3.5$ in line with conventional calibrations from New Keynesian models (see Galí, 2011).

**Firm Productivity Transition Matrix**

Assume a firm's productivity level can take the value of one of two states, high and low (H and L). Assume that firms transition between these two states freely, where the probability of transitioning from one state to the other is solely dependent on the state which a firm finds themselves in. We therefore obtain the following Markov transition matrix, where $\pi_{ST}$ is the conditional probability of moving to state S, conditional on being in state T:

| | $\epsilon_t$ = H | $\epsilon_t$ = L |
|---|---|---|
| $\epsilon_{t+1}$ = H | $\pi_{HH}$ | $1 - \pi_{HH}$ |
| $\epsilon_{t+1}$ = L | $1 - \pi_{LL}$ | $\pi_{LL}$ |

TABLE 2.F1: MARKOV TRANSITION MATRIX BETWEEN PRODUCTIVITY STATES

From this transition matrix, we obtain the following equations, telling us the total number of firms in each state in the next period. Let $n_t^i$ be the number of firms in state $i$ at time $t$:

$$\pi_{HH} \cdot n_t^h + (1 - \pi_{LL}) \cdot n_t^l = n_{t+1}^h \tag{2.F12}$$

$$\pi_{LL} \cdot n_t^l + (1 - \pi_{HH}) \cdot n_t^h = n_{t+1}^l$$

Assume that we observe the steady state:

$$n_t^h = n_{t+1}^h = n_h \tag{2.F13}$$

$$n_t^l = n_{t+1}^l = n_l$$

And we therefore obtain:

$$\pi_{HH} \cdot n^h + (1 - \pi_{LL}) \cdot n^l = n^h \tag{2.F14}$$

$$\pi_{LL} \cdot n^l + (1 - \pi_{HH}) \cdot n^h = n^l$$

It's clear that these two equations in fact provide no new information, and both reduce to:

$$(1 - \pi_{HH}) \cdot n^h = (1 - \pi_{LL}) \cdot n^l \tag{2.F15}$$

However, we also have an extra condition: as we are in the steady state the proportions $\pi_{HH}$ and $\pi_{LL}$ must add up to 1. We therefore obtain two equations:

$$\pi_{HH} + \pi_{LL} = 1 \tag{2.F16}$$

$$(1 - \pi_{HH}) \cdot n^h = (1 - \pi_{LL}) \cdot n^l$$

which reduces to simply:

$$\pi_{HH} = n^h / (n^l + n^h) \tag{2.F17}$$

We must define now define how to interpret productivity within the data. Take low productivity firms as those who are eligible for CIG and who have at least one CIG event in post 2009. High productivity firms are those eligible but do not take up CIG at any point post 2009.

We observe that 13% of firms are treated post 2009 in the baseline DD sample. We thus obtain $\pi_{HH} = 0.87$. Further, taking the mean (log) total factor productivity of these firms, and normalising the low productivity value to 1 yields: $\epsilon_l = 1, \epsilon_h = 1.62$.

**Wage Schedule and Hours Schedule**

We assume that the wage has the following form:

$$w(\epsilon) = w_s \epsilon^{w_a} \tag{2.F18}$$

with $w_a < 1$. The wage is therefore a somewhat rigid function of productivity. Besides, it does not respond to variation in the STW subsidy, nor to variation in hours, consistent with our empirical evidence. The wage responsiveness to firm productivity, $w_a$, is set to 0.2, in line with similar models in the literature, c.f. Landais, Michaillat, and Saez (2018a).

The hours schedule in the low productivity firm is obtained by assuming that firms have all the bargaining power in low productivity firms, therefore leaving workers at their outside option. For high productivity firms, we consider a simple exogeneous hours schedule:

$$h(\theta, \epsilon) = hs_s \epsilon^{hs_a} \theta^{hs_b} \tag{2.F19}$$

To estimate the parameter $hs_b$, the responsiveness of the hours function to a change in labour market tightness, we regress log hours among ineligible firms at LLM level against log tightness, instrumented by eligibility of CIG. This model obtains a coefficient of 0.14.

**Transfer Generosity**

The unemployment benefit, b, is set to match the net replacement rate for the average worker in Italy in 2008, which is around 70%. For our purposes, this is 70% of the wage obtained if working the full hours endowment.

The STW replacement rate, $\tau$, is the policy parameter, which is determined by the legal implementation of CIG. This rate is defined as 80% of the total remuneration that would have been paid to the worker for the hours of work not provided, bounded between 0 and the fully contracted time.

**Miscellaneous Parameters**

The model imposes an exogenous separation rate, $\delta$. This is set to 0.2, which is the implied probability of being displaced from an specific firm in a specific contract. The model's discount factor, $\beta$, is set to 0.935, implying an annual interest rate of 7%.

### 2.F.2 Endogenous Parameters and Target Moments

After setting the exogenous parameters, we are left with 5 endogenous parameters: We

| Parameter | Description |
|---|---|
| $\mu$ | Matching function scaling |
| $c$ | Vacancy cost |
| $\varphi$ | Utility function labour scaling |
| $hs_a$ | Hours schedule productivity curvature |
| $w_s$ | Wage function scaling |

obtain these parameters through the method of simulated moments, with five target moments:

| Target Moments | Value |
|---|---|
| Unemployment rate | 0.108 |
| High productivity hours level | 34 |
| Low productivity hours level, without STW | 39 |
| Low productivity hours level, with STW | 20 |
| Proportion of labour demand that is high productivity | 0.9 |

The target unemployment rate is the Italian unemployment rate computed from the ISTAT/INAPP data. We target the average unemployment rate in the period 2008-2014: 0.108. Low productivity firms is defined as:

- For eligible firms, those that take up CIG
- For non-eligible firms, in eligible 5-digit industries, firms whose total factor production is in the bottom 12% of the distribution, post 2009

### 2.F.3 Counterfactual Analysis: Permanent vs Transitory Shocks

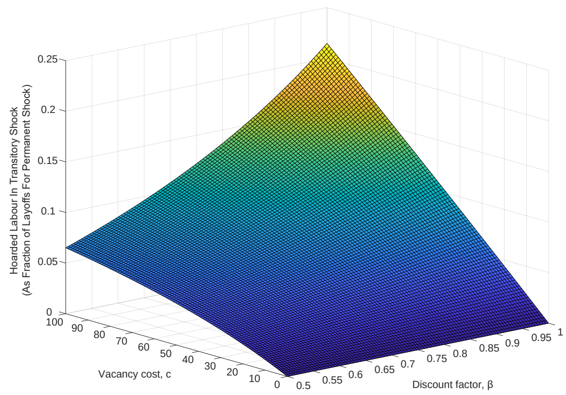
Our baseline calibration considers the Great Recession in Italy as a steady state, and asks what the value is, in such a steady state, of having STW subsidies target firms with negative idiosyncratic shocks. But the nature of shocks, whether they are permanent or transitory, aggregate or idiosyncratic, may matter as well in assessing the effects of STW policies. Firms may be more willing to hoard labor when they expect a shock to be temporary, and therefore relaxing constraints to labor hoarding may be more effective for temporary shocks. Our previous analysis in Figure 2.5 indeed indicates that employment effects of STW are larger for firms that were high productivity prior to the recession, suggesting that STW may be more effective for high productivity firms experiencing a transitory negative shock than for permanently low productivity firms. To get further insights on this, we now use our calibrated model and simulate the effects of STW under two different scenarii of aggregate shocks: a permanent shock and a transitory shock. In both scenarii, we start from the steady state, and firms face in period 0 a surprise 10% negative aggregate productivity shock. In the first case, the shock is permanent, in the second, the shock is only transitory and the aggregate productivity level recovers linearly over 3 periods. Note that in both cases, the initial shock in period 0 is unexpected, but firms then have rational expectations with respect to future states of aggregate productivity (i.e. they know, once realized, whether the shock is permanent or transitory).

The simulated employment response on impact (at time 0) to the permanent shock ($\dot{n}(0)|_{\text{perm.}}$) is, not surprisingly, larger than the employment response to the temporary shock ($\dot{n}(0)|_{\text{temp.}}$).Figure 2.F1 Panel A shows the difference in simulated employment responses $\dot{n}(0)|_{\text{temp.}} - \dot{n}(0)|_{\text{perm.}}$, expressed as a fraction of the employment loss to the permanent shock $-\dot{n}(0)|_{\text{perm.}}$, for various values of the discount factor and of the hiring costs $\mathcal{C}$. The graph confirms that, in the transitory shock scenario, firms do "hoard labor" on impact and keep 10 to 15% of the workers that they would get rid of if they knew the shock was permanent. Importantly, the figure shows that labor hoarding is significantly larger when the cost to firms of replacing their workers increases.

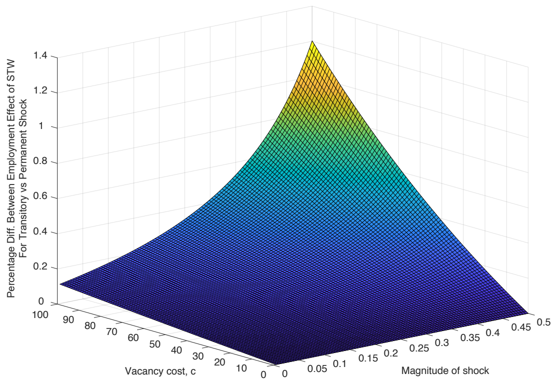
Because hoarding is more valuable when the shock is transitory then permanent, the employment effects of having STW also differ according to the permanence of the aggregate shock. To document this, we simulate $\dot{n}(0)|_{\text{perm.}}$ and $\dot{n}(0)|_{\text{temp.}}$ in a world without STW ($\tau = 0$), and compute the employment effects of STW, $\Delta\dot{n}(0) = \dot{n}(0)|^{\tau=0.8} - \dot{n}(0)|^{\tau=0}$, in both scenarii of the aggregate shock. Panel B of Figure 2.F1 plots the difference in employment effects of STW, $\Delta\dot{n}(0)|_{\text{temp.}} - \Delta\dot{n}(0)|_{\text{perm.}}$, expressed as a fraction of the employment effects of STW in the permanent shock scenario $\Delta\dot{n}(0)|_{\text{perm.}}$. The graph confirms that the employment effects of STW on impact are significantly larger (around 20% to 40%) when the shock is temporary than when it is permanent. This, again, is especially true when the cost of replacing workers is high, and when the magnitude of the aggregate shock is large.

FIGURE 2.F1: LABOR HOARDING AND MAGNITUDE OF STW EFFECTS ON EMPLOYMENT: TRANSITORY VS PERMANENT AGGREGATE SHOCK

A. Simulated Labor Hoarding for Transitory Shock




B. Employment Effects of STW for Transitory vs Permanent Shock




**Notes:** The figure uses the calibrated model to simulate the extent of labor hoarding and the employment effects of STW under two different scenarii of aggregate shocks: a permanent shock and a transitory shock. In both scenarii, we start from the steady state, and firms face in period 0 a surprise 10% negative aggregate productivity shock. In the first case, the shock is permanent, in the second, the shock is only transitory and the aggregate productivity level recovers linearly over 3 periods. Once the shock is realized, firms have rational expectations with respect to future states of aggregate productivity. Panel A shows the difference between the simulated employment response on impact (at time 0) to the permanent shock ($\dot{n}(0)|_{\text{perm.}}$) and the employment response to the temporary shock ($\dot{n}(0)|_{\text{temp.}}$), expressed as a fraction of the employment loss to the permanent shock $-\dot{n}(0)|_{\text{perm.}}$, for various values of the discount factor and of the costs of replacing workers $\mathcal{C}$. The graph confirms that, in the transitory shock scenario, firms do "hoard labor" on impact and keep 10 to 15% of the workers that they would get rid of if they knew the shock was permanent. We then simulate $\dot{n}(0)|_{\text{perm.}}$ and $\dot{n}(0)|_{\text{temp.}}$ in a world without STW ($\tau = 0$), and compute the employment effects of STW, $\Delta\dot{n}(0) = \dot{n}(0)|^{\tau=.8} - \dot{n}(0)|^{\tau=0}$, in both scenarii of the aggregate shock. Panel B plots the difference in employment effects of STW, $\Delta\dot{n}(0)|_{\text{temp.}} - \Delta\dot{n}(0)|_{\text{perm.}}$, expressed as a fraction of the employment effects of STW in the permanent shock scenario $\Delta\dot{n}(0)|_{\text{perm.}}$. The graph confirms that the employment effects of STW on impact are significantly larger (around 20% to 40%) when the shock is temporary than when it is permanent.

## Chapter 3

# Changing the Structure of Minimum Wages: Firm Adjustment and Wage Spillovers

### 3.1 Introduction

The by now centennial history of minimum wages and their widespread application across developed and developing countries has triggered a great deal of academic research on the topic. Recent years have seen a burst of renewed interest in this topic in both academic and policy settings around the world. In this paper, we study what happened to a range of economic outcomes when there was a substantive recent change in the structure of a minimum wage setting policy. This occurred in the UK when a government that had traditionally been hostile to minimum wages introduced an unexpected and sizable increase for older workers by introducing a new minimum wage rate – the National Living Wage (NLW). This new minimum wage rate for workers aged 25 and over moved the number of minimum wages in operation in the UK labor market up from four to five and, in doing so, structurally altered the minimum wage policy in operation in the labor market.

We are interested in analyzing the consequences of this change in the UK minimum wage structure on three big areas of research that have been traditionally explored in the minimum wage literature. Firstly, wage and employment effects are studied in the context of workers and firms in the UK care home sector, which has been argued to be a good testing ground for evaluating minimum wage effects on employment in earlier research (Machin, Manning, and Rahman, 2003; Machin and Wilson, 2004). Secondly, we exploit the change in minimum wage structure to study whether the UK National Living Wage induced wage or employment spillovers onto workers under 25 as the minimum wage setting process was altered. Thirdly, we explore the possibility that care homes responded to the wage cost shock by altering other margins, such as prices, productivity and the quality of care services provided. In addition, we consider whether the policy had implications for aggregate employment and firm dynamics (entry and exit). We do so by leveraging the unique natural experiment offered by the UK policy setting, coupled with rich matched employer-employee data including detailed information on individual hourly wages for the English care home sector. To the best of our knowledge, this is the first paper in which wage and employment effects, wage spillovers and margins of adjustment other than employment are studied in a unified framework.

To preview the key findings, the changed minimum wage structure and associated higher minimum wage for those aged 25 and above significantly impacted on wages, but there is much less evidence of adverse employment effects, and no significant impact on firm closure nor on entry/exit dynamics more generally one year after. Rather the margin of adjustment that was used was the quality of care services. Care homes bound more tightly by the NLW exhibited smaller short run improvements in the quality of care services than less-bound homes.

There is also strong evidence of wage spillovers resulting from the new structure of minimum wages brought about by NLW introduction as younger workers' wages rose in tandem with the higher adult minimum wage, but with there being no spillover impact on their employment. We discuss potential explanations for this pattern of spillovers, including preferences for pay fairness and administrative simplicity. The evidence suggests that employers' – rather than workers' – preferences for fairness play an important role in within-firm wage setting policies in the sector that is studied.

The content of this paper relates to all of the three main streams along which the minimum wage literature has evolved through time. Firstly, the primary focus of this literature has been on the employment and unemployment effects of minimum wages.[1] Secondly and partly in response to the fact that, in a number of settings, employment effects have proven elusive to track down, a smaller but growing body of research has examined other margins of adjustment by firms, such as prices, profits and firm value.[2] Thirdly, another strand of the minimum wage literature has studied the impact on wage inequality at the bottom of the distribution and at spillover effects up the wage distribution.[3] Thanks to a combination of rich data sources and a novel research setting, we

[1]Following an early and mostly US-based time-series work that found negative employment effects among teenagers (Brown, Gilroy, and Kohen, 1982), starting from the early 1990s quasi-experimental micro-based studies found no evidence of disemployment effects in the US and the UK (Card and Krueger, 1994; Machin, Manning, and Rahman, 2003; Stewart, 2004). A recent revival of minimum wage research in the US has adopted spatial identification strategies, also mostly finding it hard to detect evidence of job cuts due to minimum wages (Dube, Lester, and Reich, 2010; Baskaya and Rubinstein, 2015; Dube, Lester, and Reich, 2016; Clemens and Wither, 2019). In a rather different context of union bargained minima, Kreiner, Reck, and Skov (2017) study the effect of a change in the youth minimum wage in Denmark and find an employment elasticity to the wage rate of -0.8.

[2]On prices, see Aaronson (2001), MaCurdy (2015) or Harasztosi and Lindner (forthcoming); on profits, see Draca, Machin, and Van Reenen (2011); and on stock market values, see Bell and Machin (2018). Multiple adjustment channels are studied in Harasztosi and Lindner (forthcoming) and Hirsch, Kaufman, and Zelenska (2015). Sorkin (2015) emphasizes the distinction between modes of adjustment in the short and long run.

[3]See DiNardo, Fortin, and Lemieux (1996), Lee (1999) and Autor, Manning, and Smith (2016).

contribute to this literature by providing a comprehensive assessment of the impact of the NLW introduction on employment and other margins of firm adjustment, as well as novel evidence on downward wage spillovers.

The rest of the paper is structured as follows. Section 3.2 first illustrates the UK NLW introduction and then describes the care home sector studied in the empirical work that follows. Section 3.3 describes the data, together with some descriptive statistics and a discussion of representativeness. Section 3.4 presents the main results of the impact of the changed minimum wage structure on wages, employment and total hours. Section 3.5 illustrates the analysis of wage and employment spillovers, and Section 3.6 discusses possible explanations for the observed pattern of results. Additional margins of adjustments are considered in Section 3.7. Section 3.8 concludes.

## 3.2 Minimum Wages and the Care Home Sector

### 3.2.1 Minimum Wage Setting in the UK and the National Living Wage

In different settings around the world, national minimum wages have seen a burst of renewed interest in recent years as political parties have recognized the popularity of mandated wage floors with the general public.[4] This has probably become more marked in places where real wages have not been rising and where living standards have stagnated, as raising the minimum wage is a genuine policy lever that governments can use to generate wage increases at the bottom end of the wage distribution.

In this paper we consider the economic effects of one such change. The context is the introduction of the National Living Wage in the United Kingdom. Yet, the UK is not the only country in which minimum wages have recently been high on the policy agenda. Indeed, for example, Germany has introduced a national minimum wage at a high level, and some big increases have been observed in parts of the United States.[5]

[4] A 2014 Gallup poll reported that 66 percent of respondents in the UK and 76 percent in the US were in favor of minimum wage increases. According to another recent Gallup poll, in 2016, 56 percent of Americans supported raising the national minimum wage from $7.25 to $15.00 per hour by 2020, 36 percent opposed the idea and 7 percent had no opinion on the matter.

[5] In January 2015, Germany introduced a national minimum wage of €8.50 an hour (approximately £6.40 at that time). Before then, wage rates were based on industry-level collective agreements negotiated by trade unions and business representatives, which however led to an uneven application of wage minima across sectors with more and less established trade unions. As of January 2017, the statutory minimum has reached €8.84 (£7.60). In the Unites States, the Obama administration pushed for a substantial increase in the federal minimum rate from $7.25 an hour to $10.10 an hour, motivated by the desire to boost wage growth at the bottom of the wage distribution and by many US studies of minimum wages (cited above) in which detrimental employment effects have proven elusive. The US federal minimum wage has remained at $7.25 per hour since July 2009, but in 2015 cities such as Seattle and Los Angeles legislated measures to progressively increase the minimum wage to $15.00 per hour in 2017 and 2020 respectively. For recent research studying the big Seattle increase see Jardim et al. (2017) and on California see Reich, Allegretto, and Montialoux (2017).

The UK introduced a National Minimum Wage (NMW) in April 1999. Prior to that, there used to be industry-level wage floors – the Wage Councils – that were in force between 1909 and 1993, but that covered only approximately 12 percent of the workforce at the time of their repeal. In the 1997 elections, the Labour Government committed to introducing a national minimum wage and established the Low Pay Commission (LPC), an independent advisory body set up by the National Minimum Wage Act in 1998. The LPC is composed of nine members, of which three representatives of business organizations, three of employees and three of social partners (these include the Chair and two academics). The LPC's remit is set by the Government and requires that the LPC provide evidence-based advice to the Government on minimum wage rates and uprates.[6] The body submits its recommendations to the Government, which can accept or reject them. If accepted, the recommended uprating subsequently becomes effective.

In April 1999, a minimum hourly wage of £3.60 for workers aged 22 and over, and a lower rate of £3.00 for workers aged between 18 and 21 were established. Additional rates have been introduced for workers aged 16-17 in 2004 and for apprentices in 2010. Additionally, in 2010 the adult wage group was expanded to workers aged 21. As of October 2015, the NMW rates were as follows: an adult minimum rate of £6.70 for workers aged 21 and over, a youth development rate of £5.30 for those aged 18-20, a youth minimum of £3.87 for 16-17 year olds and an apprentice rate of £3.30.[7]

After winning the May 2015 election, the new Conservative Party government called an emergency budget on July $8^{th}$ 2015, in which the Chancellor George Osborne unexpectedly announced the introduction of the National Living Wage (NLW). This changed the structure of minimum wages by introducing a new minimum wage rate of £7.20 an hour for workers aged 25 or above, while leaving the minimum wage rates for younger workers unchanged. Now there are five minimum wages, the NLW for workers aged 25 and over, the NMW for 21-24 year olds, the youth development rate for 18-20 year olds, the young worker rate for 16 and 17 year old and the apprentice minimum wage. Additionally, the NLW was set to achieve a 2020 target of £9.00.[8] The main justification for the NLW introduction was to offset the sizable cuts in tax credits that were simultaneously announced as part of the emergency budget but *de facto* did not take place. Figure 3.B1 in Appendix 3.B shows the evolution of minimum wage rates since the NMW introduction in 1999.

The NLW introduction was an unexpected and radical political intervention for various reasons. Firstly, it arises from a party that traditionally opposed minimum wages,

[6]The LPC assesses research and considers evidence from a wide set of sources, including academic research, site visits around the country, and oral evidence taken from a broad range of stakeholders.

[7]The LPC's recommendations have been almost always accepted by the UK government. The apprentice rate has, however, twice been changed by the Government beyond the LPC recommendations: firstly in 2011, when the rate was increased by £0.05 even though the LPC recommended a freeze; secondly in 2015, when the business secretary uprated the apprentice minimum by an additional £0.50, substantially pushing it up from £2.73 in 2014 to £3.30 in 2015.

[8]The suggested target for 2020 is more precisely 60 percent of median earnings, which – at the time of the announcement – was forecasted to be £9.00 by the UK Office for Budget Responsibility.

especially at the time of the NMW introduction in April 1999. Admittedly, the stagnant profile of real wages in the UK since the beginning of the crisis and the growing popularity of minimum wages amongst the general public made political parties of different views recognize that minimum wages can help raise wages and improve living standards, and generated a bipartisan call for a minimum wage increase. Secondly, the NLW introduction generated a wage change much larger than recent uprates, namely an increase of 10.8 percent at the time of announcement in July 2015 and of 7.5 percent when made effective on April $1^{st}$ 2016. As a result of the change, the number of workers covered by minimum wages (formally those paid at or below the relevant minimum and up to £0.05 above) has grown from 1.6 million to 2.5 million in April 2016, and is expected to reach 3.8 million by 2020. Finally, the intervention significantly modifies the role of the LPC in providing future recommendations, given that it sets a target for 2020 and alters the structure of minimum wage rates by establishing an additional age band.

Most importantly for our analysis, the unexpected and sizable wage shock generated by the NLW introduction provides a unique "experiment" to study the wage and employment consequences of a change in the minimum wage structure.

### 3.2.2 The Residential Care Home Sector

We look at the impact of the NLW introduction on workers and firms operating in the residential care home industry. Residential care refers to the provision of accommodation and personal care to adults in a communal residential center, which may or may not provide nursing facilities. Members of staff in residential care homes are predominantly care assistants, who provide 24-hour supervision, meals and help with personal care needs.

As has been detailed in the earlier research on the sector in the period surrounding the NMW introduction (Machin, Manning, and Rahman, 2003; Machin and Wilson, 2004), the choice of looking at care homes as a good testing ground for studying the economic effects of minimum wage floors is motivated by several reasons. Firstly, the sector is highly vulnerable to changes in minimum wages, since it employs a large number of low-paid workers. Of these, many are aged 25 and over, making the setting especially suited to analyzing the NLW introduction. Secondly, the sector provides an example of what could be closely considered a competitive labor market. It consists of a large number of relatively small firms providing a rather homogeneous service. It is very labor intensive and not unionized. Consequently, a minimum wage change is likely to have a substantial impact on total costs, potentially affecting the economic outcomes of workers and firms that are more affected. Thirdly, the sector is also interesting as residents fees are regulated and paid for by local authorities. Indeed, even though approximately 75 percent of residential care places are owned and managed by private-sector firms, up to 60 percent of places are funded by local authorities at regulated prices (LaingBuisson, 2015). The inability to pass on higher costs in the form of higher prices increases the likelihood of finding large employment effects from wage shocks. Fourth, focusing on the adult social care sector allows us to have good quality data on hourly wages, which are necessary to answer well questions related to minimum wage changes.

Besides its pay and market structure, the residential care home sector is also interesting from a socio-demographic perspective. The aging of the population is generating a growing need of care services for the elderly. Yet, soaring staff costs coupled with tight local authority budgets appear to be putting the care home industry at strain and might have important consequences for access to social care.[9]

Although studying care workers raises concerns about sample selection issue and related questions of generalizability to the UK workforce more widely, we believe our estimates may be relevant for other low-pay sectors, such as hospitality and retail, where minimum wage floors are likely to have the largest effects.

## 3.3 Data and Descriptive Statistics

### 3.3.1 Data Sources

The main data source that is used in the analysis is the National Minimum Dataset for Social Care (NMDS-SC).[10] This is an online system administered by Skills for Care and funded by the Department of Health that collects information on the adult social care workforce in England. Social care providers can use NMDS-SC to store and organize efficiently information about their workers, such as payroll data, training and development, job roles, qualifications and basic demographics. By having an account and updating it regularly, providers can easily view and analyze their data, apply for training and development funds, compare their staffing and compensation profile with that of other providers locally, regionally or nationally, access publications about the social care sector, access e-learning resources for free and directly share their data and returns with governmental authorities such as the Care Quality Commission and the NHS. Access to NMDS-SC is free of charge. However, access to services such as the Workforce Development Fund is conditional on the account being updated yearly.

The dataset is a panel of matched employer-employee data. For each provider, we have information on the industry and main service provided, service capacity and uptake level, number of staff employed, geographic location and system update dates. For workers, we have information on demographics (gender, age, nationality), job role, contracted and additional weekly hours of work, hourly pay rate, date in which the hourly pay is uprated and qualification. We have access to the snapshot of the NMDS-SC online

[9]For the years 2016/17, the Government allowed local authorities who provide social care to adults to increase the council tax by up to 2 percent to fund adult social care only. Known as the "adult social care precept", this increase is in addition to the usual funding of adult social care through council tax. Of the 152 authorities with adult social care responsibilities (unitary authority districts, metropolitan boroughs, London boroughs and county councils), 144 used some or all of the precept. The almost unanimous adoption of the adult social care precepts leaves does not allow us to analyze whether the precept had any role in helping care providers cope with the NLW introduction.

[10]NMDS-SC (2013).

system at monthly frequency from September 2015 to March 2017, each snapshot including all providers in the system at that date and the latest date in which they updated their account.

A second source of information is the Care Quality Commission (CQC) registry. The registry contains a complete record of all active English care providers regulated by CQC at monthly frequency. It provides information on the activity status of businesses and so can be utilized to identify when homes shut down and when new homes enter the sector. Moreover, the registry includes firm-level ratings of the quality of care from the inspection reports conducted by the CQC. The ratings – which will be described in more detail in Section 3.7.3 – are an invaluable source of information to assess the effects of the minimum wage increase on the quality of services provided.

### 3.3.2 Sample Design

Around 22,000 providers are registered with NMDS-SC as of March 2016. Of these, approximately 10,000 are residential care homes with or without nursing. We match the sample of residential care homes with the CQC registry of active locations from September 2015 to March 2017, from which we can obtain information on whether a firm is active or closed in a given month. Our sample comprises care homes that meet the following three requirements: (i) being open in March 2016, (ii) having a record on NMDS-SC for all the months in which the firm is open according to the CQC registry and (iii) having updated their NMDS-SC account at least once after March 2016. In order to avoid sample selection driven by unobservable worker and firm characteristics that may be correlated with the timing and frequency of updating, we do not condition our sample on a specific update date and only require that a firm update its records once in the twelve months after April $1^{st}$ 2016.[11] This selection leaves us with a balanced panel of 4,134 firms that are active in March 2016 and remain open until March 2017.[12]

### 3.3.3 Descriptive Statistics

Table 3.1 reports descriptive statistics for the balanced sample of firms from one month before the NLW introduction that took place in April 2016 to three, six and twelve months after. The relatively low hourly pay and large fraction of workers aged 25 and over in the pre-NLW data confirm the high vulnerability of the care home sector to the NLW introduction, which therefore appears particularly pertinent to study the impact of the NLW as it potentially affected a large proportion of workers.

[11] Approximately 90 percent of NMDS-SC users update within a year.

[12] According to the 2017 report on the care home market of the Competition and Markets Authority (2017), there are approximately 9,500 care homes in England. This implies that our sample represents approximately 43 percent of the market for care homes.

The statistics reported in Table 3.1 also show that the care home sector is characterized by small-to-medium size establishments working close to full capacity (the occupancy rate measured as the ratio of residents to beds is above 90 percent). Mean and median employment are approximately 39 and 32 respectively. The workforce is predominantly female (84 percent), on average older than 40 and working approximately 29 hours per week. The main occupation in this sector is care assistant, which accounts for 56 percent of the workforce. Only 4 percent of the workers hold a nursing qualification. All these characteristics remain fairly constant before and after April 2017, suggesting that the NLW did not induce a compositional change in the productive structure of care homes.

### 3.3.4 Representativeness

It is important to assess the representativeness of our sample as compared to the full population of care homes and their workforce. Estimates from Skills for Care indicate that the NMDS-SC data cover more than 50 percent of the workforce in CQC regulated homes, suggesting that the system might provide a good representation of the sector in England. We also compare the characteristics of our sample with statistics on firms and workers in the care home sector that we derive from the ONS Business Registry and the Labour Force Survey. According to the 2016 ONS Business Registry, firms in the residential care industry for the elderly and disabled have an average firm size that matches the one in our sample (approximately 37 on ONS). Similarly, looking at baseline characteristics for carers in the LFS for the first quarter of 2016, we find that they line up quite satisfactorily with those in our sample of workers, as in the LFS the proportion of female carers is 0.85, average age 42, average hourly wage £7.77 and average weekly hours worked 34. Overall, these statistics are reassuring of our ability to draw any general conclusions from the analysis of the data we undertake.

## 3.4 Wages and Employment Impacts of National Living Wage Introduction

### 3.4.1 Wages Impact

As previously noted, the residential care home sector appears to be potentially vulnerable to the NLW introduction given its wage structure and workforce's age composition. In this section we confirm that the NLW had real "bite" in the care home sector and generated the expected effects on hourly wages and their distribution. This is clearly a necessary condition before analyzing the employment and other economic consequences of minimum wages.

Table 3.2 reports measures of the bite of the NLW. Specifically, these are the proportion of workers paid less than the NLW (or less than the age-specific NMW if younger than 25), the percentage paid exactly at the minimum and the wage gap. The latter is a measure of how much wages would have to increase in a given firm in order to meet the new legal requirements and is computed as follows:

$$GAP_j = \frac{\sum_i h_{ij} \max\{W_{ij}^{min} - W_{ij}\ ,\ 0\}}{\sum_i h_{ij} W_{ij}} \tag{3.1}$$

where $h_{ij}$ is weekly hours worked by worker $i$ in firm $j$, $W_{ij}$ is the hourly wage of worker $i$ in firm $j$ and $W_{ij}^{min}$ is the new age-specific minimum wage (i.e. £3.87 for workers aged 16-17, £5.30 for workers aged 18-20, £6.70 for workers aged 21-24 and £7.20 for older workers). As before, pre- and post-NLW statistics are reported for care homes in the balanced panel.

The residential care sector has clear potential to be heavily affected by the NLW. Around 55 percent of workers aged 25 and over, who would be legally affected by the NLW, were paid below the NLW before it was introduced and only 3 percent were paid exactly at £7.20. Given the small proportion of young workers, similar figures are found for the whole sample of workers (51 and 3 percent respectively). The NLW wage gap averaged 4 percent before the NLW introduction.

Results in Table 3.2 also demonstrate that the NLW strongly affected care home wages. The post-NLW data show a larger drop in underpayment over time (of 16, 18 and 29 percentage points after three, six and twelve months respectively), a halving of the wage gap and a noticeable spike of up to 20 percent at the new minimum. A substantial distributional impact of the NLW on wages can also be seen by looking at Figure 3.1, which plots the hourly wage distribution for care assistants one month before and three, six and twelve months after the NLW introduction. The charts provide compelling evidence of the sizable compression effect the NLW had at the bottom of the hourly wage distribution and the emergence of a sharp spike at the new minimum after its introduction. Among care assistants, the spike reached 20 percent in June 2016, 26 percent in September 2016 and 30 percent by March 2017.[13]

Having established a strong impact of the minimum wage on wages in the care home industry, we now show that homes with the highest potential to be affected were indeed the most affected. To this end, we estimate hourly wage change equations of the following form:

$$\Delta^q \ln W_{j,t} = \alpha_{1,t} + \beta_{1,t} \cdot MIN_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_{1,t} + \varepsilon_{j,t} \tag{3.2}$$

where $\Delta^q \ln W_{j,t}$ is the quarter-on-quarter change in the logarithm of the average hourly wage in firm $j$ between quarter $t$ and quarter $t-1$; $MIN_{j,Mar2016}$ is a measure of the

[13] Figure 3.B2 in Appendix 3.Breplicates Figure 3.1 for a subsample of workers whose wages were updated within given time windows. Specifically, the top left panel includes workers with wage updates between October 2015 and March 2016, the top right panel between April and June 2016, the bottom left panel between April and September 2016, and the bottom right panel between April 2016 and March 2017. The histograms show a spectacular spike at £6.70 in the pre-NLW period, and an even larger, sharper spike of around 40 percent at the new minimum in the post-NLW period.

NLW bite at the care home level, that is either the initial proportion of workers paid below the NLW or the NLW wage gap; $X$ is a vector of pre-NLW firm-level characteristics measured in March 2016, including the proportion of female workers, the average age, the proportion working as care assistants, the proportion with nursing qualification, the occupancy rate and a set of indicators for the nine English regions; $\varepsilon$ is a disturbance term.[14]

The parameter of interest is $\beta_{1,t}$ for $t = 1, \ldots, 4$, which measures the relationship between wage growth and the minimum wage bite in the post-NLW period. The parameter is identified from between-home variation in pre-NLW wage levels and it therefore identifies the causal effect of the minimum wage on wage growth only if – absent the minimum wage change – there was no relationship between the initial level of wages and wage growth. The coefficients $\beta_{1,t}$ for $t = -4, \ldots, 0$ are treatment leads and provide an easy way to test whether there is any correlation between wage growth and the NLW bite prior to the NLW introduction. In other words, the leads allow to test whether there were divergent trends in wage growth between firms more and less exposed to the minimum wage increase before the policy change. This is equivalent to testing for the parallel trends assumption in a traditional difference-in-differences setting.

To document the evolution of the relationship between the NLW bite and wage growth in the post-reform quarters, we measure the outcome variable $\Delta^q \ln W_{j,t}$ as the long difference between March 2016 and, respectively, June 2016, September 2016, December 2016 and March 2017.

The coefficient estimates for model (3.2) are plotted in Panels A and B of Figure 3.2, where $MIN_{j,Mar2016}$ corresponds, respectively, to the proportion of low-paid workers and the wage gap. Both graphs report the estimated coefficients for the balanced panel of firms that are active throughout all months between September 2015 and March 2017 (hollow circles), and for the panel of firms in our main sample (black circles). The vertical capped bars represent 95 percent confidence intervals based on robust standard errors. Firstly, the results provide compelling evidence of the causal effect of the minimum wage change on wage growth. Secondly, the graphs reveal that our measures of the NLW bite are sufficiently exogenous since they display little if any correlation with wage growth prior to the NLW introduction.[15]

Table 3.3 reports estimates of the wage equations in model (3.2) for the balanced panel of firms in our main sample. Panel A uses to $\Delta^q \ln W_{j,t}$ between March 2016 and June 2016, Panel B between March 2016 and September 2016, and Panel C between March 2016 and March 2017. For each of the three panels, the specifications in columns (1) and (2) report the estimated coefficient $\beta_1$ for a model in which $MIN$ is the pre-NLW proportion of

[14]Data on the gender and age composition, and on the occupancy rate is missing for some firms. Such missing information is controlled for via a set of dummy variables.

[15]There is little if any association between the low-pay proportion and wage growth, and only some small association between the wage gap and wage growth in the pre-NLW period. However, both appear negligible compared to the marked shift in the post-NLW period.

workers paid below the NLW (or their age-specific NMW if less than 25 years old), while columns (3) and (4) for a model using the wage gap as main regressor. The regression models in columns (2) and (4) include the above-listed firm-level controls.

In all cases there is significant evidence of larger increases in wages in homes with more low-wage workers in the pre-NLW period, as measured by the low-wage proportion or the wage gap. According to the regression estimates in Panel C, a one standard deviation increase in the proportion of low-paid workers (corresponding to a 33 percentage point change) implies a 1.6 percentage-point faster wage growth on a baseline of 4 percent. A similar effect of 1.6 percentage point faster wage growth is found as a result of a one standard deviation increase in the wage gap (corresponding to a 4 percentage point change). Both effects are sizable and establish a strong and significant relationship between minimum wages and wage growth. We find comparable results when looking at weekly earnings growth as shown in Table 3.C1 in Appendix 3.C.[16]

### 3.4.2 Employment Impact

Having established that the NLW had important wage and wage structure effects, we next consider a "second stage" of whether or not the wage cost shock induced by the NLW had consequences on employment and total hours. We start by estimating reduced-form employment and total hours change equations similar to the wage equations illustrated in the previous subsection. Specifically, we regress the change in the logarithm of the number of employees and of total weekly hours ($\Delta^q \ln Y_{j,t}$) on measures of the NLW bite, as follows:

$$\Delta^q \ln Y_{j,t} = \alpha_{2,t} + \beta_{2,t} \cdot MIN_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_{2,t} + \nu_{j,t} \tag{3.3}$$

where $MIN$ and $X$ are defined as before, and $\nu$ is a disturbance term. Similar to the wage equations, in the post-treatment period we measure the outcome variable $\Delta^q \ln Y_{j,t}$ as the long difference between March 2016 and, respectively, June 2016, September 2016, December 2016 and March 2017 in the post-NLW quarters.

Similarly to the wage equation, the identifying assumption for $\beta_2$ is that – absent the minimum wage increase – there would be no relationship between initial wages and employment (or total hour) growth. Panels A and B of Figure 3.3 show the estimated $\beta_{2,t}$ for $t = -4, \ldots, 4$ from model (3.3) using employment growth as an outcome, and using the proportion of low-paid workers and the wage gap as main regressors respectively. Panels A and B of Figure 3.4 report similar results for total hours growth. Each graph reports the estimated coefficients for the balanced panel of firms that are active throughout all months between September 2015 and March 2017, and for the panel of firms in

[16] The coefficients reported in Columns (1) to (4) in the three panels of Table 3.C1 in Appendix 3.C closely match those obtained for hourly wages, suggesting that the wage elasticity of weekly earnings is approximately one. This is indeed what we find in columns (5) and (6) where we estimate the structural form equation described in Section 3.4.2.

our main sample. The correlation between $MIN$ and employment (total hours) growth is very small and statistically insignificant (or at the margins of statistical significance) in the quarters preceding the NLW introduction. The only exception is a statistically significant, albeit small, negative correlation in the quarter before the policy change.[17] All in all, we take these results as evidence that model (3.3)'s identifying assumption appears to be supported by the data.

Columns (1) to (4) of Tables 3.4 and 3.5 report the regression estimates of the key parameter of interest $\beta_2$ for employment and total hours respectively. The estimates reported in column (2) of Tables 3.4 and 3.5 indicate that a one standard deviation increase in the proportion of low-paid workers reduces employment growth by 0.6 percentage points from a baseline of 1.4 percent, and reduces total hours growth by 0.3 percentage points from a baseline of 2.1 percent. As for the wage gap, columns (4) of Tables 3.4 and 3.5 show that a one standard deviation increase in the wage gap reduces employment and total hours growth by 0.4 and 0.7 percentage points respectively. However, none of the estimates is significantly different from zero despite being rather precisely estimated.[18]

We further investigate the employment and hours consequences of the NLW introduction by estimating a structural model of labor demand of the following form:

$$\Delta^q \ln Y_{j,t} = \alpha_{3,t} + \beta_{3,t} \cdot \Delta^q \ln W_{j,t} + X'_{j,Mar2016} \cdot \gamma_{3,t} + \eta_{j,t} \tag{3.4}$$

where all variables are as previously defined. The parameter $\beta_3$ measures the wage elasticity of labor demand and is estimated by instrumenting the change in the logarithm of the average wage $\Delta^q \ln W_{j,t}$ using $MIN_{j,Mar2016}$ as instrumental variable. The wage equations illustrated in the previous section can be therefore considered as the first stage of this instrumental variable model and show that the instrument is relevant. To be valid, an instrument should also satisfy the exclusion restriction and be as good as randomly assigned, i.e. our measures of the NLW bite should only affect the outcome through their impact on wage growth and be uncorrelated with any other proximate determinant of employment or total hours growth. Although neither of these two assumptions can be formally tested, the evidence in Figures 3.2, 3.3 and 3.4 seems to support the validity of our instruments.

Estimates of the structural elasticities are reported in columns (5) and (6) of Tables 3.4 and 3.5, using the initial proportion of low paid and the wage gap as instruments

[17] A possible interpretation of the negative correlation in March 2016 is that care homes take actions to decrease employment in anticipation of the NLW introduction. However, if we measure the variable $MIN$ in December 2015 (or September 2015), the March-2016 dip disappears – in spite of a 95 (91) percent correlation in the $MIN$ variable across quarters. This result suggests that the March 2016 dip is likely due to idiosyncratic negative shocks affecting high-$MIN$ homes rather than anticipation effects.

[18] Results reported in Tables 3.4 and 3.5 refer to the period between March 2016 and March 2017. Tables 3.C2 and 3.C3 in Appendix 3.C report the coefficient estimates for the periods between March and June 2016, and March and September 2016.

for the wage change respectively.[19] The estimated wage elasticity of employment ranges between -0.23 and -0.41 (Table 3.4), while that of hours is between -0.21 and -0.44 (Table 3.5). Evaluated at an average wage growth of approximately 4 percent, these elasticities indicate that headcount employment would drop by 0.9-1.6 percent and total hours by 0.8-1.8 percent. The estimated employment and total hours elasticities are modest, but relatively large compared to many of the estimates in the recent minimum wage literature. However, none of the structural elasticities nor the reduced-form estimates is significantly different from zero, leading to the conclusion that there is no clear evidence of detrimental employment, nor hours effects, of the NLW introduction.[20,21]

## 3.5 Wage and Employment Spillovers

### 3.5.1 Wage Spillovers Down the Wage Distribution

The NLW increased the minimum wage for workers aged 25 and over to £7.20 per hour, but left the minimum wage rate for workers aged 21-24 at the October 2015 level of £6.70 per hour. It is an interesting question, then, whether care homes left wages for workers under 25 unchanged at the old NMW, or whether they decided to also raise them, perhaps for reasons of administrative simplicity or inequality aversion within the firm.

In this subsection, we provide compelling graphical evidence that it is indeed the case that the NLW generated positive spillover effects on the wages of younger cohorts. Figure 3.5 shows the evolution of the hourly wage distribution for care assistants aged under 25 from one month before to twelve months after the NLW introduction.[22] Strikingly, we observe a spectacular spike located at the new adult minimum after April 2016, and a strong wage compression in the bottom half of the distribution. Both the location and size of the spike, and the amount of bottom wage compression are analogous to what we found for the entire sample of care assistants over all age groups. According to Figure 3.5, while 20 percent of care assistants aged under 25 were paid at the NMW in March 2016; up to 28 percent of younger workers are at the new NLW after its introduction.

[19] In Tables 3.4 and 3.5, both the dependent variable and the main regressor are computed as the change between March 2016 and March 2017. Tables 3.C2 and 3.C3 in Appendix 3.C report estimates for the period between March and June 2016 in Panel A, and between March 2016 and September 2016 in Panel B.

[20] In order to check that our results are not driven by the lack of updating by firms, we estimate the wage, employment and total hours equations on the subsample of firms that updated the wages of at least 50 percent of their workers in the period between October 2015 and March 2016, and in the period after March 2016. All results hold in this subsample and are available upon request.

[21] The absence of employment effects could in fact mask changes in the composition of the workforce, for a given level of employment. We find no evidence of differential levels of inflows, outflows and total flows as a consequence of the NLW introduction, which leads us to exclude the presence of such compositional changes.

[22] Figure 3.B3 in Appendix 3.B reproduces the same graphs on the subsample of care assistants whose hourly wages were updated between October 2015 and March 2016 (top left panel), between April 2016 and June 2016 (top right panel), between April 2016 and September 2016 (bottom left panel), and between April 2016 and March 2017 (bottom right panel).

We complement the graphical analysis illustrated above by performing some regression analysis of spillover effects on wages. Firstly, we run a simple reduced-form model of the growth rate of hourly wages for workers under 25 as a function of measures of the NLW bite for older workers. The reduced-form model reads as follows:

$$\Delta^q \ln W^Y_{j,t} = \alpha_{4,t} + \beta_{4,t} \cdot MIN^O_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_{4,t} + \theta_{j,t} \tag{3.5}$$

where $\Delta^q \ln W^Y_{j,t}$ is the change in the natural logarithm of the average wage of workers under 25 in firm $j$ between March 2016 and three, six or twelve months after; $MIN^O_{j,Mar2016}$ indicates alternatively the initial proportion of workers aged 25 and over that are paid below the NLW, or the NLW wage gap for older workers; $X$ is the vector of pre-NLW firm-level characteristics that we described in our previous analyses. The reduced-form estimates of $\beta_{4,t}$ for $t = 4$ are reported in columns (1) to (4) of Table 3.6.[23]

We also perform a structural estimation of the cross wage elasticity between wages of younger workers and adult workers. In the structural estimation, we regress the change in log average wages for younger workers $\Delta^q \ln W^Y_{j,t}$ on the change in log average wages for older workers $\Delta^q \ln W^O_{j,t}$, and we instrument the latter using $MIN^O_{j,Mar2016}$ as instrumental variable. The structural model reads as follows:

$$\Delta^q \ln W^Y_{j,t} = \alpha_{5,t} + \beta_{5,t} \cdot \Delta^q \ln W^O_{j,t} + X'_{j,Mar2016} \cdot \gamma_{5,t} + \iota_{j,t} \tag{3.6}$$

Estimates of the structural cross elasticity parameter $\beta_5$ are reported in columns (5) and (6) of Table 3.6, where we respectively use the proportion of low paid workers among those aged 25 and over, and the wage gap for older workers as instruments. The first stage regression coefficients are reported in Table 3.C4 in Appendix 3.C.

All the estimates in Table 3.6 indicate significantly positive spillovers on the hourly wages of younger workers and cross elasticities of approximately 0.7. According to columns (2) and (4) of Panel C, a one standard deviation increase in the proportion of older workers paid below the NLW or in the adult wage gap (corresponding respectively to 34 and 4 percentage points in the estimation sample) are associated with a 1.3 and 1.2 percentage point faster wage growth for younger workers, on a baseline youth wage growth of 4.1 percent. Cross-elasticity estimates indicate that a one percent increase in average adult wages induces a 0.7 percent increase in average youth wages.[24, 25]

[23] Panels A and B of Figure 3.B4 in Appendix 3.B provide compelling evidence that wage spillovers are driven by the NLW introduction, as no systematic correlation between measures of the NLW bite among older workers and wage growth among younger workers can be detected prior to the NLW introduction.

[24] We also investigated whether the size of wage spillovers changes with the bite of the NLW on older workers. There was no evidence of statistically significant differential effects between firms with a proportion of low-paid older workers above and below the mean in the sample (and similarly for firms with an NLW gap for older workers above and below the mean in the sample).

[25] We also consider spillover effects on weekly earnings. As reported in Table 3.C5 in Appendix 3.C, the coefficients are not as precisely estimated as those of the wage spillover equations, except for those in Panel C that are highly statistically significant. Nonetheless, in none of the estimates in columns (5) and (6) we can reject a coefficient magnitude comparable to the corresponding effect in Table 3.6.

### 3.5.2 Employment and Total Hours Spillovers

Having documented significant and positive spillovers on wages that resulted from the changed minimum wage structure, we also test for the presence of spillover effects on employment and total hours for workers under 25. Indeed, firms might be induced to raise wages of younger workers for reasons of fairness or administrative simplicity, but at the same time may reduce youth employment along the intensive or extensive margin if youth productivity is lower than the uprated wage.

We adopt a methodology similar to the one used to investigate wage spillovers, regressing the change in the share of total employment aged under 25 and the change in the share of total hours worked by workers under 25 ($\Delta^q \ln Y^Y_{j,t}$) on (i) measures of the NLW bite amongst workers aged 25 and over ($MIN^O_{j,Mar2016}$), and (ii) $\Delta^q \ln W^O_{j,t}$ instrumented using $MIN^O_{j,Mar2016}$. Reduced-form estimates of employment and total hours spillovers are reported in columns (1) to (4) of Tables 3.7 and 3.8, while structural cross wage elasticities of demand are reported in columns (5) and (6).[26] Overall we find no statistically significant evidence of negative spillovers at the extensive and the intensive margins of employment, suggesting that the residential care home sector has so far coped with the NLW introduction since it managed to raise wages of legally unaffected workers without reducing employment.[27]

## 3.6 Reasons for Wage Spillovers

### 3.6.1 Wage Spillovers in the Domiciliary Care Sector

In this and the next subsection, we investigate potential explanations of why the wage spillovers that we uncovered in the previous analysis may have come about. A first obvious candidate for explaining why we observe positive spillovers on younger workers is that either workers or firms are concerned with the fairness of the within-home wage structure and prefer that workers doing the same job receive the same wage, even though some of them may be more productive. There is considerable evidence for such preferences for fairness in the minimum wage literature. Survey data on fast food restaurants in Texas and administrative data on the retail sector in Finland indicate that employers have been reluctant to apply youth sub-minima (Katz and Krueger, 1992; Böckerman and Uusitalo, 2009), and laboratory experiments have shown that minimum wage increases

[26] Tables 3.7 and 3.8 report estimates for the period between March 2016 and March 2017. Estimates for the periods between March and June 2016, and March and September 2016 can be found in Tables 3.C6 and 3.C7 in Appendix 3.C.

[27] The lack of spillovers on employment could in fact mask a change in the composition of the younger workforce, for a given proportion of employees aged under 25. An analysis of inflows, outflows and total flows of younger workers indicated that – if anything – firms that had the larger wage spillovers experienced lower levels of churning amongst the younger segments of their workforce, thus excluding significant compositional changes in response to the wage cost shock.

generate entitlement effects and change workers' perceptions of what a fair wage is (Falk, Zehnder, and Fehr, 2006).

It seems plausible that if workers' preferences for "equal pay for equal job" were entirely responsible for the emergence of wage spillover effects, the latter should be stronger for employees working in team or with direct sight of their colleagues while working. In order to test whether spillover effects are driven by workers' as opposed to employers' equity concerns, we replicate our analysis of spillover effects in the domiciliary care sector for which we have data on NMDS-SC.

Domiciliary care is a social care service provided to people who live in their own houses and require assistance with personal care routines, household tasks such as cleaning and cooking, or any other activities they may need to live independently. Domiciliary care assistants typically work individually, drive their own car to visit customers' homes, and are often contracted on flexible working hours or zero hours contracts since domiciliary care work tends to be organized into short and fragmented home visits. Given the nature and organization of work, workers employed by domiciliary care agencies tend to have limited face-to-face interactions with co-workers on the job and are unlikely to be fully aware of their working conditions. If downward wage spillovers were entirely due to workers' fairness preferences, we would expect them to be milder in the domiciliary care sector than the care homes one, *ceteris paribus*.

The summary statistics reported in Table 3.9 illustrate the main differences between firms and workers in the care home and domiciliary care sectors.[28] While the gender and age composition is essentially identical across the two sectors, and wage differentials are relatively limited, working arrangements diverge strikingly. The incidence of zero hours contracts is nine times as large in the domiciliary care sector when considering workers of all ages and five times as large for workers aged under 25. Similarly the proportion of workers on alternative work arrangements, i.e. employed with temporary, bank or agency contracts, is almost twice as large in the domiciliary care sector (14 against 8 percent). These substantial differences corroborate the notion that domiciliary care work schedules are inherently fragmented as the nature of the job would suggest.

We replicate the analysis of wage spillover effects on the sample of domiciliary carers. Figure 3.6 shows the evolution of the hourly wage distribution for domiciliary carers aged under 25 from one month before to twelve months after the NLW introduction.[29] The similarity with the patters observed for care assistants in the care home sector is striking. A large spike at the new minimum and a strong wage compression in the bottom half of the wage distribution clearly emerge after April 2016. The size of the spike is in line with the one found for care assistants aged under 25, with approximately 24 percent

[28] The sample of care homes is the one used in the previous analysis, while the sample of domiciliary care agencies is selected following the same criteria used to select the sample of care homes.

[29] Figure 3.B5 in Appendix 3.B reproduces the same graph on the subsample of domiciliary carers whose hourly wages were updated between October 2015 and March 2016 (top left panel), between April 2016 and June 2016 (top right panel), between April 2016 and September 2016 (bottom left panel), and between April 2016 and March 2017 (bottom right panel).

of young domiciliary carers being paid exactly £7.20 as of March 2017. We also estimate empirical models (3.5) and (3.6) on the domiciliary care sample. Results are reported in Table 3.10. None of the structural cross elasticities reported in columns (5) and (6) of Panels A, B and C is statistically different from one, indicating that wages for younger workers increased one for one with wages of adult workers.[30, 31]

Therefore, in spite of the remarkably different working arrangements documented above, domiciliary care workers experience wage spillovers very similar in magnitude to those we identified in the care home industry. We interpret this evidence as supportive of the fact that team dynamics and worker-specific preferences for fairness are not key drivers of downward minimum wage spillovers.[32]

### 3.6.2 Evidence on the "Fairness" Hypothesis

The evidence presented in the previous subsection seems to exclude a strong role for workers' preferences alone in within-firm wage setting. Two additional theories could explain the emergence of downward wage spillovers. The first is fairness concerns and inequality aversion by employers. The second is administrative simplicity, whereby employers try to minimize the costs of managing a diverse wage structure and of individual-level bargaining. While we cannot formally test which of these two alternative theories has the largest bearing, in this section we discuss evidence we gathered from a survey of care homes that seems to support the "fairness hypothesis".

For an earlier project funded by the Low Pay Commission, we ran a survey of English care homes. We obtained information on all care homes in England from the CQC directory and sent questionnaires to all homes in January and February 2016 for the pre-NLW part of the survey, and in late June, August and November 2016 for the post-NLW part of the survey. We obtained a total of 1390 responses in the pre-NLW survey and of 827 responses in the post-NLW survey that were provided by the owner manager of the care homes.[33] In both the pre- and post-NLW surveys, we asked respondents about their views on the level of the NLW. Table 3.C12 in Appendix 3.C reports answers to this question by firms in the balanced panel, splitting the sample between firms with a pre-NLW low-paid proportion above and below the median. Before the NLW introduction, 41.2 percent of firms with above-median low-paid proportions believed that the level of the

[30]Table 3.C8 in Appendix 3.C reports the coefficient estimates of the wage equations in the sample of domiciliary care agencies. Results in columns (1) and (2) of Panels A, B and C are very much in line with those reported in Table 3.3 for the sample of care homes. Results in columns (3) and (4) are instead smaller in magnitude and less precisely estimated. Given the high incidence of zero hours contracts in the domiciliary care sector, the NLW gap appears less appropriate as a measure of the NLW bite in this context as opposed to the care home one.

[31]The first-stage coefficients for the wage spillover equations in the domiciliary care sector are reported in Table 3.C9 in Appendix 3.C.

[32]For the sake of completeness, we also investigate employment and total hours spillovers in the domiciliary care sector in Tables 3.C10 and 3.C11 in Appendix 3.C. None of the estimated coefficients is statistically significantly different from zero.

[33]More information on the survey of care homes is available upon request.

NLW was about right, 16.0 percent too low and 42.8 percent too high. Interestingly, after the implementation of the new wage floor, those same respondents appear to be much more favorable to the minimum wage floor, with 55.1 percent considering it about right, 26.3 percent too low and only 18.6 percent too high. Such shift in preferences turns out to be more pronounced for firms with low-paid proportions above than below the median, lending further support to the employer's fairness hypothesis.[34] In the post-NLW survey we asked respondents to leave a verbal comment about what they believed would be the impact of the NLW on their business. While it is not uncommon for respondents to state that it is fair for a worker to earn a "living wage", none of the replies refers to administrative simplicity and bargaining costs.

We perform a back-of-the-envelope calculation and estimate what the average counterfactual savings from paying all care assistants their age-specific minima would be. It turns out that, if all care assistants were paid their minimum wage, the total wage bill would decrease by 2.6-2.9 percent.[35] The same figure would drop to 1.2-1.3 percent if only care assistant under 25 were paid their age-specific minima. For a labor share of total costs of approximately 60 percent and assuming no scope for efficiency wages, we conclude that after the NLW introduction employers have been willing to take a profit hit of up to 1.7 percent – above and beyond the 2.4 percent needed to meet the NLW requirements – when raising wages above the legal minimum.[36]

## 3.7 Other Margins of Adjustment

Given the lack of evidence of employment effects in spite of significant wage increases for both legally affected and unaffected workers, this section explores whether the minimum wage increase had an impact on outcomes other than employment and total hours. It is possible that firms respond to the wage cost shock by adjusting other margins, such as prices, profits, productivity and the quality of care services. We consider these outcomes in the following subsections.

### 3.7.1 Price Setting and Resident Intake

In theory, the lack of evidence of employment responses could be explained by the ability to pass minimum wage increases onto consumers in the form of higher prices. In practice, though, this is unlikely to happen since residential care fees are, in the majority of cases, regulated by local authorities. Even though private for-profit companies dominate the

[34]See Table 3.C12 in Appendix 3.C for results on the group of firms with low-paid proportions below the median.

[35]The age specific minima are the NLW of £7.20 for those aged 25 and over, the NMW for those under 25, a youth development rate of £5.30 for those aged 18-20, a youth minimum of £3.87 for 16-17 year olds and an apprentice rate of £3.30.

[36]We obtain an estimate of the labor share of total costs from our post-NLW survey, where we ask the question "Approximately what percentage of your total costs are labor costs?".

care home industry, a large fraction of their residents are funded by local authorities.[37] According to LaingBuisson, 60 percent of residential care home places were funded by local authorities in 2014, making local authorities the largest purchaser of adult social care services. Limited by tight budgets, local authorities have kept fee levels low, leading to an average 5 percent reduction in real fee rates over the period 2010 to 2016 (LaingBuisson (2015)). Analyses based on our survey of care homes – where we collected data on minimum and maximum weekly prices – do not provide significant evidence of larger price increases in firms where the NLW introduction bit harder, as the presence of price regulations would suggest.[38]

Firms' limited ability to change prices may lead them to alter the care mix that they provide by decreasing the proportion of residents paid for by the local authority or by increasing the share of relatively more expensive services, for a given level of prices. While we do not have information on the mix of residents in the NMDS-SC data, we collected information on the proportion of residents funded by the local authority and the proportion requiring specialist care in our survey of care homes. Estimates based on the survey data do not point to significant changes in the proportion of local authority funded residents, but are suggestive, albeit at the margins of statistical significance, of an increase in the proportion of residents requiring specialist care.[39]

### 3.7.2 Productivity

A margin that firms may try to improve in response to the increase in costs is productivity. In order to explore this hypothesis, we construct a measure of productivity as the logarithm of residents per worker hour. We regress the change in productivity against measures of the NLW bite and the change in the logarithm of average wages appropriately instrumented. According to the estimates reported in Table 3.11, there is no evidence of larger productivity improvements by those firms that were more heavily affected by the NLW introduction.[40]

[37] According to Jarret (2018), in 2014 private sector residential care places reached 74 percent of all places, followed by voluntary sector (18 percent) and local authority places (8 percent). The role of the private sector was even more prominent in care homes with nursing, where it had 86 percent of all places, while the voluntary sector 8 percent and the public sector the remaining 6 percent. The data refer to the UK. In our sample, 82 percent of homes are private sector for-profit companies, 14 percent are voluntary and 0.6 percent local authority (the remaining 3.4 percent being classified as "Other").

[38] Results available upon request.

[39] Results available upon request.

[40] Results in Table 3.11 refer to the period between March 2016 and March 2017. Analogous results for the periods between March and June, and March and September 2016 are reported in Table 3.C14 in Appendix 3.C.

### 3.7.3 Quality of Care Services

Another possibility is that firms respond to the cost shock by reducing the quality of care services provided. We have information on the quality of care from the inspection reports conducted by the CQC. The CQC is the independent regulator of health and adult social care in England. It is responsible for setting standards of care and for monitoring, inspecting and rating adult social care providers, to make sure that they meet fundamental standards of quality and safety. At the heart of CQC's regulatory activity, the rating process is based on periodic inspections of care providers followed by the publication of reports showing the evaluation of the quality of care. The ratings are articulated into five key lines of enquiry and an overall judgement. The five lines of enquiry ask if the service is safe, effective, caring, responsive to people's needs and well-led, while the overall judgement is an aggregation of these five dimensions.[41] The rating can be "outstanding", "good", "requires improvement" or "inadequate".[42]

We have access to the most recent firm-level CQC ratings as of March 2016 and March 2017, and can link them to observations in the NMDS-SC database. Of the 2480 homes that we could match, 931 had been inspected and rated before and after the NLW introduction.[43] Figure 3.B6 in Appendix 3.B displays the distribution of ratings by key line of enquiry as of March 2016 for the full sample (Panel A) and for the subsample of firms with ratings both before and after March 2016 (Panel B). In a similar fashion, Panels A and B of Figure 3.B7 in Appendix 3.B show the distribution of the change in ratings between March 2016 and March 2017 for the two samples. Ratings tend to be concentrated in the mid-range categories, with approximately 65 percent of homes providing a good overall service and 35 percent requiring improvement as of March 2016 (Panel A of Figure 3.B6). The subgroup of firms that were inspected both before and after March 2016 tend to have poorer performances across all lines of enquiry (Panel B of Figure 3.B6), suggesting that performance and the frequency of inspections might be negatively correlated. Ratings vary upward or downward between March 2016 and March 2017 for approximately 50 percent of the sample inspected in both periods (Panel B of Figure 3.B7).

[41] The key lines of enquiry are specified as follows. *Safe*: residents are protected from abuse and avoidable harm. *Effective*: care, treatment and support achieves good outcomes, helps residents maintain quality of life and is based on the best available evidence. *Caring*: staff involve and treat residents with compassion, kindness, dignity and respect. *Responsive*: services are organized so that they meet the resident's needs. *Well-led*: the leadership, management and governance of the organization make sure it is providing high-quality care that is based around the resident's individual needs, it encourages learning and innovation, and it promotes an open and fair culture. Further details can be found at `http://www.cqc.org.uk/what-we-do/how-we-do-our-job/five-key-questions-we-ask`.

[42] *Outstanding*: the service is performing exceptionally well. *Good*: the service is performing well and meeting CQC's expectations. *Requires improvement*: the service is not performing as well as it should and has been told that it must improve. *Inadequate*: the service is performing badly and CQC has taken action against the person or organization that runs it. Further details can be found at `www.cqc.org.uk/what-we-do/how-we-do-our-job/ratings`.

[43] Estimates of the wage equations, employment and hours equations and wage spillover equations all hold in this subsample and are available upon request.

We investigate whether the NLW introduction caused a change in the quality of care services by running regression models similar to equations (4) and (5), where – for each line of enquiry – we regress the change in rating between March 2016 and March 2017 against measures of the NLW bite ($MIN_{j,Mar2016}$) and against the change in the logarithm of the average wage ($\Delta^q \ln W_{j,t}$) instrumented with $MIN_{j,Mar2016}$. As pointed out before, care homes with lower initial ratings are more likely to be inspected in the post-NLW period, i.e. are more likely to experience a change in ratings. If initial ratings are correlated with the initial level of wages and, in turn, with the bite of the NLW, our estimates of the causal effect of the NLW on the quality of care would be biased. To account for the potential confounding effect of initial ratings, we include them among the controls.

Results are reported in Table 3.12, where Panel A refers to the overall rating and subsequent panels refer each to one of the five key lines of enquiry. Both reduced-form and structural-form coefficients are negatively and statistically significantly different from zero across all specifications and quality dimensions, indicating that the quality of care is a margin of response to increased wage costs. According to the structural estimates in columns (5) and (6) of Panel A, a 4 percent increase in average hourly wages leads to a drop of approximately 0.1 in the overall rating on a baseline change of 0.11. In other words, care homes bound more tightly by the NLW exhibited smaller short run improvements in the quality of care services than less-bound homes did.

### 3.7.4 Firm Closure

The analysis of employment and total hours effects is based on the balanced sample of firms that remain active throughout the period of our analysis. We are also interested in assessing whether the wage shock induced by the NLW introduction impacted the probability of survival of firms in the residential care home sector. To this end, we consider the panel of firms that were active in March 2016 (but may close in subsequent months) and that we could match with the CQC registry to obtain information on the activity status of each care home at monthly frequency. The resulting panel is composed of 4,306 care homes, of which 0.1 percent closed by June 2016, 0.6 percent by September 2016 and 1 percent by March 2017.

In order to empirically assess whether the NLW had a role in the pattern of closures, we run reduced-form linear probability models of the probability of being closed three, six or twelve months after the NLW introduction on our measures of the wage bite $MIN_{j,Mar2016}$. Regression estimates are reported in Table 3.13, for closures as of March 2017, and in Table 3.C14 in Appendix 3.C for closures as of June 2016 and September 2016. All coefficient estimates are statistically insignificant and their magnitudes modest, suggesting that care homes where the minimum wage change hit the most were not more likely to go out of business, at least in the short run.

Not having access to information on profits or balance sheet data, we are unable to assess whether the wage shock induced by the NLW introduction caused a significant reduction of firm profits. Even though we cannot exclude the existence of a profit hit, the above results make it clear that any profit hit that could have occurred has so far not been large enough to drive firms out of business.

### 3.7.5 Aggregate Employment and Firm Dynamics

Finally, we consider whether the NLW introduction impacted aggregate employment and firm dynamics (entry and exit). To this end, instead of restricting the sample to firms that were active throughout the period of analysis, we consider all firms ever active in the months surrounding the NLW introduction. Our findings suggest that aggregate employment did not suffer as a consequence of the NLW introduction, since jobs that were paid below the NLW before April 2016 are fully replaced by jobs paid at or above the NLW after its introduction. Likewise, firm dynamics – entries and exits – were not significantly affected by the NLW in the twelve months after it came into force. The analysis of aggregate employment effects and firm dynamics is discussed in detail in Appendix 3.A.

## 3.8 Conclusion

This paper contributes to the recent revival of research and policy interest in minimum wages by studying the impact of a significant change in the structure of minimum wages that occurred in the UK in 2016. Leveraging unique exogenous variation brought about by the NLW introduction and novel matched employer-employee data with good-quality information on individual wages, we provide a comprehensive analysis of the effects of minimum wages on employment, the wage distribution and firm adjustment levers, thus contributing to the three key research areas in the minimum wage literature in a unified framework.

The altered structure was brought about by the government introducing a new minimum wage – the National Living Wage – for older workers. This resulted in there being a fifth minimum wage rate in operation, as compared to the four that operated prior to the change, with quite sizable differences in the minima paid to different age workers who previously were paid the same.

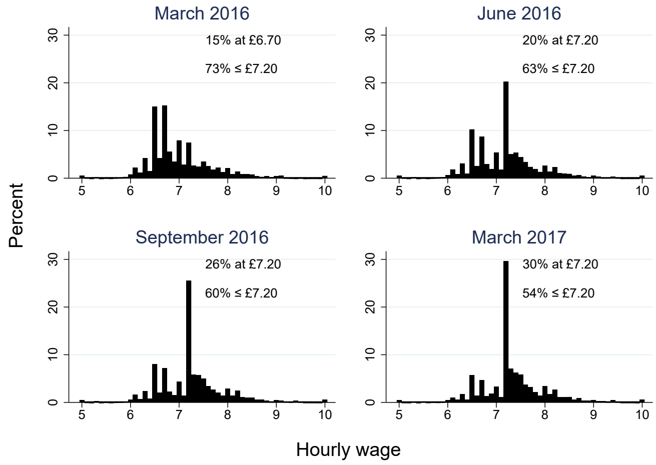
This change in the minimum wage structure is utilized to study the wage and employment effects of minimum wages in the care homes sector of the UK economy, a sector whose organizational structure makes it potentially particularly vulnerable to changes in wage costs induced by minimum wages. The changed minimum wage structure is also used as a means to identifying wage and employment spillovers because of the age-related change in the operation of minimum wages. Margins of adjustment other than employment are also explored.

The analysis finds that, on the labor demand side of things, care homes mostly seemed to manage to cope with the additional wage costs that resulted from the NLW as there is at best modest evidence of employment changes in response to the sizable wage cost shock that ensued, and no evidence of home exit resulting from this, at least in the short run. Conversely, and rather worryingly from the perspective of care home residents, the quality of care services appears to have significantly suffered as a consequence of the wage shock. Smaller improvement in care quality seems to be the main margin of adjustment we are able to identify amongst a range of possible firm responses.

The structure of wages by age also substantively changed, as there are significant wage spillovers for younger workers from the NLW introduction. Thus the main wage impact of the changed minimum wage structure was on both the wages of directly affected older workers and indirectly affected younger workers, but with less evidence of employment adjustment in response to these. Employers' preferences for fairness emerge as the most plausible explanation for the observed wage spillovers.

## 3.9 Figures

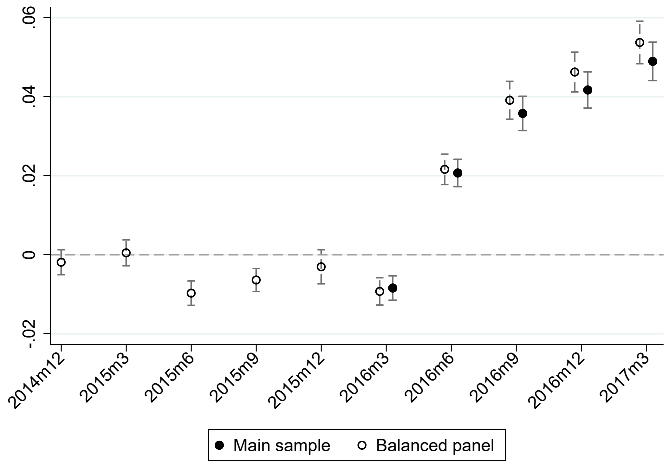
FIGURE 3.1: HOURLY WAGE DISTRIBUTION FOR CARE ASSISTANTS



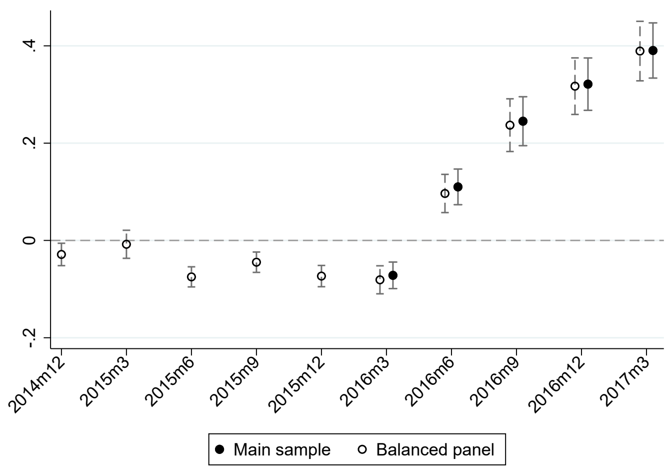

**Notes:** The graph shows the distribution of hourly wages for care assistants. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins.
**Source:** NMDS-SC.

FIGURE 3.2: EFFECT OF NLW BITE IN MARCH 2016 ON QUARTER-ON-QUARTER WAGE GROWTH

A. Effect of low-paid proportion in March 2016 on quarter-on-quarter wage growth



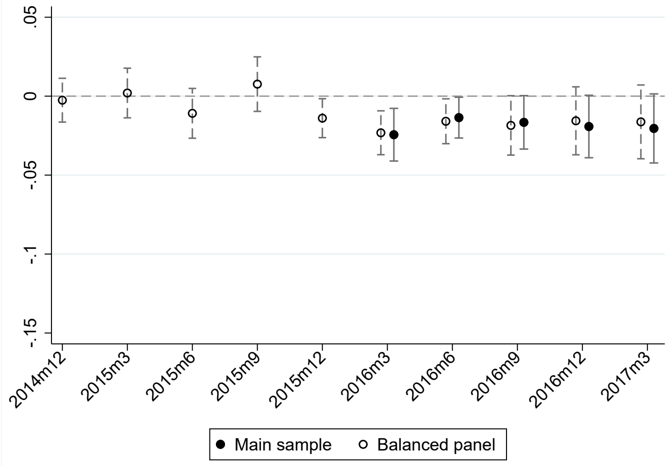

B. Effect of wage gap in March 2016 on quarter-on-quarter wage growth



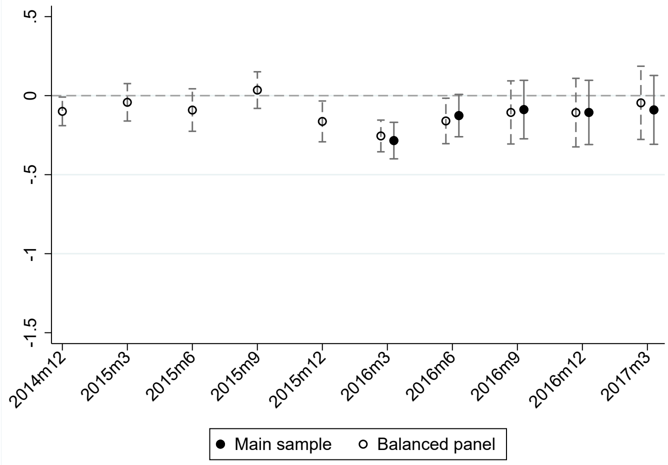

**Notes:** The graphs report the estimated coefficient $\beta_{1,t}$ from model (3.2). The outcome variable is the quarter-on-quarter change in log average wages in the pre-treatment period, and the long difference between March 2016 and quarter $t$ in the post-treatment period. The main regressor $MIN_{j,Mar2016}$ is the proportion of low-paid workers in Panel A and the wage gap in Panel B. The graphs report estimates for both a balanced panel of care homes always active between September 2015 and March 2017, and for the sample of firms used in the main analysis (i.e. the panel of firms always active between March 2016 and March 2017). The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

FIGURE 3.3: EFFECT OF NLW BITE IN MARCH 2016 ON QUARTER-ON-QUARTER EMPLOYMENT GROWTH

A. Effect of low-paid proportion in March 2016 on quarter-on-quarter employment growth



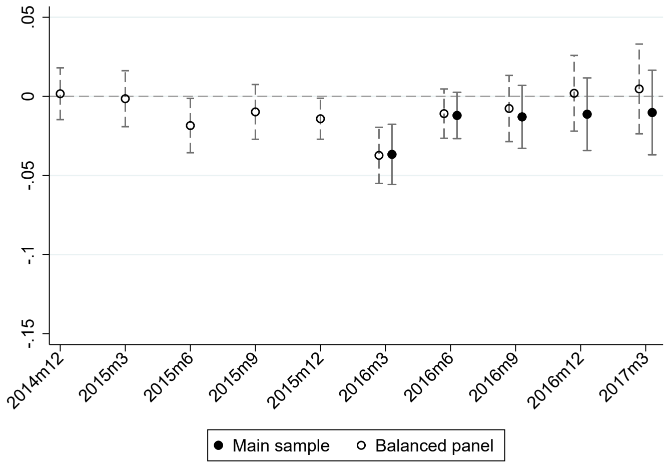

B. Effect of wage gap in March 2016 on quarter-on-quarter employment growth



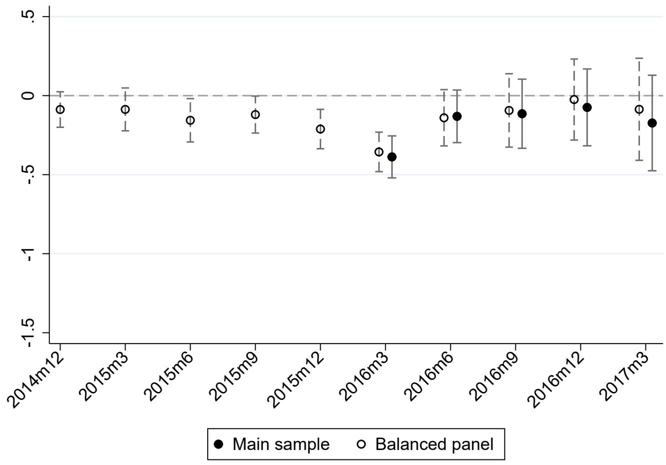

**Notes:** The graphs report the estimated coefficient $\beta_{2,t}$ from model (3.3). The outcome variable is the quarter-on-quarter change in log number of employees in the pre-treatment period, and the long difference between March 2016 and quarter $t$ in the post-treatment period. The main regressor $MIN_{j,Mar2016}$ is the proportion of low-paid workers in Panel A and the wage gap in Panel B. The graphs report estimates for both a balanced panel of care homes always active between September 2015 and March 2017, and for the sample of firms used in the main analysis (i.e. the panel of firms always active between March 2016 and March 2017). The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

FIGURE 3.4: EFFECT OF NLW BITE IN MARCH 2016 ON QUARTER-ON-QUARTER TOTAL HOURS GROWTH

A. Effect of low-paid proportion in March 2016 on quarter-on-quarter total hours growth

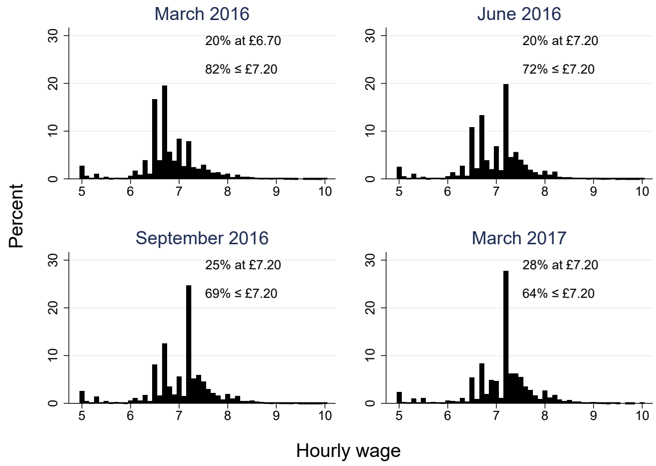



B. Effect of wage gap in March 2016 on quarter-on-quarter total hours growth



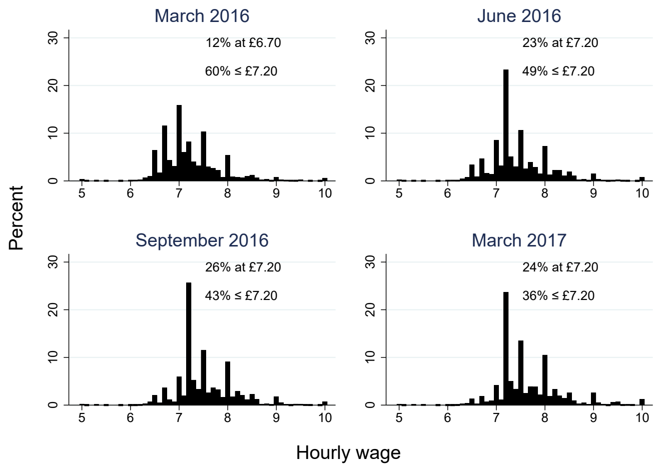

**Notes:** The graphs report the estimated coefficient $\beta_{2,t}$ from model (3.3). The outcome variable is the quarter-on-quarter change in log total weekly hours in the pre-treatment period, and the long difference between March 2016 and quarter $t$ in the post-treatment period. The main regressor $MIN_{j,Mar2016}$ is the proportion of low-paid workers in Panel A and the wage gap in Panel B. The graphs report estimates for both a balanced panel of care homes always active between September 2015 and March 2017, and for the sample of firms used in the main analysis (i.e. the panel of firms always active between March 2016 and March 2017). The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

FIGURE 3.5: HOURLY WAGE DISTRIBUTION FOR CARE ASSISTANTS UNDER 25




**Notes:** The graph shows the distribution of hourly wages for care assistants aged under 25. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins.
**Source:** NMDS-SC.

FIGURE 3.6: HOURLY WAGE DISTRIBUTION FOR DOMICILIARY CARERS UNDER 25




**Notes:** The graph shows the distribution of hourly wages for domiciliary carers aged under 25. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins.
**Source:** NMDS-SC.

## 3.10 Tables

TABLE 3.1: DESCRIPTIVE STATISTICS

| | Mar 2016 | | Jun 2016 | | Sep 2016 | | Mar 2017 | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Number of employees | 38.93 | 30.94 | 39.21 | 31.27 | 39.35 | 31.77 | 39.58 | 31.29 |
| Median | 32.00 | | 32.00 | | 33.00 | | 33.00 | |
| Proportion under 25 | 0.12 | 0.09 | 0.12 | 0.09 | 0.12 | 0.09 | 0.11 | 0.09 |
| Hourly wage | 7.55 | 1.08 | 7.70 | 1.09 | 7.76 | 1.08 | 7.85 | 1.08 |
| Hourly wage (25+) | 7.64 | 1.11 | 7.80 | 1.11 | 7.86 | 1.11 | 7.95 | 1.10 |
| Hourly wage (under 25) | 6.82 | 0.78 | 6.97 | 0.81 | 7.03 | 0.80 | 7.11 | 0.80 |
| Weekly hours | 28.78 | 5.10 | 28.77 | 5.10 | 28.80 | 5.10 | 28.79 | 5.13 |
| Weekly earnings | 215.00 | 54.47 | 219.15 | 55.35 | 221.30 | 55.45 | 223.92 | 56.12 |
| Proportion female | 0.84 | 0.13 | 0.84 | 0.13 | 0.84 | 0.13 | 0.84 | 0.13 |
| Age | 42.69 | 4.60 | 42.74 | 4.58 | 42.82 | 4.61 | 43.04 | 4.63 |
| Proportion carer | 0.56 | 0.16 | 0.56 | 0.16 | 0.56 | 0.16 | 0.56 | 0.16 |
| Prop. with nursing qual. | 0.04 | 0.07 | 0.04 | 0.07 | 0.04 | 0.07 | 0.04 | 0.07 |
| Occupancy rate | 0.92 | 0.15 | 0.92 | 0.14 | 0.92 | 0.14 | 0.92 | 0.14 |
| Number of firms | 4.134 | | 4.134 | | 4.134 | | 4.134 | |

**Notes:** The table reports the mean and standard deviation of a set of firm-level variables for the balanced sample of firms used in the analysis.
**Source:** NMDS-SC.

TABLE 3.2: THE BITE OF THE NATIONAL LIVING WAGE

| | Mar 2016 | | Jun 2016 | | Sep 2016 | | Mar 2017 | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Prop. paid less than MW | 0.51 | 0.32 | 0.35 | 0.35 | 0.29 | 0.33 | 0.22 | 0.30 |
| Prop. paid less than MW (25+) | 0.55 | 0.34 | 0.37 | 0.37 | 0.30 | 0.35 | 0.23 | 0.31 |
| NLW gap | 0.04 | 0.04 | 0.03 | 0.04 | 0.02 | 0.04 | 0.02 | 0.03 |
| NLW gap (25+) | 0.04 | 0.04 | 0.03 | 0.04 | 0.02 | 0.03 | 0.02 | 0.03 |
| Prop. paid at MW | 0.03 | 0.10 | 0.13 | 0.23 | 0.17 | 0.25 | 0.20 | 0.26 |
| Prop. paid at MW (25+) | 0.03 | 0.10 | 0.13 | 0.23 | 0.17 | 0.26 | 0.20 | 0.27 |

**Notes:** The table reports the mean and standard deviation of a set of firm-level variables for the balanced sample of firms used in the analysis. The NLW GAP is defined as $GAP_j = \frac{\sum_i h_{ij} \max\{W_{ij}^{min} - W_{ij}\ ,\ 0\}}{\sum_i h_{ij} W_{ij}}$, where $h_{ij}$ is weekly hours worked by worker $i$ in firm $j$, $W_{ij}$ is the hourly wage of worker $i$ in firm $j$ and $W_{ij}^{min}$ is the new age-specific minimum wage (i.e. £3.87 for workers aged 16-17, £5.30 for workers aged 18-20, £6.70 for workers aged 21-24 and £7.20 for older workers).
**Source:** NMDS-SC.

TABLE 3.3: WAGE EQUATIONS

Change in log average hourly wage

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.023*** | 0.021*** | | |
| | (0.002) | (0.002) | | |
| Initial NLW gap | | | 0.136*** | 0.110*** |
| | | | (0.018) | (0.019) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.020 | | | |
| F-stat ($\beta_{1,t} = 0$): | 196.54 | 136.59 | 59.89 | 34.45 |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.037*** | 0.036*** | | |
| | (0.002) | (0.002) | | |
| Initial NLW gap | | | 0.264*** | 0.244*** |
| | | | (0.023) | (0.026) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.028 | | | |
| F-stat ($\beta_{1,t} = 0$): | 343.03 | 260.48 | 126.94 | 90.87 |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.049*** | 0.049*** | | |
| | (0.002) | (0.002) | | |
| Initial NLW gap | | | 0.400*** | 0.390*** |
| | | | (0.027) | (0.029) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.040 | | | |
| F-stat ($\beta_{1,t} = 0$): | 458.98 | 388.53 | 221.82 | 182.05 |

**Notes:** The table reports the estimated coefficient $\beta_{1,t}$ from model (3.2). The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.4: EMPLOYMENT EQUATIONS

Change in log number of employees

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.012 | -0.020* | | | | |
| | (0.011) | (0.011) | | | | |
| Initial NLW gap | | | -0.033 | -0.088 | | |
| | | | (0.103) | (0.111) | | |
| Change in log average hourly wage | | | | | -0.410* | -0.225 |
| | | | | | (0.230) | (0.289) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.014 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log number of employees as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.5: HOURS EQUATIONS

Change in log total weekly hours

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.011 | -0.010 | | | | |
| | (0.013) | (0.014) | | | | |
| Initial NLW gap | | | -0.158 | -0.173 | | |
| | | | (0.146) | (0.154) | | |
| Change in log average hourly wage | | | | | -0.212 | -0.444 |
| | | | | | (0.280) | (0.404) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.021 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log total weekly hours as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.6: WAGE SPILLOVER EQUATIONS

Change in log average hourly wage for employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.017*** | 0.017*** | | | | |
| | (0.003) | (0.003) | | | | |
| Initial NLW gap (25+) | | | 0.109*** | 0.110*** | | |
| | | | (0.031) | (0.033) | | |
| Change in log average wage (25+) | | | | | 0.643*** | 0.592*** |
| | | | | | (0.103) | (0.152) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.022 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.029*** | 0.031*** | | | | |
| | (0.004) | (0.004) | | | | |
| Initial NLW gap (25+) | | | 0.220*** | 0.234*** | | |
| | | | (0.040) | (0.043) | | |
| Change in log average wage (25+) | | | | | 0.747*** | 0.724*** |
| | | | | | (0.089) | (0.118) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.030 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.033*** | 0.038*** | | | | |
| | (0.005) | (0.005) | | | | |
| Initial NLW gap (25+) | | | 0.268*** | 0.308*** | | |
| | | | (0.060) | (0.063) | | |
| Change in log average wage (25+) | | | | | 0.722*** | 0.654*** |
| | | | | | (0.100) | (0.125) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.041 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6). The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.7: EMPLOYMENT SPILLOVER EQUATIONS

Change in share of employees aged under 25

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.004 | -0.001 | | | | |
| | (0.004) | (0.004) | | | | |
| Initial NLW gap (25+) | | | -0.002 | 0.007 | | |
| | | | (0.034) | (0.038) | | |
| Change in log average wage (25+) | | | | | -0.026 | 0.014 |
| | | | | | (0.075) | (0.080) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.009 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of employees aged under 25 as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.8: TOTAL HOURS SPILLOVER EQUATIONS

Change in share of total weekly hours worked by employees aged under 25

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.002 | 0.001 | | | | |
| | (0.004) | (0.004) | | | | |
| Initial NLW gap (25+) | | | 0.009 | 0.020 | | |
| | | | (0.036) | (0.040) | | |
| Change in log average wage (25+) | | | | | 0.022 | 0.042 |
| | | | | | (0.078) | (0.084) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.008 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of total weekly hours worked by employees aged under 25 as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.9: DIFFERENCES BETWEEN THE CARE HOME AND DOMICILIARY CARE SECTORS

*March 2016*

| | Care homes | | Domiciliary care agencies | | Difference | |
|---|---|---|---|---|---|---|
| *Firm-level outcomes* | | | | | | |
| Number of employees | 38.93 | 30.94 | 62.90 | 70.52 | -23.97 | *** |
| Proportion under 25 | 0.12 | 0.09 | 0.12 | 0.09 | 0.01 | * |
| Number of firms | 4,134 | | 1,248 | | | |
| | | | | | | |
| *Worker-level outcomes* | | | | | | |
| Wage | 7.65 | 2.07 | 7.52 | 1.25 | 0.13 | *** |
| Wage (under 25) | 6.85 | 1.10 | 7.25 | 0.79 | -0.41 | *** |
| Prop. on ZHC | 0.07 | 0.25 | 0.64 | 0.48 | -0.57 | *** |
| Prop. on ZHC (under 25) | 0.12 | 0.33 | 0.67 | 0.47 | -0.54 | *** |
| Prop. on permanent contr. | 0.90 | 0.29 | 0.83 | 0.38 | 0.08 | *** |
| Prop. on temporary contr. | 0.01 | 0.12 | 0.05 | 0.23 | -0.04 | *** |
| Prop. bank worker | 0.07 | 0.26 | 0.04 | 0.20 | 0.03 | *** |
| Prop. agency worker | 0.00 | 0.04 | 0.05 | 0.23 | -0.05 | *** |
| Weekly hours | 28.43 | 11.80 | 12.27 | 15.89 | 16.16 | *** |
| Weekly earnings | 215.45 | 118.30 | 74.44 | 118.79 | 141.01 | *** |
| Prop. female | 0.84 | 0.37 | 0.87 | 0.34 | -0.02 | *** |
| Age | 42.31 | 13.92 | 41.65 | 13.39 | 0.65 | *** |
| Prop. carer | 0.55 | 0.50 | 0.81 | 0.39 | -0.26 | *** |
| Prop. with nursing qual. | 0.05 | 0.23 | 0.00 | 0.05 | 0.05 | *** |
| Number of workers | 181,888 | | 131,680 | | | |

**Notes:** The table reports the mean and standard deviation of a set of firm-level and worker-level variables for the employees working in care homes and domiciliary care agencies in the sample used in the analysis. The last column reports the difference in means between the care home and domiciliary care sectors and the associated significance level. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
**Source:** NMDS-SC.

TABLE 3.10: WAGE SPILLOVER EQUATIONS IN THE DOMICILIARY CARE SECTOR

Change in log average hourly wage for employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.026*** | 0.025*** | | | | |
| | (0.005) | (0.006) | | | | |
| Initial NLW gap (25+) | | | 0.183*** | 0.155** | | |
| | | | (0.069) | (0.072) | | |
| Change in log average wage (25+) | | | | | 0.953*** | 1.037*** |
| | | | | | (0.199) | (0.293) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.021 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.036*** | 0.035*** | | | | |
| | (0.006) | (0.007) | | | | |
| Initial NLW gap (25+) | | | 0.263*** | 0.226** | | |
| | | | (0.086) | (0.088) | | |
| Change in log average wage (25+) | | | | | 1.001*** | 1.001*** |
| | | | | | (0.173) | (0.227) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.030 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.046*** | 0.047*** | | | | |
| | (0.008) | (0.009) | | | | |
| Initial NLW gap (25+) | | | 0.443*** | 0.417*** | | |
| | | | (0.150) | (0.158) | | |
| Change in log average wage (25+) | | | | | 0.981*** | 0.892*** |
| | | | | | (0.175) | (0.178) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.044 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6). The sample is a balanced panel of domiciliary care agencies active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.11: PRODUCTIVITY

Change in log residents per worker hour

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.012 | 0.001 | | | | |
| | (0.015) | (0.015) | | | | |
| Initial NLW gap | | | 0.079 | 0.015 | | |
| | | | (0.159) | (0.169) | | |
| Change in log average wage | | | | | 0.015 | 0.037 |
| | | | | | (0.311) | (0.423) |
| Observations | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.011 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log residents per worker hour as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.12: QUALITY OF CARE SERVICES

Change in rating between March 2016 and March 2017 (latest rating)

*Panel A - Overall quality*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.142*** | -0.125*** | | | | |
| | (0.022) | (0.023) | | | | |
| Initial NLW gap | | | -1.146*** | -1.008*** | | |
| | | | (0.203) | (0.211) | | |
| Change in log avg wage | | | | | -2.441*** | -2.513*** |
| | | | | | (0.485) | (0.563) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.113 | | | | | |

*Panel B - Safe*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.095*** | -0.082*** | | | | |
| | (0.023) | (0.025) | | | | |
| Initial NLW gap | | | -0.815*** | -0.735*** | | |
| | | | (0.221) | (0.231) | | |
| Change in log avg wage | | | | | -1.611*** | -1.839*** |
| | | | | | (0.497) | (0.594) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.088 | | | | | |

*Panel C - Effective*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.091*** | -0.077*** | | | | |
| | (0.021) | (0.022) | | | | |
| Initial NLW gap | | | -0.628*** | -0.524** | | |
| | | | (0.200) | (0.207) | | |
| Change in log avg wage | | | | | -1.491*** | -1.294** |
| | | | | | (0.433) | (0.517) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.099 | | | | | |

TABLE 3.C12 CONTINUED: QUALITY OF CARE SERVICES

Change in rating between March 2016 and March 2017 (latest rating)

*Panel D - Caring*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.078*** | -0.077*** | | | | |
| | (0.015) | (0.016) | | | | |
| Initial NLW gap | | | -0.520*** | -0.506*** | | |
| | | | (0.150) | (0.156) | | |
| Change in log avg wage | | | | | -1.512*** | -1.266*** |
| | | | | | (0.335) | (0.404) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.035 | | | | | |

*Panel E - Responsive*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.091*** | -0.075*** | | | | |
| | (0.020) | (0.021) | | | | |
| Initial NLW gap | | | -0.795*** | -0.697*** | | |
| | | | (0.194) | (0.200) | | |
| Change in log avg wage | | | | | -1.460*** | -1.727*** |
| | | | | | (0.423) | (0.517) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.079 | | | | | |

*Panel F - Well-led*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.124*** | -0.110*** | | | | |
| | (0.024) | (0.026) | | | | |
| Initial NLW gap | | | -0.997*** | -0.883*** | | |
| | | | (0.226) | (0.236) | | |
| Change in log avg wage | | | | | -2.150*** | -2.202*** |
| | | | | | (0.521) | (0.604) |
| Observations | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 | 2,480 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.066 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in various measure of care quality as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the latest rating in the relevant line of enquiry as of March 2016, the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC and CQC.

TABLE 3.13: CLOSURES

Indicator for firm closure

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.004 | 0.007 | | |
| | (0.005) | (0.006) | | |
| Initial NLW gap | | | 0.046 | 0.078 |
| | | | (0.044) | (0.050) |
| Observations | 4,306 | 4,306 | 4,306 | 4,306 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.010 | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), using the probability of closure as of March 2017 as outcome variable. The sample is a balanced panel of care homes active between March 2016, unconditional on their survival until March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

## 3.A Aggregate Employment and Firm Dynamics

### 3.A.1 Aggregate Employment Effects

We explore the aggregate employment effects of the NLW introduction using a bunching approach as in Cengiz et al. (forthcoming). The bunching approach allows us to infer the effect on employment throughout the wage distribution by comparing the number of missing jobs below the minimum to the number of excess jobs above the minimum before and after the policy change. The main intuition behind this approach is that when a higher minimum wage is introduced, workers who used to be paid at a wage below the new minimum can no longer be paid at their old rate. As a consequence, provided that there is almost full compliance with the law, the mass of jobs at the bottom of the wage distribution should disappear. Some of these jobs will obtain the wage uprate and therefore appear at or right above the new minimum, some might be destroyed, and some other new jobs might be created through a labor supply effect. Therefore, the size of the excess mass above the new minimum provides an account of preserved and newly created jobs, and the sum of the excess and the missing mass measures the total employment change, whether positive or negative. It is reasonable to believe that the bulk of the dynamics will occur in a neighborhood of the new minimum, as changes in the upper tail of the wage distribution are unlikely to be driven by minimum wage changes.

To implement this strategy, we consider the entire workforce of care homes ever active between October 2015 and March 2017, therefore allowing for entries and exits. This allows us to investigate the aggregate employment effects of the NLW introduction. We collapse the individual data and calculate monthly employment counts at the local authority district level by £0.50 hourly wage bins from six months prior to a year after the NLW introduction, and from two pounds below to five pounds above the NLW rate of £7.20.[44] We then estimate how changes in the excess and missing mass by wage bin evolve relative to March 2016 adopting the following fixed effect framework:

$$N_{l,w,m} = \gamma_0 + \sum_{k=-4}^{8} \sum_{\tau \neq -1, \tau=-7}^{11} \gamma_{k,\tau} \cdot \mathbb{I}_w^k \times \mathbb{I}_m^\tau + \phi_{l,w,m} \tag{3.A1}$$

where $N_{l,w,m}$ is employment headcount in local authority $l$, wage bin $w$ and month $m$, $\mathbb{I}_w^k$ is an indicator taking value one if wage bin $w$ is $k$-bin distant from the £7.20 bin, $\mathbb{I}_m^\tau$ is an indicator taking value one if month $m$ is $\tau$ -month distant from April 2016 and $\phi_{l,w,m}$ a disturbance term. The key parameters of interest are $\gamma_{k,\tau}$ for $k = -4, \ldots, 8$ and $\tau = -7, \ldots, -2, 0, \ldots, 11$, as they trace the evolution of the missing and excess mass relative to the month before of the policy change. In this model, given that the timing of the NLW introduction is common to all local authorities, identification comes from variation

[44] While we do not have any ex-ante information on what is the range over which the minimum wage change can have distributional consequences, we draw on information in Figure 3.1 and restrict our analysis to wages between £5.20 and £11.20.

in the number of workers for which the minimum wage change is binding across local authorities.

In Figure 3.B8 in Appendix 3.B the vertical bars correspond to the estimated $\gamma_{k,\tau}$ for $k = -4, \ldots, 8$ and for selected values of $\tau$, namely $\tau = 2$ in the top left panel, $\tau = 5$ in the top right panel, $\tau = 8$ in the bottom left panel and $\tau = 11$ in the bottom right panel. For each bar, a capped line indicates the 95 percent confidence interval of the $\hat{\gamma}_{k,\tau}$. The connected dots indicate instead the cumulative sum of the bin-specific effects. Across all the panels, the missing mass is concentrated in the two wage bins right below the new minimum and the excess mass in the first bin above it, while employment changes in the other bins are very small and statistically indistinguishable from zero. The pattern of the cumulated effects suggests that jobs previously paid below the NLW are fully replaced by jobs in the three bins right above the new minimum, and that there are no spillover effects in the upper part of the wage distribution.

While the previous chart displays the change in mass by wage bin for selected post-treatment periods relative to March 2016, Figure 3.B9 in Appendix 3.B documents the evolution of the total number of jobs below the minimum $\alpha_\tau = \sum_{k=-4}^{-1} \gamma_\tau^k$ (missing mass), the total number of jobs above the minimum $\beta_\tau = \sum_{k=0}^{8} \gamma_\tau^k$ (excess mass), and their sum $\Delta_\tau = \alpha_\tau + \beta_\tau$ (net excess mass) for $\tau = -7, \ldots, -2, 0, \ldots, 11$. The numbers reported at the bottom of the figure are the point estimates $\hat{\Delta}_\tau$. The graph shows a sharp reduction in the number of jobs below the NLW between the six months prior and the twelve months after its implementation. The below mass decreases by a statistically significant amount exactly in April 2016 – showing that the minimum wage increase had real bite – and remains persistently negative throughout the following twelve months. The evolution of the excess mass almost perfectly mirrors this pattern, displaying a significant and positive jump from $\tau = 0$ onwards. This is confirmed by the behavior of the "net excess mass" that is very small in magnitude and never statistically different from zero. Interestingly enough, there is no pre-trend in $\hat{\alpha}_\tau$ nor $\hat{\beta}_\tau$. According to these result, there is little if no indication of negative aggregate employment effects due to the NLW introduction.

Following our previous investigation of potential spillover effects, we extend the bunching framework to account for different patterns between workers aged under 25, and workers aged 25 and over. In practice, we augment the bunching model interacting the main regressor with an age-group dummy.[45] Results are reported in Figures 3.B10 and 3.B11 in Appendix 3.B for adult workers, and in Figures 3.B12 and 3.B13 for younger workers. The age-specific patterns are very similar to the aggregate ones. The evolution of the net excess mass in Figure 3.B13 seems to suggest a mild but nonetheless small and statistically insignificant negative employment effect for younger workers. All in all, we take this bunching exercise as evidence that the NLW introduction did not have any significant aggregate employment effects.

[45]This model requires collapsing the data by age category, wage bin, month and local authority.

### 3.A.2 Aggregate Firm Dynamics

We are also interested in whether the NLW introduction had an impact on firm entry. We therefore consider all firms ever active in the period between March 2016 and March 2017, allowing for both entries into and exits out of the sample. Estimating reduced-form linear probability models for the probability of entry as we did above for the probability of exit is infeasible, since we do not have a measure of the minimum wage bite for entrants. We therefore collapse the data at the local authority district level and run reduced-form regressions of the following form:

$$E_{l,t} = \alpha_6 + \beta_6 \cdot MIN_{l,Mar2016} + Z'_{l,Mar2016} \cdot \gamma_6 + \omega_{l,t} \tag{3.A2}$$

where $E_{l,t}$ is the proportion of entrants in local authority $l$ between March 2016 and time $t$ – where $t$ can be June 2016, September 2016 or March 2017 –, $MIN_{l,Mar2016}$ is either the proportion of low-paid workers or the wage gap at local authority level in March 2016, and $Z$ is a vector of local-authority controls including the proportion of female workers, average age, the proportion working as care assistants, the proportion with nursing qualification, the occupancy rate and a set of regional dummies.[46] For entries between March 2016 and March 2017, reduced form estimates are reported in columns (1) to (4) of Panel C of Table 3.C15 in Appendix 3.C. Columns (5) and (6) instead show structural form estimates in which $MIN_{l,Mar2016}$ is used as an instrument for the change in the logarithm of the average wage in the local authority $\Delta \ln W_{l,t}$.[47] The statistical insignificance of the estimated coefficients and their limited size indicate that the NLW introduction did not have an impact on firm entry at the local authority level.

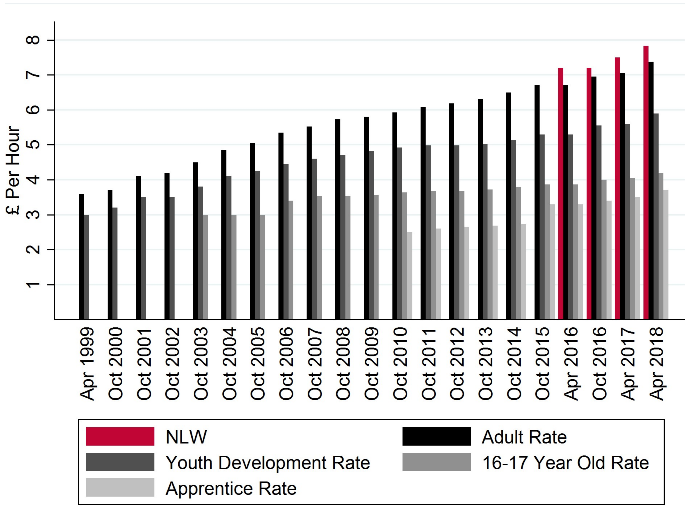
For completeness, we also report reduced-form and structural-form estimates for firm exits at the local authority level in Table 3.C16 in Appendix 3.C. Consistently with the firm-level results, we do not find evidence of a detrimental effect of the NLW introduction on care home survival.

[46] Local authority district areas as defined by ONS split England into 326 areas of local governance.

[47] Estimates for entries between March and June 2016 are reported in Panel A of Table 3.C15 in Appendix 3.C, while those for entries between March and September 2016 in Panel B of the same table.

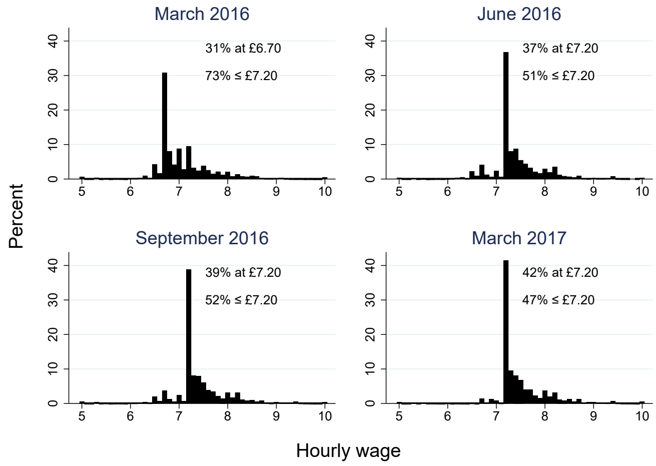
## 3.B Appendix Figures

FIGURE 3.B1: NATIONAL MINIMUM WAGE RATES 1999-2018



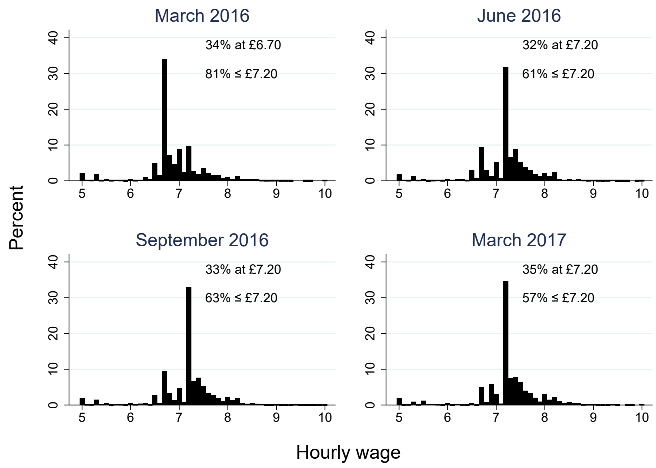

**Notes:** The graph reports the various minimum wage rates in the UK between 1999 and 2018. The apprentice rate applies to apprentices. The 16-17 year-old rate to workers aged 16 and 17. The youth development rate to workers aged 18-20. The adult rate applied to workers aged 21 and over until March 2016. From April 2016, the adult rate applies to workers aged 21-24 and the NLW to those aged 25 and over.
**Source:** Low Pay Commission.

FIGURE 3.B2: HOURLY WAGE DISTRIBUTION FOR CARE ASSISTANTS: UPDATED WAGES




**Notes:** The graph shows the distribution of hourly wages for care assistants. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins. The top left panel includes workers with wage updates between October 2015 and March 2016, the top right panel between April and June 2016, the bottom left panel between April and September 2016, and the bottom right panel between April 2016 and March 2017.
**Source:** NMDS-SC.

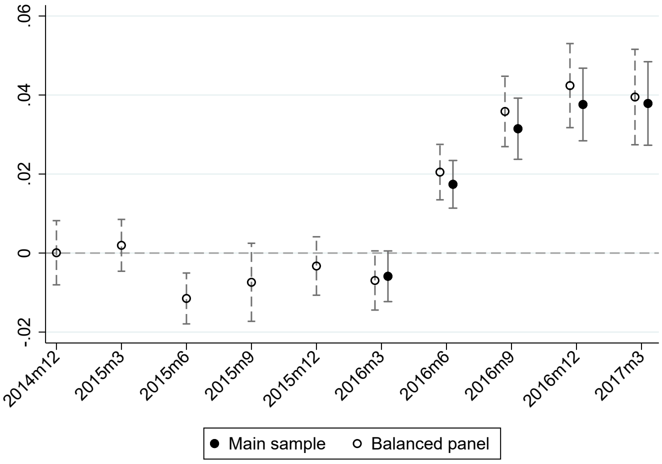
FIGURE 3.B3: HOURLY WAGE DISTRIBUTION FOR CARE ASSISTANTS UNDER 25: UPDATED WAGES



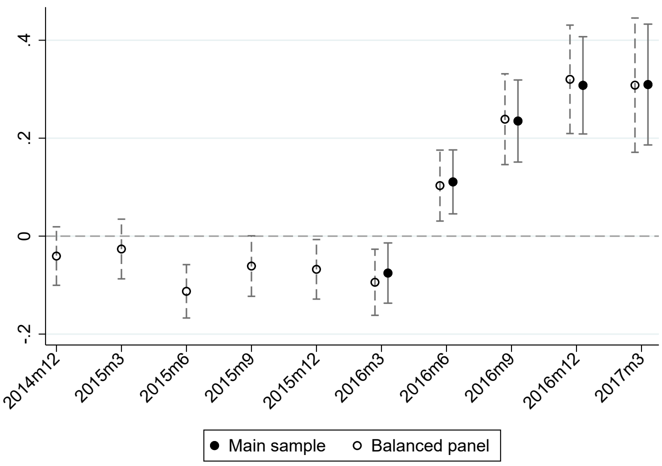

**Notes:** The graph shows the distribution of hourly wages for care assistants aged under 25. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins. The top left panel includes workers with wage updates between October 2015 and March 2016, the top right panel between April and June 2016, the bottom left panel between April and September 2016, and the bottom right panel between April 2016 and March 2017.
**Source:** NMDS-SC.

FIGURE 3.B4: EFFECT OF NLW BITE IN MARCH 2016 ON QUARTER-ON-QUARTER WAGE SPILLOVERS

A. Effect of low-paid proportion among 25s and over in March 2016 on quarter-on-quarter wage growth among under 25s

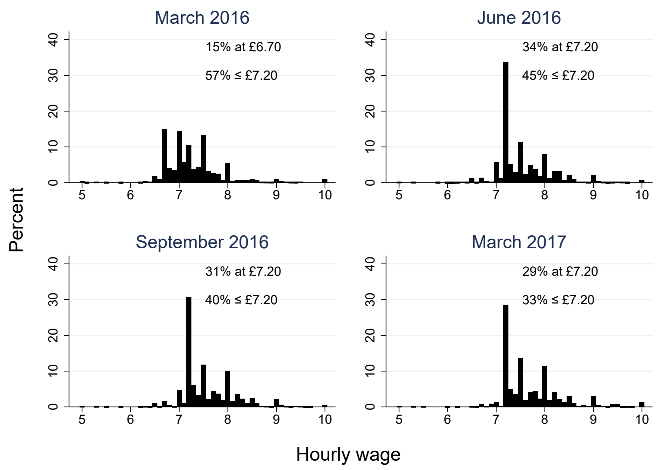



B. Effect of wage gap among 25s and over in March 2016 on quarter-on-quarter wage growth among under 25s




**Notes:** The graphs report the estimated coefficient $\beta_{4,t}$ from model (3.5). The outcome variable is the quarter-on-quarter change in log average wage for workers aged under 25 in March 2016 in the pre-treatment period, and the long difference between March 2016 and quarter $t$ in the post-treatment period. The main regressor $MIN^{O}_{j,Mar2016}$ is the proportion of low-paid workers aged 25 and over in Panel A and the wage gap for workers aged 25 and over in Panel B. The graphs report estimates for both a balanced panel of care homes always active between September 2015 and March 2017, and for the sample of firms used in the main analysis (i.e. the panel of firms always active between March 2016 and March 2017). The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

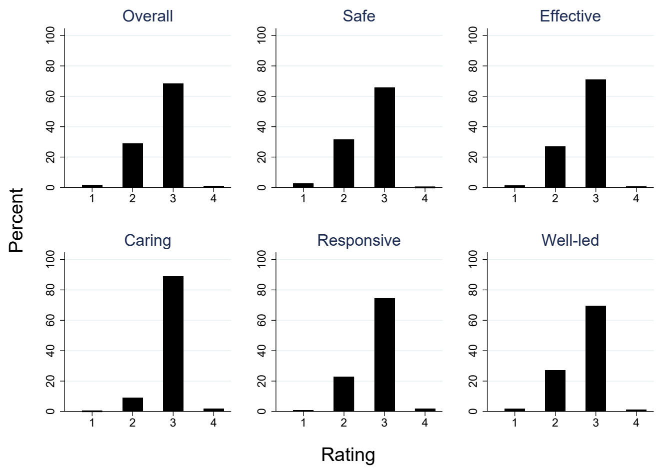
FIGURE 3.B5: HOURLY WAGE DISTRIBUTION FOR DOMICILIARY CARERS UNDER 25: UPDATED WAGES



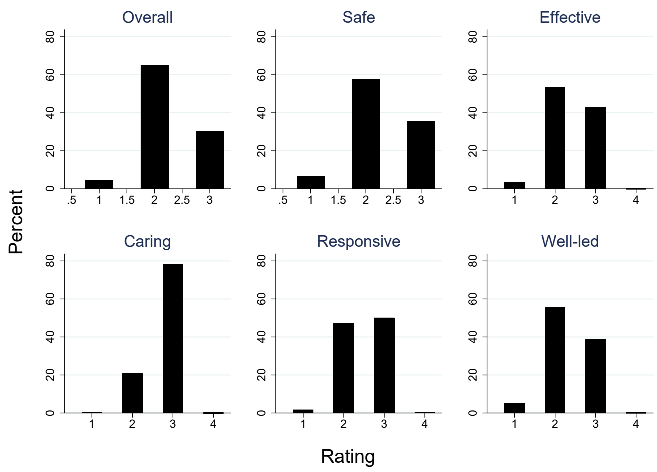

**Notes:** The graph shows the distribution of hourly wages for domiciliary carers aged under 25. The distribution is censored at £5.00 and £10.00. The data are binned into £0.10 bins. The top left panel includes workers with wage updates between October 2015 and March 2016, the top right panel between April and June 2016, the bottom left panel between April and September 2016, and the bottom right panel between April 2016 and March 2017.
**Source:** NMDS-SC.

FIGURE 3.B6: DISTRIBUTION OF RATINGS IN MARCH 2016

A. Sample of homes inspected by CQC before March 2016



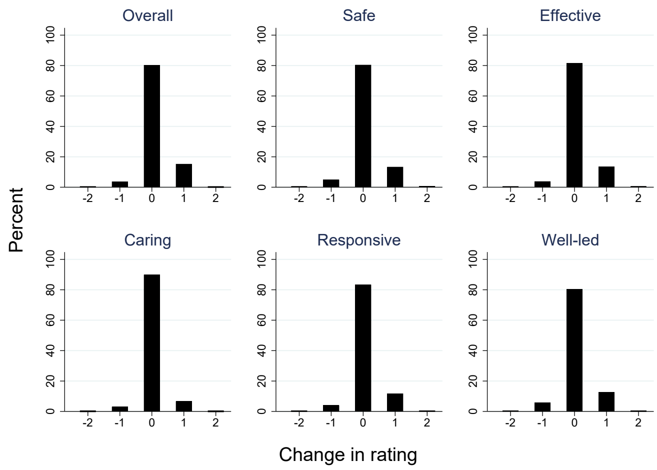

B. Sample of homes inspected by CQC both before and after March 2016



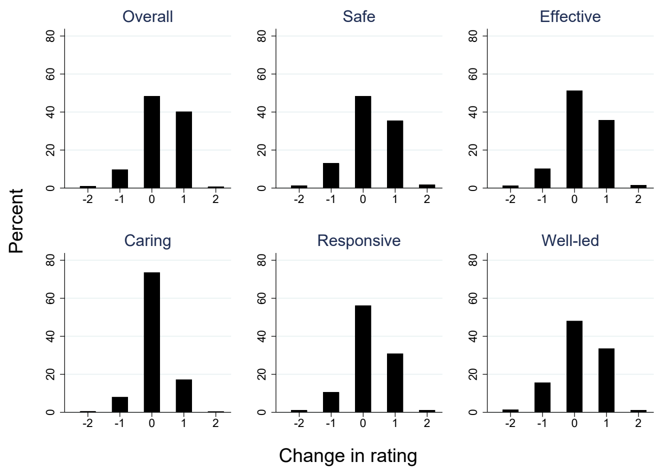

**Notes:** The graph reports the distribution of ratings by key line of enquiry in March 2016. Panel A is based on the sample of 2480 homes with CQC ratings as of March 2016. Panel B is based on the subgroup of firms that were inspected and rated by CQC both before and after March 2016. Legend: 1 = inadequate, 2 = requires improvement, 3 = good, 4 = outstanding.
**Source:** NMDS-SC and CQC.

FIGURE 3.B7: DISTRIBUTION OF CHANGE IN RATINGS BETWEEN MARCH 2016 AND MARCH 2017

A. Sample of homes inspected by CQC before March 2016

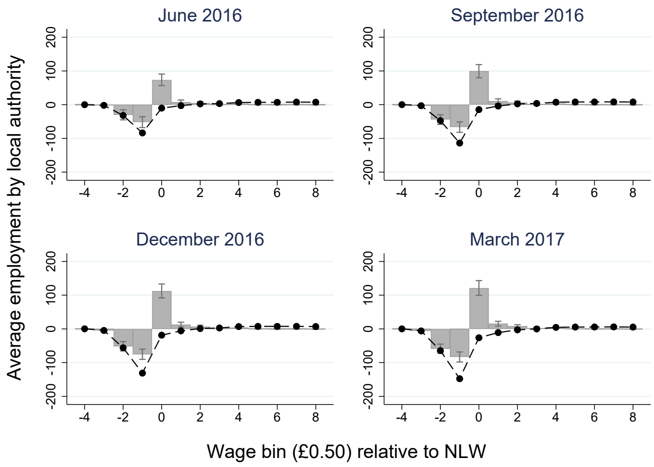



B. Sample of homes inspected by CQC both before and after March 2016



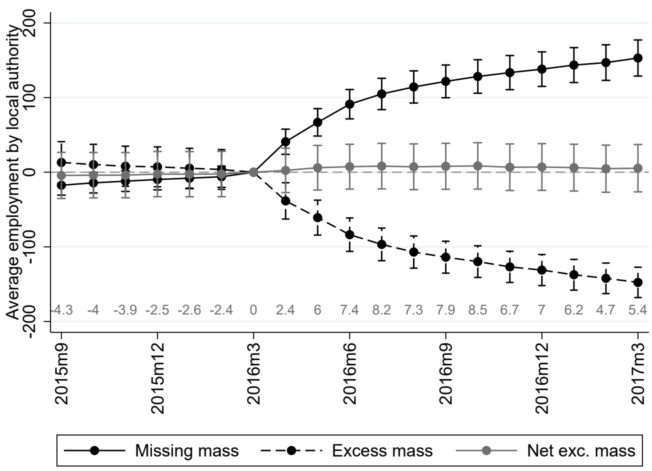

**Notes:** The graph reports the distribution of the change in ratings by key line of enquiry in March 2016. Panel A is based on the sample of 2480 homes with CQC ratings as of March 2016. Panel B is based on the subgroup of firms that were inspected and rated by CQC both before and after March 2016.
**Source:** NMDS-SC and CQC.

FIGURE 3.B8: CHANGE IN EMPLOYMENT BY WAGE BIN RELATIVE TO MARCH 2016



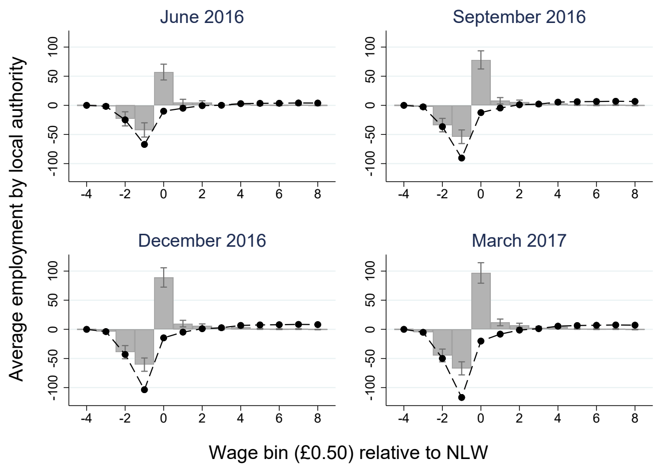

**Notes:** The vertical bars correspond to the estimated $\gamma_\tau^k$ from equation (3.A1) for wage bins $k = -4, \ldots, 8$ and for $\tau = 2$ in the top left panel, $\tau = 5$ in the top right panel, $\tau = 8$ in the bottom left panel and $\tau = 11$ in the bottom right panel. Capped lines indicate 95 percent confidence intervals, computed using robust standard errors. The connected dots indicate the cumulative sum of the bin-specific effects.
**Source:** NMDS-SC.

FIGURE 3.B9: EVOLUTION OF MISSING AND EXCESS MASS



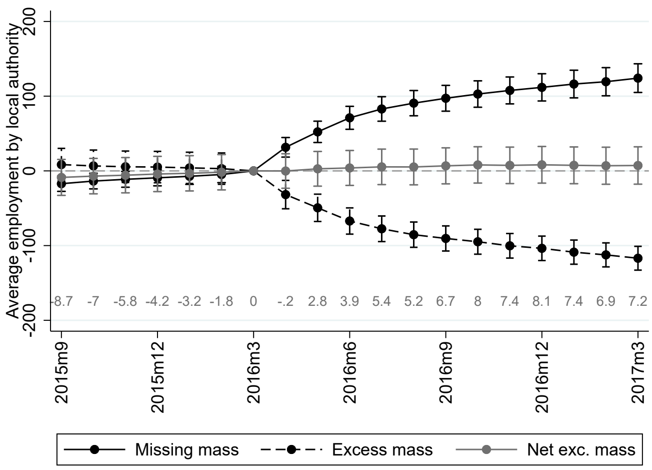

**Notes:** Based on model (3.A1), missing mass is the total number of jobs below the minimum $\alpha_\tau = \sum_{k=-1}^{-1} \gamma_\tau^k$, excess mass the total number of jobs above the minimum $\beta_\tau = \sum_{k=0}^{8} \gamma_\tau^k$, net excess mass the sum $\Delta_\tau = \alpha_\tau + \beta_\tau$. Coefficient estimates of the net excess mass are reported in grey at the bottom of the figure. Vertical bars indicate 95 percent confidence intervals, computed using robust standard errors.
**Source:** NMDS-SC.

FIGURE 3.B10: CHANGE IN EMPLOYMENT BY WAGE BIN RELATIVE TO MARCH 2016: EMPLOYEES AGED 25 AND OVER



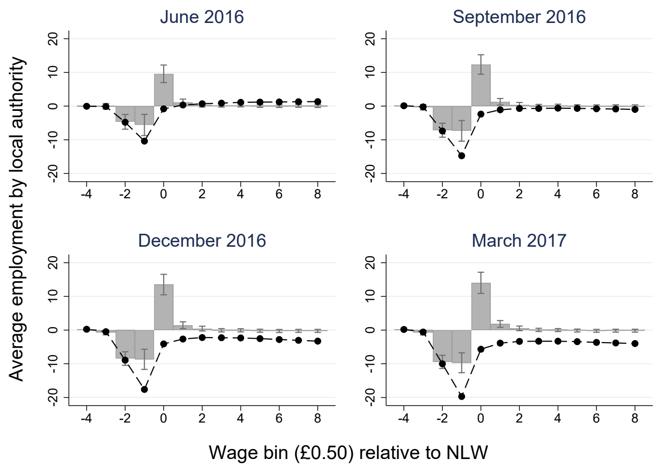

**Notes:** The vertical bars correspond to the estimated $\gamma_\tau^k$ from equation (3.A1) for wage bins $k = -4, \ldots, 8$ and for $\tau = 2$ in the top left panel, $\tau = 5$ in the top right panel, $\tau = 8$ in the bottom left panel and $\tau = 11$ in the bottom right panel. Capped lines indicate 95 percent confidence intervals, computed using robust standard errors. The connected dots indicate the cumulative sum of the bin-specific effects.
**Source:** NMDS-SC.

FIGURE 3.B11: EVOLUTION OF MISSING AND EXCESS MASS: EMPLOYEES AGED 25 AND OVER



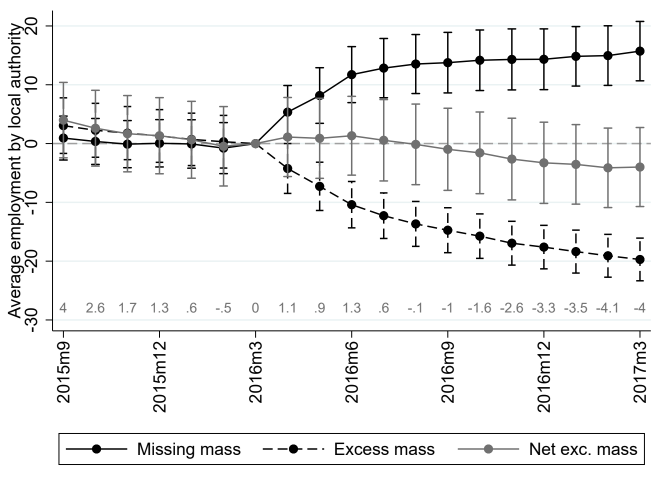

**Notes:** Based on model (3.A1), missing mass is the total number of jobs below the minimum $\alpha_\tau = \sum_{k=-1}^{-1} \gamma_\tau^k$, excess mass the total number of jobs above the minimum $\beta_\tau = \sum_{k=0}^{8} \gamma_\tau^k$, net excess mass the sum $\Delta_\tau = \alpha_\tau + \beta_\tau$. Coefficient estimates of the net excess mass are reported in grey at the bottom of the figure. Vertical bars indicate 95 percent confidence intervals, computed using robust standard errors.
**Source:** NMDS-SC.

FIGURE 3.B12: CHANGE IN EMPLOYMENT BY WAGE BIN RELATIVE TO MARCH 2016: EMPLOYEES AGED UNDER 25




**Notes:** The vertical bars correspond to the estimated $\gamma_\tau^k$ from equation (3.A1) for wage bins $k = -4, \ldots, 8$ and for $\tau = 2$ in the top left panel, $\tau = 5$ in the top right panel, $\tau = 8$ in the bottom left panel and $\tau = 11$ in the bottom right panel. Capped lines indicate 95 percent confidence intervals, computed using robust standard errors. The connected dots indicate the cumulative sum of the bin-specific effects.
**Source:** NMDS-SC.

FIGURE 3.B13: EVOLUTION OF MISSING AND EXCESS MASS: EMPLOYEES AGED UNDER 25




**Notes:** Based on model (3.A1), missing mass is the total number of jobs below the minimum $\alpha_\tau = \sum_{k=-1}^{-1} \gamma_\tau^k$, excess mass the total number of jobs above the minimum $\beta_\tau = \sum_{k=0}^{8} \gamma_\tau^k$, net excess mass the sum $\Delta_\tau = \alpha_\tau + \beta_\tau$. Coefficient estimates of the net excess mass are reported in grey at the bottom of the figure. Vertical bars indicate 95 percent confidence intervals, computed using robust standard errors.
**Source:** NMDS-SC.

## 3.C Appendix Tables

TABLE 3.C1: WEEKLY EARNINGS EQUATIONS

Change in log average weekly earnings

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.022*** | 0.021*** | | | | |
| | (0.004) | (0.005) | | | | |
| Initial NLW gap | | | 0.141*** | 0.122*** | | |
| | | | (0.038) | (0.042) | | |
| Change in log average wage | | | | | 1.015*** | 1.116*** |
| | | | | | (0.219) | (0.309) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.019 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.041*** | 0.040*** | | | | |
| | (0.006) | (0.007) | | | | |
| Initial NLW gap | | | 0.333*** | 0.318*** | | |
| | | | (0.054) | (0.060) | | |
| Change in log average wage | | | | | 1.123*** | 1.302*** |
| | | | | | (0.167) | (0.211) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.029 | | | | | |

TABLE 3.C1 CONTINUED: WEEKLY EARNINGS EQUATIONS

Change in log average weekly earnings

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.063*** | 0.066*** | | | | |
| | (0.007) | (0.008) | | | | |
| Initial NLW gap | | | 0.454*** | 0.455*** | | |
| | | | (0.077) | (0.082) | | |
| Change in log average wage | | | | | 1.353*** | 1.166*** |
| | | | | | (0.150) | (0.193) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.040 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log average weekly earnings as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C2: EMPLOYMENT EQUATIONS

Change in log number of employees

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.011* | -0.014** | | | | |
| | (0.006) | (0.007) | | | | |
| Initial NLW gap | | | -0.104 | -0.127* | | |
| | | | (0.065) | (0.068) | | |
| Change in log average wage | | | | | -0.661** | -1.157* |
| | | | | | (0.325) | (0.676) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.003 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.011 | -0.016* | | | | |
| | (0.008) | (0.009) | | | | |
| Initial NLW gap | | | -0.048 | -0.086 | | |
| | | | (0.087) | (0.094) | | |
| Change in log average wage | | | | | -0.460* | -0.354 |
| | | | | | (0.246) | (0.395) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.007 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log number of employees as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C3: TOTAL HOURS EQUATIONS

Change in log total weekly hours

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.012* | -0.012 | | | | |
| | (0.007) | (0.007) | | | | |
| Initial NLW gap | | | -0.122 | -0.132 | | |
| | | | (0.078) | (0.085) | | |
| Change in log average wage | | | | | -0.589 | -1.206 |
| | | | | | (0.371) | (0.850) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.008 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | -0.010 | -0.013 | | | | |
| | (0.009) | (0.010) | | | | |
| Initial NLW gap | | | -0.078 | -0.114 | | |
| | | | (0.101) | (0.111) | | |
| Change in log average wage | | | | | -0.362 | -0.468 |
| | | | | | (0.289) | (0.469) |
| Observations | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 | 4,134 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.015 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log total weekly hours as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C4: WAGE EQUATIONS FOR EMPLOYEES AGED 25 AND OVER

Change in log average hourly wage for workers aged 25 and over

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.028*** | 0.027*** | | |
| | (0.002) | (0.002) | | |
| Initial NLW gap (25+) | | | 0.204*** | 0.186*** |
| | | | (0.024) | (0.026) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.022 | | | |
| F-stat ($\beta_{1,t} = 0$): | 195.64 | 145.20 | 74.60 | 49.64 |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.043*** | 0.042*** | | |
| | (0.002) | (0.003) | | |
| Initial NLW gap (25+) | | | 0.339*** | 0.323*** |
| | | | (0.028) | (0.031) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.030 | | | |
| F-stat ($\beta_{1,t} = 0$): | 345.61 | 259.28 | 146.53 | 109.92 |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.052*** | 0.052*** | | |
| | (0.003) | (0.003) | | |
| Initial NLW gap (25+) | | | 0.467*** | 0.471*** |
| | | | (0.031) | (0.034) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.041 | | | |
| F-stat ($\beta_{1,t} = 0$): | 384.88 | 330.08 | 227.40 | 197.16 |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{1,t}$ from model (3.2), using the change in log average wage for workers aged 25 and over as outcome and measures of the NLW bite among workers aged 25 and over as main regressor. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C5: WEEKLY EARNINGS EQUATIONS FOR EMPLOYEES AGED UNDER 25

Change in log average weekly earnings for workers aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.009 | 0.011 | | | | |
| | (0.012) | (0.013) | | | | |
| Initial NLW gap (25+) | | | 0.046 | 0.079 | | |
| | | | (0.123) | (0.130) | | |
| Change in log average wage (25+) | | | | | 0.423 | 0.423 |
| | | | | | (0.490) | (0.688) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.023 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.027* | 0.033** | | | | |
| | (0.015) | (0.017) | | | | |
| Initial NLW gap (25+) | | | 0.209 | 0.272* | | |
| | | | (0.145) | (0.156) | | |
| Change in log average wage (25+) | | | | | 0.791** | 0.840* |
| | | | | | (0.389) | (0.472) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.031 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.057*** | 0.061*** | | | | |
| | (0.019) | (0.020) | | | | |
| Initial NLW gap (25+) | | | 0.471*** | 0.509*** | | |
| | | | (0.175) | (0.190) | | |
| Change in log average wage (25+) | | | | | 1.174*** | 1.082*** |
| | | | | | (0.389) | (0.398) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.043 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in log average weekly earnings for workers aged under 25 as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies. **Source:** NMDS-SC.

TABLE 3.C6: EMPLOYMENT SPILLOVER EQUATIONS

Change in share of employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.002 | -0.001 | | | | |
| | (0.002) | (0.002) | | | | |
| Initial NLW gap (25+) | | | -0.019 | -0.018 | | |
| | | | (0.019) | (0.022) | | |
| Change in log average wage (25+) | | | | | -0.052 | -0.099 |
| | | | | | (0.083) | (0.118) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.001 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.001 | -0.001 | | | | |
| | (0.003) | (0.003) | | | | |
| Initial NLW gap (25+) | | | 0.000 | -0.013 | | |
| | | | (0.026) | (0.028) | | |
| Change in log average wage (25+) | | | | | -0.034 | -0.039 |
| | | | | | (0.071) | (0.086) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.004 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of employees aged under 25 as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C7: TOTAL HOURS SPILLOVER EQUATIONS

Change in share of total weekly hours worked by employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.002 | -0.001 | | | | |
| | (0.002) | (0.002) | | | | |
| Initial NLW gap (25+) | | | -0.023 | -0.017 | | |
| | | | (0.021) | (0.023) | | |
| Change in log average wage (25+) | | | | | -0.027 | -0.094 |
| | | | | | (0.088) | (0.128) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.000 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.001 | -0.000 | | | | |
| | (0.003) | (0.003) | | | | |
| Initial NLW gap (25+) | | | -0.004 | -0.013 | | |
| | | | (0.027) | (0.029) | | |
| Change in log average wage (25+) | | | | | -0.003 | -0.039 |
| | | | | | (0.074) | (0.090) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.003 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of total weekly hours worked by employees aged under 25 as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C8: WAGE EQUATIONS IN THE DOMICILIARY CARE SECTOR

Change in log average hourly wage

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.026*** | 0.027*** | | |
| | (0.003) | (0.004) | | |
| Initial NLW gap | | | 0.061** | 0.060** |
| | | | (0.030) | (0.030) |
| Observations | 1,248 | 1,248 | 1,248 | 1,248 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.016 | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.038*** | 0.037*** | | |
| | (0.004) | (0.004) | | |
| Initial NLW gap | | | 0.095** | 0.090** |
| | | | (0.041) | (0.039) |
| Observations | 1,248 | 1,248 | 1,248 | 1,248 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.023 | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.053*** | 0.050*** | | |
| | (0.005) | (0.006) | | |
| Initial NLW gap | | | 0.176** | 0.168** |
| | | | (0.077) | (0.073) |
| Observations | 1,248 | 1,248 | 1,248 | 1,248 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.033 | | | |

**Notes:** The table reports the estimated coefficient $\beta_{1,t}$ from model (3.2). The sample is a balanced panel of domiciliary care agencies active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C9: WAGE EQUATIONS FOR EMPLOYEES AGED 25 AND OVER IN THE DOMICILIARY CARE SECTOR

Change in log average hourly wage for workers aged 25 and over

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.026*** | 0.026*** | | |
| | (0.004) | (0.004) | | |
| Initial NLW gap (25+) | | | 0.169*** | 0.149** |
| | | | (0.058) | (0.059) |
| Observations | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.018 | | | |
| F-stat ($\beta_{1,t} = 0$): | 51.84 | 44.69 | 8.47 | 6.51 |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.036*** | 0.035*** | | |
| | (0.004) | (0.004) | | |
| Initial NLW gap (25+) | | | 0.266*** | 0.226*** |
| | | | (0.078) | (0.077) |
| Observations | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.026 | | | |
| F-stat ($\beta_{1,t} = 0$): | 82.28 | 63.85 | 11.55 | 8.57 |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.050*** | 0.048*** | | |
| | (0.005) | (0.006) | | |
| Initial NLW gap (25+) | | | 0.518*** | 0.468*** |
| | | | (0.145) | (0.154) |
| Observations | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.036 | | | |
| F-stat ($\beta_{1,t} = 0$): | 92.22 | 68.44 | 12.79 | 9.26 |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{1,t}$ from model (3.2), using the change in log average wage for workers aged 25 and over as outcome and measures of the NLW bite among workers aged 25 and over as main regressor. The sample is a balanced panel of domiciliary care agencies active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C10: EMPLOYMENT SPILLOVER EQUATIONS IN THE DOMICILIARY CARE SECTOR

Change in share of employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.002 | -0.001 | | | | |
| | (0.003) | (0.003) | | | | |
| Initial NLW gap (25+) | | | -0.013 | -0.006 | | |
| | | | (0.036) | (0.037) | | |
| Change in log average wage (25+) | | | | | -0.041 | -0.041 |
| | | | | | (0.128) | (0.241) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.000 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.003 | -0.001 | | | | |
| | (0.004) | (0.005) | | | | |
| Initial NLW gap (25+) | | | 0.006 | 0.025 | | |
| | | | (0.054) | (0.055) | | |
| Change in log average wage (25+) | | | | | -0.033 | 0.111 |
| | | | | | (0.135) | (0.260) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.003 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.006 | -0.001 | | | | |
| | (0.005) | (0.006) | | | | |
| Initial NLW gap (25+) | | | 0.005 | 0.055 | | |
| | | | (0.059) | (0.058) | | |
| Change in log average wage (25+) | | | | | -0.022 | 0.118 |
| | | | | | (0.120) | (0.129) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.011 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of employees aged under 25 as outcome variable. The sample is a balanced panel of domiciliary care agencies active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies. **Source:** NMDS-SC.

TABLE 3.C11: TOTAL HOURS SPILLOVER EQUATIONS IN THE DOMICILIARY CARE SECTOR

Change in share of total weekly hours worked by employees aged under 25

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | 0.001 | 0.004 | | | | |
| | (0.006) | (0.006) | | | | |
| Initial NLW gap (25+) | | | 0.024 | 0.056 | | |
| | | | (0.077) | (0.076) | | |
| Change in log average wage (25+) | | | | | 0.174 | 0.373 |
| | | | | | (0.247) | (0.575) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.003 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.004 | 0.001 | | | | |
| | (0.008) | (0.009) | | | | |
| Initial NLW gap (25+) | | | 0.056 | 0.118 | | |
| | | | (0.115) | (0.111) | | |
| Change in log average wage (25+) | | | | | 0.036 | 0.521 |
| | | | | | (0.251) | (0.606) |
| Observations | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 | 2,866 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.005 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion (25+) | -0.012 | -0.001 | | | | |
| | (0.009) | (0.011) | | | | |
| Initial NLW gap (25+) | | | -0.070 | 0.046 | | |
| | | | (0.129) | (0.120) | | |
| Change in log average wage (25+) | | | | | -0.011 | 0.099 |
| | | | | | (0.221) | (0.274) |
| Observations | 847 | 847 | 847 | 847 | 847 | 847 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.012 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{4,t}$ from model (3.5) in columns (1)-(4), and the estimated IV coefficient $\beta_{5,t}$ from model (3.6) in columns (5)-(6), using the change in the share of total weekly hours worked by employees aged under 25 as outcome variable. The sample is a balanced panel of domiciliary care agencies active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C12: RESPONDENTS' VIEWS ABOUT THE LEVEL OF THE NLW

| | Pre-NLW | | Post-NLW | |
|---|---|---|---|---|
| | Low-paid proportion *below* median | Low-paid proportion *above* median | Low-paid proportion *below* median | Low-paid proportion *above* median |
| Level of NLW is | | | | |
| About right | 47.15% | 41.18% | 58.20% | 55.08% |
| Too low | 17.07% | 15.97% | 13.11% | 26.27% |
| Too high | 26.83% | 42.86% | 23.77% | 18.64% |
| Don't know | 8.94% | 0.00% | 4.92% | 0.00% |
| Number of respondents | 123 | 119 | 123 | 119 |

**Notes:** The data are from a survey of all CQC regulated English care homes that we ran before and after the NLW introduction. We obtained information on all active care homes in England from the CQC registry and sent questionnaires to all homes in January and February 2016 for the pre-NLW part of the survey, and in late June, August and November 2016 for the post-NLW part of the survey. Responses were provided by the owner manager of the care homes. We obtained a total of 1390 responses in the pre-NLW survey and of 827 responses in the post-NLW survey, of which 248 responded to both surveys. In the pre-NLW survey we asked: "Do you think that the proposed level of the NLW is: (i) about right, (ii) too high, (iii) too low, (iv) don't know?". In the post-NLW survey we asked: "Do you think that the current level of the NLW is: (i) about right, (ii) too high, (iii) too low, (iv) don't know?". The table reports respondents' answers to these questions in the pre- and post-NLW waves for the balanced panel, separately for firms with a pre-NLW proportion of low-paid workers below and above the median.
**Source:** LSE-CEP Survey of Care Homes.

TABLE 3.C13: PRODUCTIVITY

Change in log residents per worker hour

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.011 | 0.008 | | | | |
| | (0.008) | (0.009) | | | | |
| Initial NLW gap | | | 0.043 | 0.024 | | |
| | | | (0.088) | (0.097) | | |
| Change in log average wage | | | | | 0.390 | 0.215 |
| | | | | | (0.435) | (0.869) |
| Observations | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.003 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.008 | 0.007 | | | | |
| | (0.010) | (0.012) | | | | |
| Initial NLW gap | | | -0.010 | -0.016 | | |
| | | | (0.113) | (0.126) | | |
| Change in log average wage | | | | | 0.186 | -0.064 |
| | | | | | (0.325) | (0.507) |
| Observations | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 | 4,083 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | -0.008 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), and the estimated IV coefficient $\beta_{3,t}$ from model (3.4) in columns (5)-(6), using the change in log residents per worker hour as outcome variable. The sample is a balanced panel of care homes active between March 2016 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C14: CLOSURES

Indicator for firm closure

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.001 | 0.002 | | |
| | (0.002) | (0.002) | | |
| Initial NLW gap | | | 0.020 | 0.022 |
| | | | (0.020) | (0.024) |
| Observations | 4,306 | 4,306 | 4,306 | 4,306 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.001 | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.001 | 0.003 | | |
| | (0.004) | (0.005) | | |
| Initial NLW gap | | | 0.037 | 0.051 |
| | | | (0.037) | (0.043) |
| Observations | 4,306 | 4,306 | 4,306 | 4,306 |
| Controls | No | Yes | No | Yes |
| Mean of dep. var.: | 0.006 | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_{2,t}$ from model (3.3) in columns (1)-(4), using the probability of closure as of June 2016 and September 2016 as outcome variable. The sample is a balanced panel of care homes active between March 2016, unconditional on their survival until March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C15: FIRM ENTRIES AT LOCAL AUTHORITY LEVEL

Probability of firm entry

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.004 | 0.003 | | | | |
| | (0.004) | (0.010) | | | | |
| Initial NLW gap | | | 0.008 | -0.006 | | |
| | | | (0.054) | (0.115) | | |
| Change in log average wage | | | | | 0.095 | -0.033 |
| | | | | | (0.374) | (0.616) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.005 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.004 | 0.002 | | | | |
| | (0.004) | (0.010) | | | | |
| Initial NLW gap | | | 0.008 | -0.006 | | |
| | | | (0.054) | (0.115) | | |
| Change in log average wage | | | | | 0.055 | -0.019 |
| | | | | | (0.235) | (0.337) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.005 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.006 | 0.003 | | | | |
| | (0.005) | (0.011) | | | | |
| Initial NLW gap | | | 0.029 | -0.009 | | |
| | | | (0.058) | (0.119) | | |
| Change in log average wage | | | | | 0.065 | -0.017 |
| | | | | | (0.199) | (0.232) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.006 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_6$ from model (3.A2) in columns (1)-(4) and the corresponding IV coefficient in columns (5) and (6), using the probability of firm entry as outcome variable. The sample is a balanced panel of local authority districts. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

TABLE 3.C16: FIRM EXITS AT LOCAL AUTHORITY LEVEL

Probability of firm exit

*Panel A - March 2016 to June 2016*

| | (1) | (2) | (3) | (4) | IV Low-pay (5) | IV NLW gap (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.004 | 0.006 | | | | |
| | (0.003) | (0.004) | | | | |
| Initial NLW gap | | | 0.029 | 0.055 | | |
| | | | (0.022) | (0.037) | | |
| Change in log average wage | | | | | 0.225 | 0.294 |
| | | | | | (0.153) | (0.224) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.005 | | | | | |

*Panel B - March 2016 to September 2016*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.001 | -0.004 | | | | |
| | (0.007) | (0.009) | | | | |
| Initial NLW gap | | | 0.002 | -0.008 | | |
| | | | (0.061) | (0.078) | | |
| Change in log average wage | | | | | -0.106 | -0.023 |
| | | | | | (0.220) | (0.229) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.006 | | | | | |

*Panel C - March 2016 to March 2017*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid proportion | 0.003 | 0.004 | | | | |
| | (0.014) | (0.018) | | | | |
| Initial NLW gap | | | -0.030 | -0.036 | | |
| | | | (0.123) | (0.162) | | |
| Change in log average wage | | | | | 0.073 | -0.070 |
| | | | | | (0.338) | (0.319) |
| Observations | 321 | 321 | 321 | 321 | 321 | 321 |
| Controls | No | Yes | No | Yes | Yes | Yes |
| Mean of dep. var.: | 0.006 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\beta_6$ from model (3.A2) in columns (1)-(4) and the corresponding IV coefficient in columns (5) and (6), using the probability of firm exit as outcome variable. The sample is a balanced panel of local authority districts. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and regional dummies.
**Source:** NMDS-SC.

# Chapter 4

# Zero Hours Contracts and Labor Market Policy

## 4.1 Introduction

Contemporary labor markets feature the use of "atypical" work arrangements. Some of these – like self-employment and agency work – have emerged in their current format as an evolution of previous work structures. Others – like short hours and zero hours contracts – reflect more the work demands of the modern age, with their introduction driven by technical and social change.[1] The increased incidence of this kind of work has led to discussions of there being a trade-off between additional flexibility and the emergence of low wage, dead end jobs, which function outside the job legislation offered in conventional forms of employment.

From a research perspective, it is important to try to determine which side of this trade-off dominates, and if it differs by work arrangement. In this paper, we consider the case of the UK labor market where the rise of atypical work has been a key feature of the post-financial crisis period. The focus is placed specifically on one kind of alternative work arrangement that has increasingly entered the UK setting, namely zero hours contracts (ZHCs). Almost a million people are on ZHCs at the time of writing, out of a total workforce of 32 million. Many of these ZHC work positions are prominent in the low-wage sectors of employment. Their relevance to labor market policy that affects low wage levels is therefore high.

The principal focus of the paper is placed upon developing a better understanding of ZHCs and labor market policy. More specifically, in doing this, the paper has two main aims. The first is to empirically document the evolution and characterization of ZHCs in the UK setting. There are two parts to this, the first drawing on data from the Quarterly Labour Force Survey and the second on newly collected survey data on alternative work arrangements. Part of the latter survey is devoted to ZHCs, which are only limitedly surveyed and understood in existing survey data sources (Abraham and

[1] Workers on zero hours contracts agree to be available for work as and when required, with no guaranteed hours or times of work.

Amaya, 2018) and – consequently – in the literature, and the intention is to fill this gap with new evidence.

The second aim is to explore the extent to which labor market institutions have the scope to be, at least partly, responsible for the increased diffusion of flexible work arrangements, or – conversely – whether the latter are a consequence of factors that have little to do with labor market institutions and rigidities. In this paper, a particular policy focus is placed on minimum wages, where we are interested in understanding whether higher minimum wages have potential to induce a larger utilization of alternative work arrangements by firms and, consequently, a shift in the composition of their workforce towards more flexible, but also insecure jobs.

In Europe, the rise of alternative work arrangements and gig-economy jobs is often considered an expression of the duality of the labor market, whereby the existence of rigidities in the "primary" market creates the conditions for an expansion of more flexible contractual relationships in the "secondary" market. Alternative work arrangements have also grown in the US, where labor markets are overall less rigid than in Europe, but minimum wages are an important component of labor market policies. By providing direct evidence on the role – or lack thereof – of minimum wage policies on the incidence of flexible work arrangements, this paper contributes to understanding a policy question relevant to both the US and European labor markets.

In the first part of the paper, survey-based evidence is presented to show that ZHCs are a key contract type in some, predominantly low wage, sectors of the UK labor market. They are characterized by the flexibility/dead end jobs trade-off already introduced above. They also feature, in different guises or by different names, in other countries' employment structures. The second part of the paper analyzes minimum wage policy and ZHC utilization by exploiting a substantial increase in the minimum wage rate for workers aged 25 and over that took place in the UK in April 2016, when a new minimum wage rate – the National Living Wage (NLW) – was introduced (Bell and Machin, 2018; Giupponi and Machin, 2018). In the UK setting, ZHC usage by employers does seem to have been affected by changes in labor market policy, as the sizable hike of the minimum wage that occurred when the NLW was introduced did shift more workers onto ZHC positions in the adult social care sector (and in low wage sectors more generally). To our knowledge, this is the first study connecting minimum wage changes to employer use of different types of job contracts.

The rest of the paper is structured as follows. In Section 4.2, a description of the atypical work arrangement under study, ZHCs, is given, together with a discussion of the extent to which other countries have similar job contracts. In Section 4.3, the relevant literature to the subject matter of the paper is discussed. Section 4.4 reports the analysis that documents the patterns of ZHC coverage in the UK labor market. Section 4.5 describes the evidence on minimum wages and ZHC jobs. Section 4.6 concludes.

## 4.2 Atypical Work Arrangements: Zero Hours Contracts

### 4.2.1 Zero Hours Contracts in the UK

ZHCs are an employment contract under which a worker is not guaranteed any hours and is only paid for work carried out. It can be viewed as a form of on-call working, as workers can be offered hours at short notice, as and when an employer needs them. Workers are not obliged to accept work that has been offered to them and, similarly, employers are not obliged to offer any work.[2] Thus, ZHCs offer flexibility to both the employer and the employee, and, as a result, some workers may prefer them to typical fixed hour employment contracts. Conversely, due to the lack of security and guaranteed income, they are unlikely to be suitable for many workers. Such contracts have become prevalent in particular industries such as retail, health, and hospitality.

ZHCs have, in theory, always been possible to be used by employers in the UK and have no specific legal status, rather being an informal term to refer to a type of contract. Their use has seen an increase over the past decade. Estimates from the Office of National Statistics (ONS) suggest that in 2008 143,000 employees were on ZHCs whereas by 2017 this figure was 883,000. Until 1998, ZHCs were often used to "clock off" workers during quiet periods nonetheless expecting them to stay on site, though this exploitative practice was ended in 1998 with the passing of the Working Time Regulations.

Table 4.1 presents a breakdown of various legislation coverage for different forms of employment relation in the UK setting. While ZHC workers are covered by some employment legislation, such as minimum wage coverage and holiday pay, legal complications have arisen due to the nature of the contract. One key area of contention has been whether a worker is also considered an "employee", which would in turn grant them additional rights, such as unfair dismissal protection (Adams, Freedman, and Prassl, 2018).[3] While the contract itself would not classify workers as employees, case law in the UK to date has concentrated more on whether there is a pattern of regular work being accepted, and if so the employee classification would be granted (Pyper and McGuinness, 2018).

ZHCs have received a fair amount of attention both in the UK media and from the UK Parliament. The Conservative-Liberal Democrat coalition government that was in power from 2010 to 2015 launched a review of the use of ZHCs in 2013. This raised four main areas of concern – exclusivity clauses, transparency of contracts offered to workers, uncertainty of earnings and an imbalance in the employment relationship. Up to now, the only area which has been legislatively addressed is that concerning exclusivity clauses, i.e. clauses that prevented workers on ZHCs from working for more than one employer. As of March 2015, the *Small Business, Enterprise and Employment Act 2015* came into force and effectively banned exclusivity clauses on ZHCs.

[2] It is questionable however, whether all ZHC roles afford workers this ability in practice (Wakeling, 2014).

[3] Workers are still afforded a number of core employment rights, unlike for example, those gig economy workers who are officially self-employed and thus are not covered.

### 4.2.2 Zero Hours-Like Contracts: the International Setting

As stated above there is no legal definition in the UK for ZHCs, and thus international comparisons rely on assessing qualitative similarities. This can often be problematic due to the differences in terminology, legal status and governance. Similar atypical working arrangements however do exist and there is varied diffusion across Europe and other developed economies, though they often operate under different names, and levels of regulation. Caution should nonetheless be taken when drawing parallels as the welfare implications of such arrangements will also rely on factors such as union coverage and domestic economic performance.

Probably the largest proportion of such atypical contracts exists in Australia, where "casual employment" contracts are a legal classification and approximately 25 percent of employees are on such contracts. Around half of workers on these contracts receive variable earnings from one period to the next, and around a third would like more hours (Gilfillan, 2018). Australia is however an outlier in this case, since most developed economies where zero hours-like contracts are used generally have usage rates in the same region as the UK. In Canada 3.2 percent of employment is in "casual employment" and in the US approximately 2.6 percent is "on-call". In Europe, Finland reported 4 percent of employees on ZHCs and Norway 0.8 percent; in Netherlands 6.4 percent are "on-call", and the Irish Quarterly National Household Survey reports that approximately 5.3 percent of Irish employees have constant variation in their working hours.[4] Given the varied definition and sometimes lack of a legal classification, equivalent statistics do not necessarily exist for all countries where there is diffusion.

The attention these types of contracts have received in the media and political sphere are not unique to the UK. Following union pressure, New Zealand passed regulation in 2016 which stipulated that firms needed to outline a minimum number of guaranteed hours each week and employee refusal of hours beyond that should not result in any detriment to the worker. Furthermore, it introduced the requirement of compensation to the worker if shifts were cancelled at short notice. In Finland, a citizen's initiative gathered 50,000 supporters to ban ZHCs, and though it was rejected by the parliament, a number of proposals have been made in order to regulate such employment relationships. The most recent looks to ensure that employers present a valid reason (relating to demand fluctuations) as to why they require to use a ZHC. Extensive regulation was introduced in 2012 and 2013 to "on-call" work in Italy and has severely restricted the use of zero hours-like contracts to only older and younger age groups, and in 2014 further regulation was introduced in both the Netherlands and France.

Table 4.2 presents a comparative set of descriptions and associated regulations for zero hours-like contracts in Western Europe (where they are present) and for the US. Western Europe generally experiences significant regulation of zero hours-like contracts.

[4]Figures for the Netherlands are from 2016; for Finland, Ireland and the US from 2015, and for Norway from 2010.

For example, while proliferation in the EU is largest in the Netherlands, workers there enjoy regulations that ensure a minimum number of hours of work whenever they are called to work, as well as agreed hour adjustments based on the previous three months of work. Conversely, unlike the UK, employees must work when called upon. Such idiosyncrasies exemplify how outwardly similar contractual agreements may have very different implications when in action. What is evident, however, is that the UK, Sweden and the US (aside from some specific cities) appear to have the least regulation of zero hours-like contracts. Union density in Sweden is high (around 70 percent), but in both the UK and the US rates are much lower (23.2 percent and 10.7 percent respectively). Thus, proliferation of zero hours (-like) contracts in the UK and the US, where workers' real wage growth has been weak, are likely to have the most significant welfare implications.

## 4.3 Related Literature

### 4.3.1 Atypical Work Arrangements

Employment relationships such as ZHCs, diverging from the standard full-time, permanent, regular and single employer set-up have been characterized as "atypical" (Eurofund, 2017) and such working arrangements have seen a large amount of growth in the past two decades in a number of developed economies (Eichhorst and Tobsch, 2013; Gielen and Schils, 2014; LSE Growth Commission, 2017; Katz and Krueger, 2019a). The concept of "atypical" work arrangements has always been somewhat nebulous, but spans a variety of working practices including part-time, agency, contract, fixed-term, contingent and independent contracting. Studies have demonstrated the large heterogeneity across these types of employment relationships, though part time and temporary work fare relatively badly in terms of wages when compared to their standard counterpart (Kalleberg, 2000).

ZHCs most closely match the definition for contingent work, and early literature suggested that atypical working arrangements, especially in the form of temporary or contingent work, offered workers lower wages, fewer benefits, less security and little scope for human capital development (Rodgers and Rodgers, 1989; Beard and Edwards, 1995; Nollen, 1996; Kalleberg, 2000).[5] Conversely, however, more recent (albeit weak) evidence has suggested that atypical work may serve as a stepping stone to more stable employment in the long run, when faced between an option of continued job search and atypical employment (Addison and Surfield, 2008).

The past few years have seen a growth in the interest in atypical or "alternative" work arrangements with a small portion of the literature presenting descriptive evidence as well as trying to understand the mechanisms driving the shift to such types of work.

[5] Contingent work is defined as "any job in which an individual does not have an explicit or implicit contract for long-term employment or one in which the minimum hours worked can vary in a non-systematic manner" (Polivka and Nardone, 1989).

Factors that have been suggested to be contributors include weak demand conditions, worker's preferences and technological change; where the latter may work by reducing transaction costs. Since transaction costs – such as search, monitoring and enforcement costs – are, according to Coase (1937), factors that lead to the creation of the firm, it is likely that technological change would lead to a blurring of the boundaries of the firm.

Katz and Krueger (2019b) found that, over the ten year period between 2005 and 2015, the proportion of workers engaged in some form of alternative work arrangement grew by 10-20 percent in the United States, while analysis of the UK labor market has shown a growth in both the prevalence of ZHCs as well as individuals described as "self-employed with no employees" (LSE Growth Commission, 2017).

Katz and Krueger (2017) report US findings that individuals who suffer periods of unemployment are 7-17 percent more likely to be employed in alternative work arrangements 1 to 2.5 years later than their observational counterparts who did not experience such unemployment. These results suggest that at least one factor that could be driving the supply side of the atypical labor market is a weakening of market power for workers. Additionally, Mas and Pallais (2017) use a discrete choice experiment to elicit willingness to pay for alternative work arrangements for call center workers and find that the average worker is willing to give up a fifth of their wages to avoid an employer dictated work schedule. This gives further evidence that low paid workers finding themselves in contingent work arrangements are likely to be engaged in such work out of necessity rather than choice.

To our knowledge there is little recent research concerning the factors driving labor demand for contingent work arrangements. There are obvious benefits to employers, in particular the ability to reduce wage liabilities and cope with seasonal and weekly fluctuating demand conditions. Dube et al. (forthcoming) present evidence demonstrating significant monopsony power on an online labor market platform, though it should be noted such self-employed "HIT" work does have some key differences to more traditional sectors, which generally offer more on-going work.

### 4.3.2 Minimum Wages

Over its long existence as a key research area in labor economics, the minimum wage literature has evolved along three main lines of research. The primary and most traditional focus has been on the employment and unemployment effects of minimum wages, which have proven elusive to detect in many cases. Early studies based mostly on US time-series work found negative employment effects among teenagers (Brown, Gilroy, and Kohen, 1982). However, apart from those, the vast majority of quasi-experimental micro-based work that started in the early 1990s in the US and the UK (Card and Levine, 1994; Machin, Manning, and Rahman, 2003; Stewart, 2004; Giupponi and Machin, 2018), and of more recent analyses based on spatial identification in the US find hardly any evidence of disemployment effects of minimum wages (Dube, Lester, and Reich, 2010; Baskaya and Rubinstein, 2015; Dube, Lester, and Reich, 2016; Clemens and Wither, 2019).[6]

Partly in response to this fairly widespread inability to find evidence of disemployment effects, a second strand of research has investigated other margins through which firms can adjust to the wage cost shock induced by the minimum wage increase. Examples of such margins of adjustment are prices (Aaronson, 2001; MaCurdy, 2015; Harasztosi and Lindner, forthcoming), profits (Draca, Machin, and Van Reenen, 2011), firm value (Bell and Machin, 2018) and the quality of services provided (Giupponi and Machin, 2018). A third body of the literature has looked at the impact on wage inequality at the bottom of the distribution, and at wage spillover effects up the wage distribution and onto legally unaffected workers (DiNardo, Fortin, and Lemieux, 1996; Lee, 1999; Autor et al., 2016; Giupponi and Machin, 2018).

To the best of our knowledge, this is the first paper examining the impact of a minimum wage change on contractual arrangements. We thus contribute to the existing literature by assessing the impact of minimum wages on workers' employment conditions (other than pay) and on the utilization of flexible contractual forms by firms that can act as buffers against the wage cost shock. We do this by exploiting the introduction of the National Living Wage (NLW) in the UK in April 2016. The NLW is the mandated minimum wage rate for workers aged 25 and over; it was set at £7.20 an hour from April 2016 to March 2017, then uprated to £7.50 in April 2017.[7] As demonstrated by Figure 4.A1 in Appendix 4.A, while the UK has had various national minimum wages (NMW) in place since 1999, the NLW introduction represented a substantial (7.5 percent) increase in the wage floor for those aged 25 and over.

## 4.4 Survey Evidence of Zero Hours Contracts

### 4.4.1 ZHCs in the Labour Force Survey

The Labour Force Survey (LFS) is a quarterly cross-sectional survey of the UK labor market. Each quarter contains data on approximately 35,000 employees, some of whom could be on a ZHC. Questions relating to flexible work arrangements are asked only in quarters April-June and October-December therefore in each year it is only these two quarters that are analyzed.

Table 4.3 presents summary statistics for both all employees and ZHC employees for 2017. Of all workers in 2017, around 2.7 percent are recorded as being on ZHCs. ZHC workers are on average more likely to be younger, female, and still in full time education, though still a large proportion (over 80 percent) have completed their full-time education.

[6] In a rather different context of union bargained minima, Kreiner, Reck, and Skov (2017) study the effect of a change in the youth minimum wage in Denmark and find an employment elasticity to the wage rate of -0.8.

[7] Further details on UK minimum wage policies and the National Living Wage will be provided in Section 4.5.

It is unsurprising that female workers experience a higher incidence of ZHCs given they are more prevalent amongst part-time employees. Typically, ZHC workers have lower tenure, though it is unclear whether this is due to higher ZHC worker turnover rates or if longer tenured ZHC workers are more likely to be placed on more secure contracts. The mean hourly wage for ZHC workers is around £5 lower than the equivalent for all workers, and they work on average 10 hours less per week than the average employee. Interestingly, the median hourly wage for ZHC workers is very close to the 2017 NLW of £7.50 per hour, within approximately 5 percent.

Figure 4.1 and Table 4.4 exemplify the importance of the NLW for ZHC workers. Figure 4.1 shows there to be a very sizable spike in the wage distribution for ZHC workers at the 2017 NLW of £7.50 an hour. Table 4.4 shows that, while the NLW is important for a significant proportion of all employees, with around 6 percent paid exactly the NLW and 20 percent likely to be affected by the subsequent minimum wage uprating, the 2016 and 2017 upratings affected a lot more – around half – of all ZHC workers. This latter figure could increase when one considers the possibility of wage spillover effects up the distribution.[8] While the NLW is age specific and mandatory only for those aged 25 and over, there is strong evidence that there are spillovers for workers aged under 25 (Giupponi and Machin, 2018). Indeed, one can see that the proportion paid exactly the NLW is identical for all employees and for those aged 25 and over. This identity is lost, but only marginally, when considering ZHC workers.

The LFS also has a panel version of the survey, albeit with a much smaller sample size. We use this to produce transition Tables 4.5 and 4.6, which detail flows into/out of ZHC positions from/to different types of economic activity. As can be seen by the diagonals in both tables, ZHCs have the lowest persistence of all working arrangements presented. Over the period analyzed (2015-2018) just over a third of ZHC workers remained in ZHC positions after five quarters and, of ZHC workers, only a quarter were ZHC workers five quarters before. ZHC workers are most likely to transition from and to other forms of part time employment, full time employment and inactivity.

These patterns of work dynamics act to confirm the somewhat precarious nature of ZHCs as a form of employment. One issue that emerges is whether workers who move from ZHCs into more secure working arrangements (part-time and full-time employment) do so by changing employer, or if after a period of time their employer offers a more secure contract. Equally, there is the question of whether those in "regular" work get reclassified by employers onto ZHCs. Sample size issues preclude any systematic and robust probing of this question with the data we have available, but when we investigated the interaction between job changes and changes in ZHC status for non-job changers, we found there to be a roughly half and half mixture of job moves and reclassifications. Clearly both are happening, but this remains suggestive as reaching a firmer

[8] For evidence on the existence (or lack thereof) of spillover effects in the UK see Stewart (2012), Low Pay Commission (2009) and Butcher, Dickens, and Manning (2012).

conclusion would require more detailed and larger sample size longitudinal data than we are currently able to study.

### 4.4.2 ZHCs in the LSE-CEP Survey of Alternative Work Arrangements

In order to better understand the role of alternative work arrangements in the UK, between February $5^{th}$ and March $2^{nd}$ 2018, we ran the "LSE-CEP Survey of Alternative Work Arrangements" using an online platform. While the survey was designed to be representative of the UK population aged 18-65, its main goal was to collect information on both the types of jobs and characteristics of workers involved in alternative work arrangements. The survey questionnaire is reproduced in Appendix 4.C. The survey questioned approximately 20,000 individuals, of which just fewer than 19,000 remained in the cleaned sample.[9]

Table 4.B1 in Appendix 4.B presents descriptive statistics for the sample of respondents of the LSE-CEP survey. The survey is equally represented across sex and the age distribution, with a slightly lower participation rate for the ends (18-24 and 55-65) of the surveyed age distribution. Additionally, there is a healthy mixture of qualification attainment as well as regional representativeness across the UK. Around half of our sample are employed by a private company, a further quarter are employed by either a non-profit or government and the remainder are split between some form of self-employment or not working. Sample attrition during cleaning does not appear to fundamentally change any of these statistics.

Table 4.7 presents descriptive statistics for ZHC workers, for the cleaned sample. ZHCs are spread roughly equally across the sexes of respondents, which is marginally different to the LFS proportion shown earlier in Table 4.3. ZHC workers in our survey are on average younger than the average worker, though surprisingly share a similar distribution of educational qualifications as all workers in the survey. One may have assumed that workers experiencing more insecure employment contracts would be those with lower skill sets and thus market power, however these summary statistics suggest otherwise. On the whole, a region's share of ZHC workers is roughly the same as their share of workers overall. However, London appears to be anomalous in that its share of ZHC workers is about four fifths higher than its share of workers. Interestingly, a large proportion of ZHC workers (42 percent in the cleaned sample) hold multiple jobs, and around a third hold a job with a more secure contract. This is suggestive that ZHC jobs may act as a form of "top up" income for some workers, and additionally some ZHC workers may hold multiple ZHC jobs as a form of insurance due to the possibility of lack of hours.

Hourly wages for ZHC workers in our survey are paid an average of £11.63 per hour; this is slightly higher than the same figure produced by the LFS for ZHC workers (£9.77).

[9]Respondents were excluded from the cleaned sample if they responded with gibberish to any open questions and/or did not answer the attention questions correctly.

Figure 4.2 presents the hourly wage distribution for ZHC workers in our survey. It can be seen that the modal hourly rate is £8 and that there is a large proportion of individuals paid around the region of the NLW rate of £7.50. Thus, it is likely to be the thicker right tail that is driving up the mean wage in the CEP survey compared to the LFS, rather than the entire distribution being centered higher.

The average number of hours worked is low (around 19 per week) and similar to the figure found in the LFS. This further concretes the fact that many ZHC workers are working less than full time. Figure 4.3 presents the weekly hours distribution. There is a large spread of the hours performed, with almost 10 percent of workers not doing any hours the previous week, which may well be reflective of the insecurity related to some ZHC jobs. There does appear to be a selection of workers performing full-time (or above full-time) hours, whether these hours are regular is however unclear.

What is striking is that around one third of ZHC workers do unpaid work each week, averaging at 7 hours per week. This would imply the average worker is losing out on approximately £80 per week. Such losses may be particularly important for social care workers (who we study in more detail below). As discussed in Rubery, Grimshaw, and Hebson (2014) domiciliary carers for example only get paid for face to face time, and time spent driving between clients may result in what they call a "fragmented time contract". Almost two thirds of ZHC workers have been working for over five years. Conversely, over half of those sampled have less than one year experience on a ZHC, suggesting that an abundance of those on ZHCs have previously held non-ZHC working arrangements.

There are a few industries which stand out as having a large share of workers on ZHCs. In particular, retail, education, accommodation and food services, and health and social work. For retail and accommodation and food services this is unsurprising, as these professions are characterized by having a larger proportion of workers on part-time contracts and may be subject to seasonal fluctuations. The health and social work sector has the highest proportion of ZHC workers (15 percent). The social care sector, which falls under this heading, has not only a large number of low paid staff, but also faces an informal price cap for its output good, as a large proportion of those receiving social care are council funded. It is thus a perfect sector to analyze to assess whether firms facing growing wage bills due to the NLW are likely to use ZHCs to reduce their wage liability.

### 4.4.3 LSE-CEP Survey Representativeness

Table 4.B2 in Appendix 4.B presents demographic variables (similar to those in Table 4.7 and Table 4.B1 in Appendix 4.B) for both all respondents and ZHC workers from the LFS, and can be used to check the representativeness of the CEP survey. In terms of overall representativeness, our survey fairs well with respect to age, qualifications and regional distribution. Our survey does however under sample those who did not have a job last week. Furthermore, the survey's representativeness of ZHC workers is generally good, however one can see that the mean hourly wage is just under £2 per hour higher

in our survey. The median wages however are more similar (the gap reduces to £0.64), which suggests that the LSE-CEP survey has a slightly fatter right-hand tail of the wage distribution as discussed in Section 4.4.2.

### 4.4.4 LSE-CEP Survey Results

In this subsection, we report a second set of results that emerged from the survey of employees on ZHCs, with a focus on workers preferences and employment conditions.

An important question is whether workers choose to be on ZHCs for the flexibility that they offer, or would instead like a job with a minimum number of guaranteed hours but could only find employment as ZHC workers. Our survey results suggest an almost even split between workers who are satisfied with their number of hours (40 percent) and workers who would rather work more hours (44 percent), while a remaining 16 percent would like to work fewer hours (Figure 4.4). Of those wanting to work more hours, when asked about the reason why there are unable to work more hours, 74 percent point to the lack of available work, followed by another 15 percent who are instead constrained by domestic commitments (Figure 4.5). As reported in Figure 4.6, domestic commitments are also the main reason brought about by people who would like to work fewer hours (38 percent), followed by the desire to spend more time on leisure and other unpaid activities (26 percent) or other types of work (14 percent), impediments due to illness or disability (10 percent) and study commitments (7 percent). In addition to the number of hours worked, the pattern of those hours may also be a relevant dimension of workers' satisfaction with their jobs. As with the desired number of hours, there appears to be an almost even split between respondents who would like to have a more regular pattern of hours (45 percent) and those who are satisfied with their current pattern of hours (43 percent), with the remaining 12 percent wanting a less regular schedule (Figure 4.7).

The survey responses regarding desired hours and work time patterns are suggestive of an almost even dichotomy between workers who are happy with the amount of work that they do, and workers who would like to work more but are unable to. We further investigate this issue by asking ZHC workers what are the reasons for their being on a ZHC (Figure 4.8). In line with our previous findings, the two main reasons that stand out are the inability to find employment in a job with a guaranteed number of hours (28 percent) and the flexibility to perform other activities (28 percent). Less prominent reasons are – in order of relevance – better remuneration than other available jobs (20 percent), complementing pay from other jobs (14 percent) and earning while studying (7 percent). Overall, 51 percent of respondents state that they are either satisfied or very satisfied with their ZHC job, 28 percent are neither satisfied nor dissatisfied, and the remaining 21 percent are dissatisfied or very dissatisfied (Figure 4.9).

Finally, we are interested in whether ZHC workers receive training and what type of training they would find most useful. According to our survey results, 55 percent of ZHC workers had received some form of training in the past year. As illustrated in column (1) of Table 4.8, the most common types of training are – in order of importance – safety training (56 percent), skills training (54 percent), quality training (30 percent), and professional and legal training (22 percent). Training was paid for by employers, contractors, customers or someone else in 72 percent of cases, by the respondent in 16 percent of cases and free for the remainder 12 percent (Table 4.9). We also asked all ZHC respondents what type of training they would find useful for their future job prospects (column (2) of Table 4.8): skills training stands out as 50 percent of respondents indicate is as useful, followed by safety training (27 percent) and other types of training (all deemed useful by approximately 23 percent of respondents). It therefore seems that, when offered, training meets individual requirements.

## 4.5 Zero Hours Contracts and Minimum Wages

### 4.5.1 Conceptual Framework

As documented in the previous sections, a large fraction of workers on ZHCs are paid the minimum wage. An interesting question that is relevant for policy is to assess whether labor market policies such as minimum wage upratings are responsible for the increased diffusion of ZHCs, or – conversely – the latter are a consequence of factors that have little to do with labor market institutions. In the first case we should see that a raise of the minimum wage increases the utilization of ZHCs. In second case, we should see no effect of the minimum wage on ZHC usage. The rationale for a causal effect of minimum wage policies on ZHC utilization is that ZHCs can help firms buffer the wage cost shock due to the minimum wage increase by allowing them not to commit to a minimum number of hours. At the same time, though, the burden of insecurity would be transferred from firms onto risk-averse employees, potentially worsening the employment conditions of individual workers.

In this section, we exploit a large minimum wage increase recently implemented in the UK – the National Living Wage introduction – to shed light on the causal effect of minimum wage policies on the incidence of ZHCs. We do so in the context of the English adult social care sector, which previous research has demonstrated to be highly vulnerable to minimum wage increases (Machin, Manning, and Rahman, 2003; Machin and Wilson, 2004; Giupponi and Machin, 2018) and which can therefore provide a good testing ground for the effects of minimum wage policies.

Whilst there is a sample selection issue of studying care workers, and associated questions of generalizability to the UK workforce more widely, looking at the adult social care sector allows us to have good quality data on hourly wages and contractual arrangements (which are necessary to answer well the question that we ask). Also, the fact that flexible work arrangements are already largely in use in this sector means that – if the NLW has an impact on ZHC utilization – this is a sector in which we can see it. Moreover, the estimates are relevant for other low-pay, ZHC-intense sectors, like hospitality and retail, which are those we care about the most when studying the economic effects of minimum wage floors.

### 4.5.2 The Introduction of the National Living Wage

The first UK national minimum wage policy dates back to April 1999, when the National Minimum Wage (NMW) was first introduced. At that time, a minimum hourly wage of £3.60 for workers aged 22 and over, and a lower rate of £3.00 for workers aged between 18 and 21 were established. Additional rates have been introduced in subsequent years, so that as of October 2015 the NMW rates were as follows: an adult minimum rate of £6.70 for workers aged 21 and over, a youth development rate of £5.30 for those aged 18-20, a youth minimum of £3.87 for 16-17 year olds and an apprentice rate of £3.30.

On July $8^{th}$ 2015, the newly elected Conservative Party government called an emergency budget, in which the Chancellor George Osborne announced the introduction of the National Living Wage (NLW). This unexpected intervention changed the structure of minimum wages by introducing a new minimum wage rate of £7.20 an hour for workers aged 25 or above starting from April $1^{st}$ 2016, while leaving the minimum wage rates for younger workers unchanged.[10] Five minimum wage rates are now in operation in the UK: the NLW for workers aged 25 and over, the NMW for 21-24 year olds, the youth development rate for 18-20 year olds, the young worker rate for 16 and 17 year old, and the apprentice minimum wage.[11]

The NLW introduction was an unexpected and radical policy intervention. Firstly, it came from a political party that had traditionally been hostile to minimum wages, especially at the time of the NMW introduction in April 1999. Secondly, the NLW introduction generated a wage change much larger than recent uprates, namely an increase of 10.8 percent at the time of announcement in July 2015 and of 7.5 percent at the time of implementation on April $1^{st}$ 2016. Most importantly for our analysis, the unexpected and sizable wage shock generated by the NLW introduction provides a unique "experiment" to study the consequences of the minimum wage increase and the wage cost shock it induced on employers' use of ZHCs.

### 4.5.3 The Adult Social Care Sector

The impact of the NLW introduction on ZHC utilization is studied in the context of workers and firms in the English adult social care sector. Specifically, we will consider adult social care providers operating in the residential care home industry and the domiciliary care industry. Residential care refers to the provision of accommodation and personal

[10] Additionally, the NLW was set to achieve 60 percent of median earnings by 2020, which – at the time of the announcement – was forecasted to be £9.00 by the UK Office for Budget Responsibility.

[11] See Giupponi and Machin (2018) for a comprehensive discussion of minimum wages in the UK and for an empirical analysis of the wage and employment consequences of this significant change in the structure of minimum wages.

care to adults in a communal residential center, which may or may not provide nursing facilities. Members of staff in residential care homes are predominantly care assistants, who provide 24 hour supervision, meals and help with personal care needs. Domiciliary care – also referred to as home care – is a social care service provided to people who live in their own houses and require assistance with personal care routines, household tasks such as cleaning and cooking, or any other activities they may need to live independently. Domiciliary care assistants typically work individually, and are often contracted on flexible working hours or ZHCs since domiciliary care work tends to be organized into short and fragmented home visits.

The choice of focusing on the adult social care sector is motivated by various reasons. Firstly, the sector is highly vulnerable to minimum wages changes, as it has many low-paid workers. Of these, the vast majority are older than 25, making the setting especially suited to analyzing the NLW introduction. Secondly, the sector is close to what can be considered a competitive labor market, as it consists of a large number of relatively small firms providing a rather homogeneous service, and it is very labor intensive and not unionized. Thirdly, residents' fees are regulated and paid for by local authorities, making it difficult for firms to pass higher costs onto prices. For all these reasons, a minimum wage change is likely to have a substantial impact on total costs and on economic outcomes of workers and firms in this sector, which therefore provides a useful testing ground for analyzing the impact of minimum wage policies. In other words, the high vulnerability to the minimum wage increases the likelihood of finding large effects from wage shocks. Finally, the incidence of ZHCs is high – particularly in the domiciliary care industry – making this setting especially suited to studying the impact of the NLW on ZHCs.

### 4.5.4 Data Sources

The main data source that is used to analyze the effect of the NLW introduction on ZHC utilization is the National Minimum Dataset for Social Care (NMDS-SC).[12] This is an online system administered by Skills for Care and funded by the UK Department of Health that collects information on the adult social care workforce in England. Social care providers can use NMDS-SC to record and manage information about their workers, such as payroll data, training and development, job roles, qualifications and basic demographics. By having an account and regularly updating it, providers are given access to a set of tools to visualize and analyze their data, submit applications for training and development funds, compare their employment and pay structure with those of other providers locally, regionally or nationally, access publications about the social care sector and other e-learning resources for free, and directly share their data and returns with authorities such as the Care Quality Commission and the NHS. No fee is charged to use

[12]NMDS-SC (2013); NMDS-SC (2014).

NMDS-SC. However, in order to benefit of certain facilities, providers must update their account at least once per year.

The dataset is a panel of matched employer-employee data. For each provider, we have information on the industry and main service provided, service capacity and utilization, number of staff employed, geographic location and system update dates. For workers, we have information on demographics (gender, age and nationality), job characteristics (job role, contract type and qualifications), contracted weekly hours, hourly pay and update date of the hourly pay rate. We have access to the snapshot of the NMDS-SC online system at monthly frequency from March 2015 to March 2017, each snapshot including all providers in the system at that date.

A second data source is the Care Quality Commission (CQC) registry.[13] The registry contains a complete record of all active English care providers regulated by CQC at monthly frequency. It provides information on the activity status of providers and therefore allows us to identify when homes shut down and when new homes enter into the market.

### 4.5.5 Sample Design

Around 22,000 providers are registered with NMDS-SC as of March 2016. Of these, approximately 10,000 are residential care homes with or without nursing, and 3,800 are domiciliary care agencies. We match the sample of residential care homes and domiciliary care agencies with the CQC registry of active locations from March 2015 to March 2017, from which we can obtain information on whether a firm is active or closed in a given month. Our sample comprises care homes that meet the following three criteria: *(i)* being active from March 2015 through to March 2017 according to the CQC registry, *(ii)* having a record on NMDS-SC for all those months and *(iii)* having updated their NMDS-SC account at least once after March 2016.[14] This selection leaves us with a balanced panel of 4,680 firms that are active in March 2016 and remain open until March 2017.[15]

[13] The CQC is the independent regulator of health and adult social care in England. It is responsible for setting standards of care and for monitoring, inspecting and rating adult social care providers, to make sure that they meet fundamental standards of quality and safety.

[14] In order to avoid introducing sample selection driven by unobservable worker and firm characteristics correlated with the timing and frequency of updating, we do not condition our sample on a specific update date and only require that a firm update its records once in the twelve months after April $1^{st}$ 2016. Approximately 90 percent of NMDS-SC users update within a year.

[15] In our sample we have a total of 3,599 care homes and 1,081 domiciliary care agencies. According to the 2017 report on the care home market of the Competition and Markets Authority (2017), there are approximately 9,500 care homes in England. This implies that our sample represents approximately 38 percent of the market for care homes. According to a 2016 report of the United Kingdom Home Care Association (2016), the total number of registered locations providing domiciliary care in England was 8,500 in March 2016. This implies that our sample represents approximately 13 percent of the market of domiciliary care agencies.

### 4.5.6 Descriptive Statistics

Table 4.10 reports descriptive statistics for all firms in the balanced sample, and for care homes and domiciliary care agencies separately, as of March 2016. The adult social care sector is characterized by relatively low hourly pay (£7.57 per hour on average) and a large fraction of workers are aged 25 and over (88 percent on average), which are indicative of a high vulnerability to minimum wage increases in general and to the NLW introduction in particular.

The statistics reported in Table 4.10 also show that the care home sector is characterized by medium-sized establishments employing on average 45 employees. Domiciliary care agencies have a larger pool of employees as compared to care homes (66 vs 39 employees on average), and a remarkably higher proportion of ZHC workers (38 vs 5 percent) that translates into lower average weekly hours (16 vs 29 hours). Moreover, the proportion of workers on other flexible work arrangements such as temporary, bank or agency contracts, is almost twice as large in the domiciliary care sector (14 vs 8 percent). These differences most likely stem from the very nature of domiciliary care work, which tends to be organized into short and fragmented home visits to customers, so that domiciliary care assistants are often contracted on flexible working hours.

Apart from substantial differences in the types of working arrangements, the two sectors have an almost identical gender and age composition and similar wage rates. The main occupation in both sectors is care assistant and only a very small share of the workforce holds a nursing qualification. All these characteristics confirm that the adult social care sector is a pertinent context to the studying of the effects of the NLW introduction on wages and contractual arrangements.

### 4.5.7 NMDS-SC Representativeness

We check the representativeness of the NMDS-SC data using data from the Labour Force Survey (LFS). Table 4.B3 in Appendix 4.B reports the mean and standard deviation for a set of individual-level characteristics for care workers in the LFS.[16] The table also reports the same characteristics for care workers at the firm level in NMDS-SC. Demographic variables relating to gender, age and region line up very closely. The hourly wage rate and number of weekly hours worked are slightly higher in the LFS data, while the proportion of workers on ZHC is slightly lower. The discrepancy in average weekly hours in LFS and NMDS-SC is most likely due to the fact that the variable in LFS refers to actual hours worked, while in NMDS-SC to contractual hours, which – for ZHC workers – are equal to zero and therefore pull down the mean. The larger fraction of workers on ZHCs in NMDS-SC may be due to the fact that, in this dataset, we cannot account for multiple job holders, which tend to be more frequent in ZHC jobs. All in all, the statistics appear to line up quite satisfactorily, mostly showing a consistent pattern across sources.

[16] We select employees with standard occupation classification (SOC2010) marked as "care workers" in the LFS. LFS data refer to 2015Q4 and 2016Q1. NMDS-SC data refer to March 2016.

### 4.5.8 Empirical Strategy

This section explores whether the minimum wage increase due to the NLW introduction had an impact on the share of workers on ZHCs. By tilting the composition of the workforce towards contracts without a guaranteed number of hours, employers can easily adjust employment at the intensive margin, either on top of or in substitution to adjustments along the extensive margin. Consistent with previous work (Giupponi and Machin, 2018), we will show that the NLW did not have a significant impact on employment, suggesting that any substitution toward contracts with flexible working arrangements is to be interpreted as an adjustment at the intensive margin.

The empirical strategy is based on a difference-in-differences methodology in which we exploit between-firm variation in the pre-NLW proportion of workers that would be affected by the minimum wage increase, in order to identify the effect of the minimum wage hike on ZHC utilization. The regression specification reads as follows:

$$\Delta^q Y_{j,t} = \alpha_{1,t} + \beta_{1,t} \cdot MIN_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_{1,t} + \xi_{j,t} \tag{4.1}$$

where $\Delta^q Y_{j,t}$ is the quarter-on-quarter change in the proportion of workers on a ZHC in firm $j$ between quarter $t$ and quarter $t-1$; $MIN_{j,Mar2016}$ is the proportion of low-paid workers in firm $j$ as of March 2016; $X$ is a vector of pre-NLW firm-level characteristics measured in March 2016, including the proportion of female workers, the average age, the proportion working as care assistants, the proportion with nursing qualification, the occupancy rate and a set of local authority districts fixed effects; $\xi$ is a disturbance term.[17, 18] The subscript $t$ indicates the quarter relative to March 2016, which is normalized to take value $t = 0$. The variable $MIN_{j,Mar2016}$ is constructed as the proportion of workers that in March 2016 were paid below the age-specific minimum wage rate that would be in place as of April 2016. In other words, the variable provides a measure of the NLW bite at firm level.

The coefficients $\beta_{1,t}$ for $t = -4, \ldots, 0$ are treatment leads and provide an easy way to test whether there is any correlation between ZHC utilization and the proportion of low-paid workers prior to the NLW introduction. In other words, the leads allow to test whether there were divergent trends in ZHC utilization between firms more and less exposed to the minimum wage increase before the policy change. This is equivalent to testing for the parallel trends assumption in a traditional difference-in-differences setting.

To document the evolution of the relationship between the low-paid proportion and ZHC growth in the post-reform quarters, we measure the outcome variable $\Delta^q Y_{j,t}$ as the

[17] Data on the gender and age composition, and on the occupancy rate is missing for some firms. Such missing information is controlled for via a set of dummy variables.

[18] There is a total of 325 local authority districts in our sample and of 326 local authority districts in England. They split England into 326 areas of local governance.

long difference between March 2016 and, respectively, June 2016, September 2016, December 2016 and March 2017. This is equivalent to estimating the cumulative effect of the reform over post-reform quarters, i.e. the sum $\sum_{t=1}^{k} \beta_{1,t}$ for $k = 1, \ldots, 4$.

The empirical strategy rests on the assumption that firms with the highest potential to be affected by the NLW introduction were indeed those that experienced larger wage growth in the quarters following the policy change, as a consequence of the NLW introduction. Firstly, we provide evidence that this is indeed the case. Secondly, we show that the between-firm correlation between the proportion of pre-NLW low-paid workers and wage growth is entirely due to the minimum wage change. To this end, we estimate a regression specification similar to model 4.1, using quarter-on-quarter wage growth as outcome variable. The regression specification reads as follows:

$$\Delta^q \ln W_{j,t} = \alpha_{2,t} + \beta_{2,t} \cdot MIN_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_{2,t} + \eta_{j,t} \tag{4.2}$$

where $\Delta^q \ln W_{j,t}$ is the quarter-on-quarter change in the logarithm of the average hourly wage in firm $j$ between quarter $t$ and quarter $t-1$; $MIN_{j,Mar2016}$ is the proportion of low-paid workers in firm $j$ in March 2016; $X$ is the set of above listed covariates and $\eta$ a disturbance term. Analogously to what discussed for model 4.1, the coefficients $\beta_{2,t}$ for $t = -4, \ldots, 0$ are treatment leads that allow to test the exogeneity of the minimum wage increase. For post-NLW quarters, we measure hourly wage growth ($\Delta^q \ln W_{j,t}$) between March 2016 and, respectively, June 2016, September 2016, December 2016 and March 2017.

### 4.5.9 Main Results

Figure 4.10 reports the coefficients $\beta_{2,t}$ for $t = -4, \ldots, 0$ and the cumulated sum $\sum_{t=1}^{k} \beta_{2,t}$ for $k = 1, \ldots, 4$, from estimating model 4.2 on the balanced panel of firms that are active throughout all months between March 2015 and March 2017. The dots indicate the estimated coefficients and the capped vertical bars report 95 percent confidence intervals based on robust standard errors. The specification allows for heterogeneity in the $\beta_{2,t}$ coefficients between care homes (hollow circles) and domiciliary care agencies (black circles) and includes the full set of controls. The results provide compelling evidence of the causal effect of the minimum wage change on hourly wage growth: whilst no systematic correlation between the low-paid proportion and quarter-on-quarter wage growth can be detected prior to the NLW introduction, a statistically significant correlation emerges from the first quarter following the minimum wage increase.

In order to ease the interpretation of the results, Table 4.11 reports the estimates of the cumulated sum $\sum_{t=1}^{k} \beta_{2,t}$ for $k = 4$. This is equivalent to estimating the following specification:

$$\Delta \ln W_{j,t} = \alpha_3 + \beta_3 \cdot MIN_{j,Mar2016} + X'_{j,Mar2016} \cdot \gamma_3 + \nu_{j,t} \tag{4.3}$$

where $\Delta \ln W_{j,t}$ is the change in the natural logarithm of the average hourly wage in firm $j$ between March 2016 and March 2017; all other variables are defined as above and $\nu$ is a disturbance term. The parameter $\beta_3$ captures the relationship between the proportion of low-paid workers and the average hourly wage growth in the 12 months after the NLW introduction.

The specifications in columns (1) and (3) of Table 4.11 report the estimated coefficient $\beta_3$ for the pooled sample of care homes and domiciliary care agencies, while those in columns (2) and (4) allow $\beta_3$ to vary across the two sectors. The regression models in columns (3) and (4) include the above-listed firm-level controls. In all cases there is significant evidence of larger wage increases in firms with more low-wage workers in the pre-NLW period, as measured by the March 2016 proportion of low-wage workers. According to the estimate in column (3), a one standard deviation increase in the proportion of low-paid workers (corresponding to a 34 percentage point change as reported in Table 4.10) implies a 1.9 percentage-point faster wage growth on a baseline of 4 percent, indicating a strong and significant relationship between our measure of the NLW bite ($MIN_{j,Mar2016}$) and wage growth after the policy change. According to the estimates in columns (2) and (4), there is no differential relationship between the initial proportion of low-paid workers and wage growth in the domiciliary care and care home sector.

We now consider whether the wage cost shock induced by the NLW introduction had consequences on ZHC utilization by firms. Figure 4.11 probes the relationship between the low-paid proportion in March 2016 and growth in ZHC utilization by reporting the coefficients $\beta_{1,t}$ for $t = -4, \ldots, 0$ and the cumulated sum $\sum_{t=1}^{k} \beta_{1,t}$ for $k = 1, \ldots, 4$, from estimating model 4.1 on the balanced panel of firms that are active throughout all months between March 2015 and March 2017. Similar to Figure 4.10, the dots indicate the estimated coefficients and the capped vertical bars report 95 percent confidence intervals based on robust standard errors. The specification allows for heterogeneity in the $\beta_{1,t}$ coefficients between care homes (hollow circles) and domiciliary care agencies (black circles) and includes the full set of controls. The graph shows no differential growth in ZHC utilization prior to the introduction of the NLW across firms more or less exposed to the minimum wage increase. After the policy change, a positive relationship between our measure of the NLW bite and ZHC utilization emerges in both sectors, with a larger effect size in the domiciliary care one. Starting from the second quarter after March 2016 coefficients are statistically significant and persistent over time. The overall dynamic of the effect gives strength to a causal interpretation of the impact of the minimum wage hike on ZHC utilization.

Table 4.12 reports the regression coefficient $\beta_3$ from estimating model 4.3 using the change in the share of ZHC workers between March 2016 and March 2017 ($\Delta Y_{j,t}$) as outcome variable. Estimates in columns (1) and (3) refer to the pooled sample of care homes and domiciliary care agencies, while those in columns (2) and (4) allow $\beta_3$ to vary across the two sectors. The regression models in columns (3) and (4) include firm-level controls.

The coefficient estimate reported in column (3) indicates that a one standard deviation increase in the proportion of low-paid workers is associated with a statistically significant 0.5 percentage-point faster growth in ZHC utilization. When $\beta_3$ is allowed to vary across care home and domiciliary care sectors (columns (2) and (4)), the effect increases by a factor of more than three in the domiciliary care sector. According to the results in column (4), a one standard deviation increase in the proportion of workers paid below the minimum is associated with a 0.4 percentage point larger increase in ZHC utilization from a baseline of 0.6 in the care home sector, and a 1.5 percentage point larger increase in ZHC utilization from a baseline of 6 percentage points in the domiciliary care sector. We take this evidence as suggestive of an increase in the share of contracts with no minimum guaranteed hours in response to the minimum wage increase, more so in a context – such as that of domiciliary care agencies – in which work tends to be organized into short and fragmented tasks.[19]

An interesting question to ask is whether the increased share of ZHCs is due to the conversion of previously non-ZHC positions into ZHC ones, the creation of new ZHC jobs or the displacement of workers on non-ZHC positions. For the first option to be true, we would need to observe no employment effects of the NLW introduction, for the second positive employment effects and for the third negative employment effects. We investigate this mechanism in Table 4.B4 in Appendix 4.B, where we report estimates of the coefficient $\beta_3$ of model 4.3, using the change in the logarithm of employment headcount between March 2016 and March 2017 as outcome variable. Our results do not point to significant employment effects twelve months after the NLW introduction, thus suggesting that new ZHC jobs replaced non-ZHC positions.

We also investigate whether the NLW introduction had an impact on the utilization of other flexible contractual arrangements: temporary contracts, bank work and temporary agency contracts.[20] Regression estimates of model 4.3 are reported in columns (1) to (4) of the various panels of Table 4.B5 in Appendix 4.B. For temporary contracts of all types, estimates are of limited magnitude and statistically insignificant.

### 4.5.10 Estimating the Effect of Wages on ZHC Utilization

The analysis illustrated in the previous subsection provides reduced-form evidence of the causal effect on the NLW introduction on the increased utilization of ZHCs. In this section, we are interested in estimating the effect of the wage cost shock induced by the

[19] It is worth noting that, relative to the baseline, the effect size is larger for more exposed care homes, though this is due entirely to the slower baseline growth rate.

[20] We report here the formal definitions of these three contractual arrangements, as defined by NMDS-SC. *Temporary contract*: the worker is employed for a limited duration, normally either on a fixed term contract or for a fixed task, or on a spell of casual or seasonal employment as a "temp". *Bank worker*: the worker is retained by the organisation as a whole, but deployed on a casual or short term basis. *Temporary agency work*: the worker is supplied by an outside employment agency/bureau; this category includes staff employed by NHS professionals, and workers supplied on contract e.g. by outside catering and cleaning companies.

NLW introduction on ZHC utilization, i.e. a parameter that can potentially be generalized to other policy-relevant settings.

The empirical strategy is based on the estimation of the following structural-form model:

$$\Delta Y_{j,t} = \alpha_4 + \beta_4 \cdot \Delta \ln W_{j,t} + X'_{j,Mar2016} \cdot \gamma_4 + \theta_{j,t} \tag{4.4}$$

where $\Delta Y_{j,t}$ is the change in the share of workers employed with a zero hours contract between March 2016 and March 2017; $\Delta \ln W_{j,t}$ is the change in the natural logarithm of the average wage in firm $j$ between March 2016 and March 2017; $X$ is a vector of above-listed pre-NLW firm-level characteristics and local authority districts fixed effects; $\theta$ is a disturbance term. The parameter $\beta_4$ measures the semi-elasticity of ZHC utilization to the wage rate.

Due to the potential endogeneity of $\Delta \ln W_{j,t}$, we estimate equation 4.4 via a two-stage least squares approach and instrument the change in the logarithm of the average wage $\Delta \ln W_{j,t}$ with $MIN_{j,Mar2016}$. Model 4.3 can therefore be considered as the first stage of the instrumental variable model. The estimates reported in Table 4.11 prove the relevance of $MIN_{j,Mar2016}$ as instrument for $\Delta \ln W_{j,t}$. Moreover, the patterns illustrated in Figures 4.10 and 4.11 combined provide compelling evidence in favour of the exogeneity of the instrument and of the exclusion restriction.

Estimates of the coefficient $\beta_4$ are reported in columns (5) and (6) of Table 4.12, where column (5) is based on the pooled sample, while column (6) allows the coefficient $\beta_4$ to vary between care homes and domiciliary care agencies. The estimate in column (5) points to a positive and significant wage semi-elasticity of 0.26, whereby a 4.1 percent increase in hourly wages (the average in the sample) leads to a 1.1 percentage point faster growth in ZHC utilization on a baseline of 1.9 percentage points. Once we allow the parameter to vary across the two industries, the effect becomes significantly larger in the domiciliary care sector, and smaller for the care home sector. According to the estimates in column (6), a 4.1 percent increase in wages (the average in the sample) leads to a 3.3 percentage point faster growth on a baseline of 6.1 percentage points in the domiciliary care sector. In the care home sector, a similar wage increase leads to a 0.9 percentage point faster growth in ZHC utilization, on a baseline of 0.6 percentage points.[21] Thus, it seems that one consequence of care sector employers paying higher wages to their staff is a raised likelihood of also placing them on a zero hours contract. This is especially true of domiciliary care employers.

### 4.5.11 Using LFS to Further Probe the Results for Low Paid Workers

Finally, we test whether a change in the proportion of ZHC utilization for care workers, and workers in other low paying industries, following the introduction of the NLW is also visible in the national statistics data. Figure 4.12 presents the evolution of the proportion

[21] Estimates of model 4.4 using the share of other forms of flexible contractual arrangements as outcome variable are reported in the various panels of Table 4.B5 in Appendix 4.B.

of care workers on ZHCs around the introduction of the NLW using data from the LFS, for the period from 2014 to 2017. As can be seen, in the quarter following the introduction there is an increase in the proportion of ZHCs. The first two columns of Table 4.13 present an empirical counterpart to the graph from the following estimating equation:

$$ZHC_{i,t} = \alpha_5 + \beta_5 \cdot PostNLW_t + X'_{i,t} \cdot \gamma_5 + u_{i,t} \tag{4.5}$$

where $ZHC$ is a binary indicator of ZHC status for worker $i$ in period $t$; $PostNLW$ is a dummy taking value one after March 2016; $X$ is a vector of individual-level controls including age, education, and dummies for gender, white ethnicity, British nationality, working in the public sector and regional location; $u$ is a disturbance term.[22]

The results shown in the first two columns of Table 4.13 demonstrate that, following the NLW introduction, the proportion of workers employed on ZHCs in the social care sector increased. In the column (2) specification including controls, it rose by 1 percentage point, or a sizable 24 percent of the pre-NLW mean.[23] Furthermore, this positive association appears generalizable to other lowing paying industries. Columns (3) and (4) of Table 4.13 present results for estimates of equation 4.5 using a sample of all workers employed in low paying industries.[24] As can be seen, the results are almost identical to those for the social care industry. Table 4.B6 in Appendix 4.B breaks down the results into all 13 low paying industries and as can be seen all industries (aside from security) have a positive $\beta_5$ coefficient (albeit with varying magnitudes and degrees of significance). Given the evidence outlined earlier in this section using the NMDS-SC data, we feel there is substantive evidence to suggest that the increase in ZHC utilization in the social care industry and in low paying industries in general in the national statistics is due to the NLW introduction.

## 4.6 Conclusion

This paper offers new evidence on the rise and nature of alternative work arrangements, with a specific focus on ZHCs in the context of the UK labor market. Combining both secondary and newly collected survey data, we provide a comprehensive assessment of the nature of ZHCs, which had been so far only very limitedly studied. The survey data allow us to empirically document the characteristics of workers engaged in ZHCs and to better understand the trade-off between flexibility and insecure, low pay that is inherent in this type of work arrangement.

[22] Twelve region dummies were included in total.

[23] A regression using only care workers (i.e. based on occupation rather than industry) yields a similar result, with a coefficient of 0.018 and a standard error of 0.007, representing a 17 percent increase on the pre-NLW mean.

[24] The low paying industries used are those in the UK's Low Pay Commission list, which can be found in Low Pay Comission (2017), and are listed in Table 4.B6 in Appendix 4.B.

Furthermore, we investigate whether minimum wage policies have a role in the increased utilization of ZHCs by firms. We do so by leveraging a novel matched employer employee dataset of English adult social care providers and credible identifying variation stemming from the NLW introduction in the UK labor market.

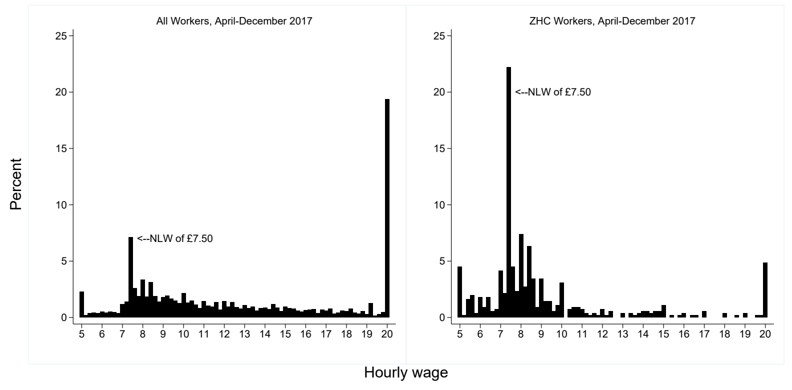
The analysis finds that many workers on ZHCs are relatively low paid, with a large proportion being paid at or slightly above the minimum wage. Such relatively low pay, coupled with limited and fragmented hours, implies high levels of earnings insecurity for workers whose only option is to work on this type of arrangement. Indeed, a stark dichotomy emerges between workers who value the flexibility provided by ZHC jobs, and workers who would rather work more and more regular hours and therefore appear to be engaged in ZHCs out of necessity rather than choice.

The analysis reveals that minimum wage policies appear to have had some bearing on the increased utilization of ZHCs. Specifically, in the context of the English adult social care sector, we find that the NLW introduction led to a larger incidence of ZHCs. The increase is more highly pronounced in the domiciliary care sector, a sector in which work has traditionally been organized around fragmented hours. This suggests that firms exploit the flexibility of ZHCs in order to buffer the wage cost shock induced by the minimum wage increase. It remains to be understood whether these effects will stabilize or grow larger in the longer run – an issue we intend to study in due course. Similarly, the issue of whether there should be a higher minimum wage for ZHC workers (as suggested in the 2017 Taylor Review of Modern Working Practices) is a research question that needs economic evidence to better inform its viability as a future option for labor market policy.[25] In particular, our evidence suggests that a domiciliary worker paid the NMW experienced both an increase of 7.5 percent in their wages and 6.1 percent in their probability of being on a ZHC as a result of the NLW introduction, and such a trade-off may have important welfare implications for workers, both in their current employment and for their future career trajectories.

[25] Taylor (2017).

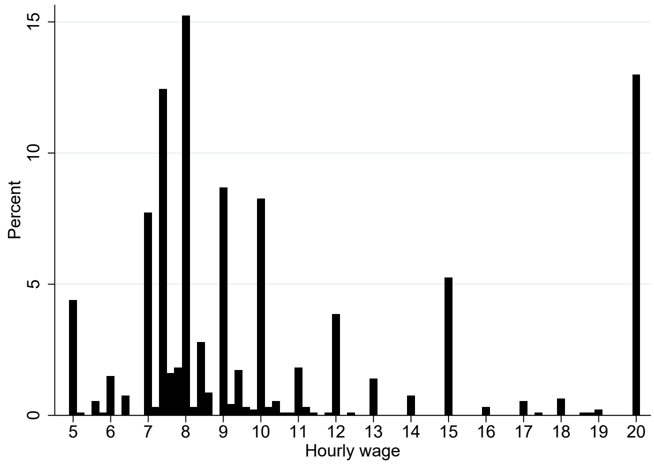
## 4.7 Figures

FIGURE 4.1: HOURLY WAGE DISTRIBUTION FOR ALL WORKERS AND WORKERS ON ZHC



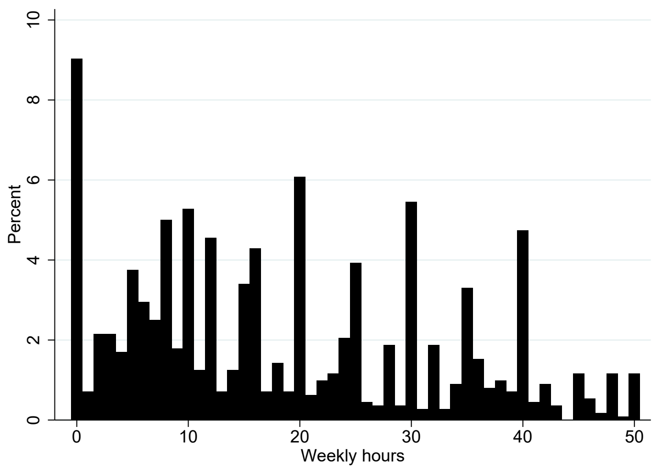

**Notes:** The graphs show the distribution of hourly wages for all workers and workers who declare to be on a ZHC. The distribution is censored at £5 and £20.00. The data are binned into £0.20 bins. NLW denotes the level of the National Living Wage.
**Source:** LFS.

FIGURE 4.2: HOURLY WAGE DISTRIBUTION FOR WORKERS ON ZHC



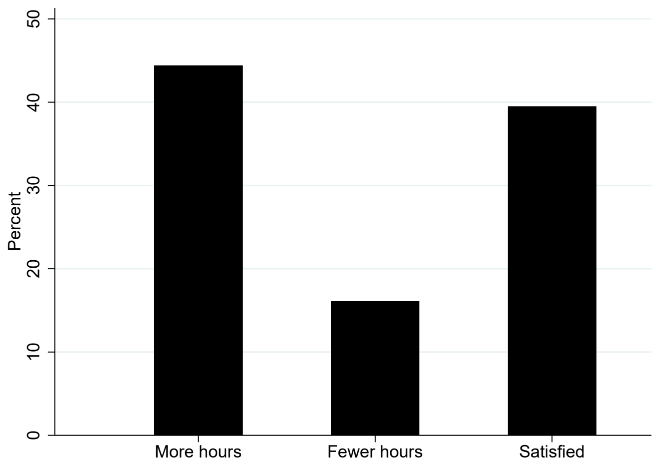

**Notes:** The graph shows the distribution of hourly wages for respondents who declare to be on a ZHC. The distribution is censored at £5.00 and £20.00. The data are binned into £0.20 bins.
**Source:** LSE-CEP survey.

FIGURE 4.3: WEEKLY HOURS DISTRIBUTION FOR WORKERS ON ZHC



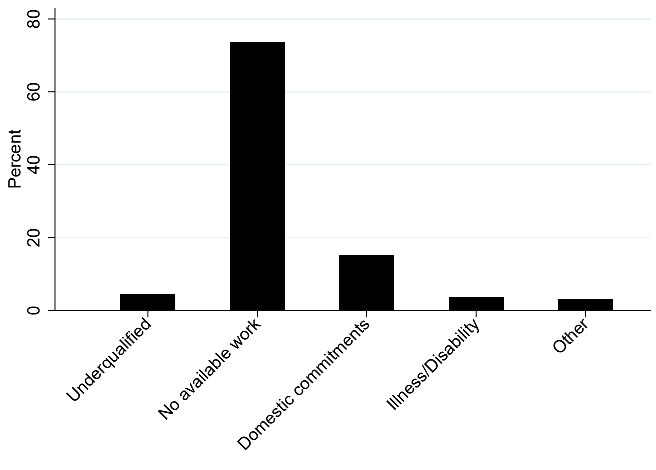

**Notes:** The graph shows the distribution of weekly hours of work for respondents who declare to be on a ZHC. The distribution is trimmed at the $95^{th}$ percentile.
**Source:** LSE-CEP survey.

FIGURE 4.4: DESIRED HOURS FOR WORKERS ON ZHC



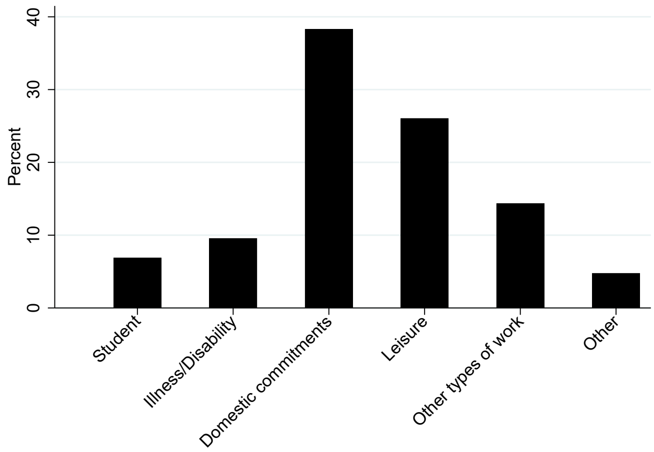

**Notes:** The graph shows the distribution of responses to the question "Would you have preferred to work more or fewer hours last week in your zero hours contract or on-call job at that wage rate? Or were you satisfied with the number of hours you worked?".
**Source:** LSE-CEP survey.

FIGURE 4.5: REASON FOR NOT WORKING MORE HOURS (WORKERS ON ZHC)



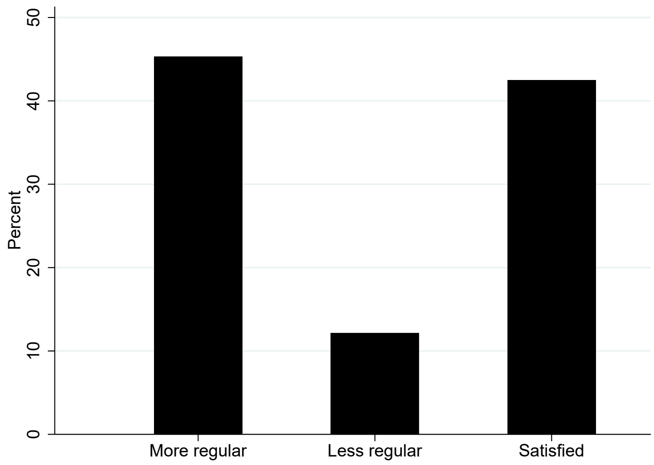

**Notes:** The graph shows the distribution of responses to the question "Why were you NOT able to work more last week?".
**Source:** LSE-CEP survey.

FIGURE 4.6: REASON FOR NOT WORKING FEWER HOURS (WORKERS ON ZHC)



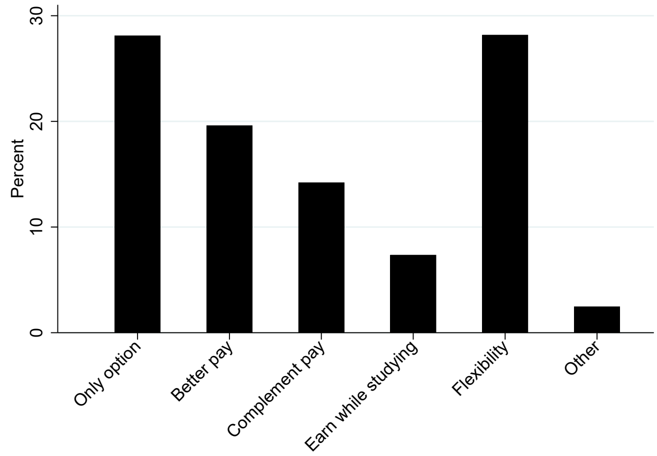

**Notes:** The graph shows the distribution of responses to the question "Why would you want to work fewer hours?".
**Source:** LSE-CEP survey.

FIGURE 4.7: DESIRED PATTERN OF HOURS FOR WORKERS ON ZHC



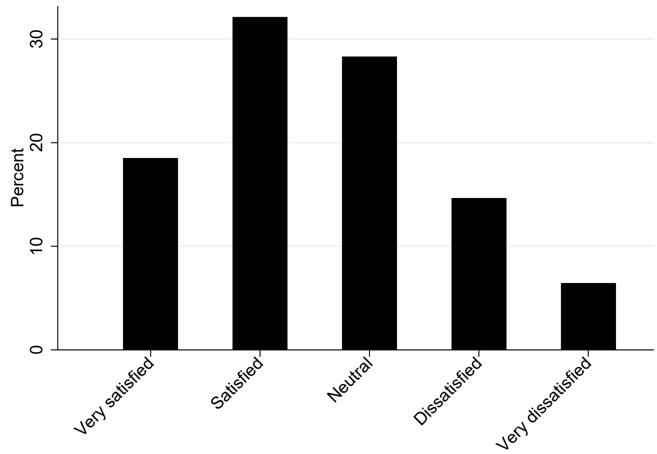

**Notes:** The graph shows the distribution of responses to the question "Would you have preferred to work a pattern of more regular hours last week on your zero hours contract or on-call job at that wage rate? Or were you satisfied with the pattern of hours you worked?".
**Source:** LSE-CEP survey.

FIGURE 4.8: MAIN REASON FOR BEING ON ZHC



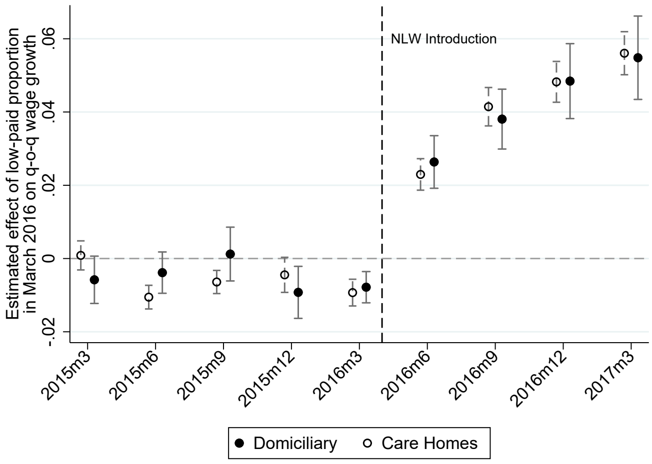

**Notes:** The graph shows the distribution of responses to the question "Which is the most important reason why you work on a zero hours contract or on-call job?".
**Source:** LSE-CEP survey.

FIGURE 4.9: JOB SATISFACTION OF WORKERS ON ZHC



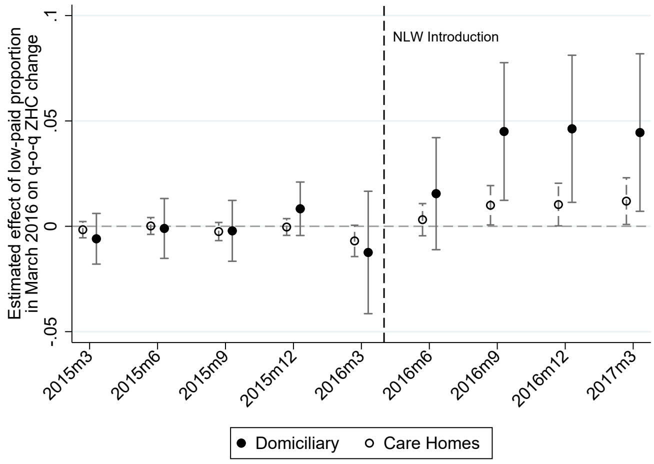

**Notes:** The graph shows the distribution of responses to the question "How satisfied are you with working on a zero hours contract or on-call job?".
**Source:** LSE-CEP survey.

FIGURE 4.10: EFFECT OF INITIAL LOW-PAID PROPORTION ON WAGE GROWTH BY SECTOR



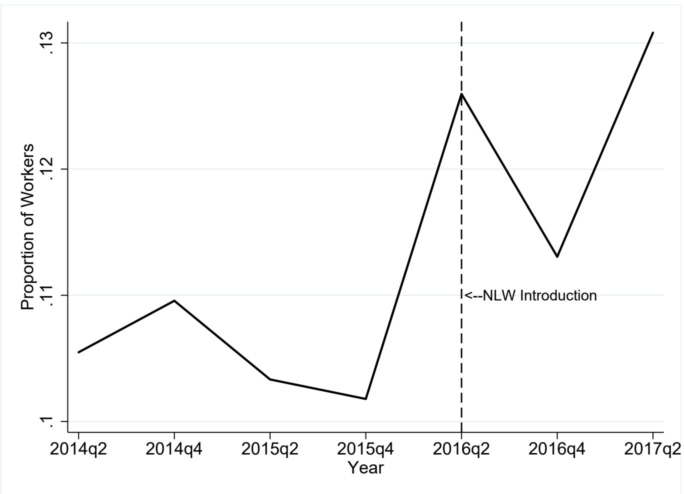

**Notes:** For the quarters before the NLW introduction, the graph reports the estimated coefficients $\hat{\beta}_{2,t}$ from model 4.2 for care homes and domiciliary care agencies. After the NLW introduction, the graph reports the estimated sum $\sum_{t=1}^{k} \hat{\beta_{2,t}}$ for $k = 1, \ldots, 4$. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables.
**Source:** NMDS-SC.

FIGURE 4.11: EFFECT OF INITIAL LOW-PAID PROPORTION ON PROPORTION OF EMPLOYEES ON ZHC BY SECTOR




**Notes:** For the quarters before the NLW introduction, the graph reports the estimated coefficients $\hat{\beta}_{1,t}$ from model 4.1 for care homes and domiciliary care agencies. After the NLW introduction, the graph reports the estimated sum $\sum_{t=1}^{k} \hat{\beta_{1,t}}$ for $k = 1, \ldots, 4$. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. The vertical bars indicate 95 percent confidence intervals based on robust standard errors. Control variables included in the underlying regression are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables.
**Source:** NMDS-SC.

FIGURE 4.12: PROPORTION OF CARE WORKERS ON ZHC (LFS)




**Notes:** The graph presents the evolution of the proportion of care workers on ZHCs from April 2014 to April 2017. The dashed line marks the introduction of the NLW at the start of $2^{nd}$ quarter in 2016.
**Source:** LFS.

## 4.8 Tables

TABLE 4.1: FORMS OF EMPLOYMENT IN THE UK

| | Permanent employment | Zero hours contract | Self-employment |
|---|---|---|---|
| National Insurance contributions | Employers pay NI contributions on their employee's earnings and benefits, above the threshold of £162 a week, at a rate of 13.8%. Employees pay NI on their earnings and benefits above the threshold of £162 a week at a rate of 12%. Above the earnings threshold of £892 a week this drops to 2% | Employers pay NI contributions on their employee's earnings and benefits, above the threshold of £162 a week, at a rate of 13.8%. Employees pay NI on their earnings and benefits above the threshold of £162 a week at a rate of 12%. Above the earnings threshold of £892 a week this drops to 2% | Contributions are only made by the worker. Above the yearly profit threshold of £6,205 there is a flat rate of £2.95 per week. Between £8,424 and £46,350 there is a rate of 9% and above £46,350 the rate drops to 2% |
| Minimum wage | Covered | Covered | Not covered |
| Holiday pay | Full-time employees are entitled to 28 days paid holiday leave per year, and part time employees the pro-rata equivalent | Entitled to the same degree of holiday pay as permanent employees. Due to the nature of ZHC work, many firms include holiday pay in the workers hourly wage rate | Not entitled to holiday pay |
| Sick pay | Entitled to statutory sick pay, only if they earn at least £116 per week | Entitled to statutory sick pay, only if they earn at least £116 on average from one employer | Not entitled sick pay |
| Unfair dismissal protection, Minimum notice periods and Statutory redundancy pay | Protected against unfair dismissal, covered by statutory minimum notice periods, and entitled to statutory redundancy pay | Employer could offer zero hours in perpetuity, thus effectively no protection against unfair dismissal, no minimum notice period, no redundancy pay | Not covered by unfair dismissal protection, minimum notice periods or statutory redundancy pay |

**Notes:** National Insurance contributions build up your state pension, whilst also helping to pay for the NHS and other welfare services. Reports from the UK's Citizens Advice Bureau suggests some employers attempt to avoid paying out sick pay to ZHC workers, and stop hours for those workers who do try to claim. Some instances of case law in the UK have tried to establish that ZHC workers who work regular hours may be eligible for aspects of dismissal protection.
**Source:** UK Government Website.

TABLE 4.2: ZERO HOUR-LIKE CONTRACTS IN EUROPE AND THE UNITED STATES

| Country | Contract type | Description and/or regulation |
|---|---|---|
| France | N/A | ZHCs are outlawed in most cases. All part-time contracts must include the number and distribution of hours. Collective bargaining agreements require a minimum of 24 hours per week but can be reduced at the request of the employee. Exceptions for youth in education and temporary agency workers |
| Germany | On-call work | Generally, contracts must specify weekly and daily working hours. If agreed by the employer and employee (or employee representative) a contract could avoid specifying weekly working hours, in which case 10 weekly working hours are deemed to be agreed. If the daily working hours are not specified, the employer is bound to call the employee for at least 3 consecutive hours per day |
| Italy | On-call work | Contracts exist but are heavily regulated. Contracts must be justified by reference to production cycles and organization needs, and companies who use them must notify the ministry of labour. Banned from public administration, weekend work and bank holiday work. Only workers under 25 and over 55 can be placed on them. Limits to 400 working days over 3 years and then automatic conversion into full-time permanent contract |
| Sweden | On-call work | These contracts give no fixed hours and the employer can vary the working hours. No known regulation |
| Norway | Zero hours contract | Until recently such contracts made up around 0.8% of the workforce. Case law from 2005 and 2017 has deemed the use of permanent contracts where employees were to work only on-call as illegal and evading temporary employment law (which has strict usage and limitations). New regulation has been proposed by government to explicitly prohibit ZHCs |
| Netherlands | Zero hours contract | Unlike the UK, there is an obligation on behalf of the employee to work when called upon. Each time an employee is called to worker, they must be paid a minimum of 3 hours wages (even if there is less than 3 hours work for them). Following 3 months of continuous employment on a ZHC, the agreed number of hours adjusts to the average number of hours during the previous 3 months |
| | Min-max contract | Employees are given a guaranteed number of hours – weekly, monthly or annually. These are always paid even if the employer is unable to provide work. If the guaranteed number of hours per week is 15 hours or less, then similar regulation to the ZHCs is enforceable. During periods of high demand, employers and employees can agree upon extra hours |
| United States | On-call/ "Just-in-time" schedules | Diffusion of on-call working arrangements have increased from 1.6% in 1995 to 2.6% in 2015 (Katz and Krueger, 2019a). There is no federal regulation, however eight states operate "show-up pay" laws, where employers are required to pay workers for a minimum number of hours (no matter how long they work), if they have been called to work. Coverage however varies across these eight states, and a number of exemptions exist. A few cities (e.g. San Francisco, Seattle, New York) operate fair scheduling ordinances. For example, San Francisco requires new employees to receive a written estimate of their expected days and hours of shifts. Schedules must be posted at least two weeks in advance, changes with less than a week notice results in compensation entitlement for the employee, and employees required to be on call but not working are also entitled to some compensation. If employers have available hours, these must be offered to current part-time employees before hiring additional part-time workers |

**Source:** Eurofund, 2015; O'Sullivan et al., 2015; McCrate, 2018.

TABLE 4.3: LFS DESCRIPTIVE STATISTICS

| | All Employees 2017 | | ZHC Employees 2017 | |
|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. |
| Age | 43.43 | 13.39 | 38.22 | 16.67 |
| Prop. female | 0.49 | 0.50 | 0.59 | 0.49 |
| Prop. in full-time education | 0.03 | 0.17 | 0.17 | 0.38 |
| Age when completed full-time education | 18.63 | 3.10 | 18.32 | 3.05 |
| Median tenure | 5-10 yrs | | 1-2 yrs | |
| Prop. part-time | 0.29 | 0.45 | 0.67 | 0.47 |
| Prop. aged under 25 | 0.09 | 0.29 | 0.31 | 0.46 |
| Hourly wage | 14.73 | 11.78 | 9.77 | 7.46 |
| Hourly wage (aged 25+) | 15.42 | 12.13 | 10.76 | 7.96 |
| Hourly wage (aged under 25) | 8.24 | 3.63 | 7.47 | 5.50 |
| Hourly wage (median) | 11.50 | | 7.90 | |
| Weekly hours | 31.40 | 17.38 | 21.33 | 16.98 |
| Prop. wanting more hours | 0.08 | 0.27 | 0.25 | 0.43 |
| Observations | 71,604 | | 1,907 | |

**Notes:** The table reports the mean and standard deviation of a set of individual characteristics for the employees from the LFS, for both all employees and ZHC workers, in 2017. The ZHC indicator only appears in April-June and October-December quarters of the LFS. Thus the above statistics use only those two quarters for each year. Wage data only appears in two waves of the survey, thus wage stats are based off approximately one third of the number of observations.
**Source:** LFS.

TABLE 4.4: THE BITE OF THE NATIONAL LIVING WAGE

| | All Employees | | | | ZHC Employees | | | |
|---|---|---|---|---|---|---|---|---|
| | 2016 | | 2017 | | 2016 | | 2017 | |
| | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. |
| Prop. paid below next NLW | 0.20 | 0.40 | 0.20 | 0.40 | 0.54 | 0.50 | 0.49 | 0.50 |
| Prop. paid below next NLW (25+) | 0.16 | 0.37 | 0.16 | 0.36 | 0.41 | 0.49 | 0.39 | 0.49 |
| Prop. paid at NLW | 0.06 | 0.23 | 0.06 | 0.24 | 0.18 | 0.38 | 0.20 | 0.40 |
| Prop. paid at NLW (25+) | 0.05 | 0.23 | 0.06 | 0.24 | 0.21 | 0.41 | 0.22 | 0.42 |
| Observations | 20,638 | | 21,102 | | 606 | | 554 | |

**Notes:** The table reports the mean and standard deviation of proportions of employees impacted by the NLW, for both all employees and ZHC workers, for the years 2016 and 2017.
**Source:** LFS.

TABLE 4.5: TRANSITIONS OUT OF ZHC WORK (BETWEEN QUARTER T AND T+5)

| Status in T | Status in T+5 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Inactive | Unempl | Emp FT | Emp PT | Self FT | Self PT | ZHC | Total |
| Inactive | 84.89 | 3.79 | 2.23 | 5.68 | 0.38 | 1.82 | 1.21 | 100.00 [2,641] |
| Unempl | 21.20 | 36.71 | 19.94 | 15.19 | 0.63 | 1.90 | 4.43 | 100.00 [316] |
| Emp FT | 2.47 | 1.13 | 88.91 | 4.41 | 1.79 | 0.49 | 0.81 | 100.00 [4,697] |
| Emp PT | 7.20 | 1.55 | 9.50 | 76.22 | 0.75 | 1.55 | 3.22 | 100.00 [1,737] |
| Self FT | 2.58 | 0.49 | 8.11 | 0.86 | 79.85 | 6.88 | 1.23 | 100.00 [814] |
| Self PT | 11.50 | 1.47 | 2.95 | 6.19 | 10.03 | 66.08 | 1.77 | 100.00 [339] |
| ZHC | 15.17 | 4.83 | 16.55 | 20.00 | 4.14 | 2.76 | 36.55 | 100.00 [145] |
| Total | 24.62 [2,632] | 2.92 [312] | 42.69 [4,563] | 16.71 [1,786] | 7.47 [799] | 3.63 [388] | 1.96 [209] | 100.00 [10,689] |

**Notes:** For each type of economic activity today, the table reports the percentage of respondents by working arrangement in 5 quarters time. The data is pooled from the LFS panel survey, from January 2015 to March 2018. For all those in some form of employment, their primary job is reported. Sample sizes reported in square brackets.
**Source:** LFS.

TABLE 4.6: TRANSITIONS INTO ZHC WORK (BETWEEN QUARTER T AND T+5)

| Status in T | Status in T+5 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Inactive | Unempl | Emp FT | Emp PT | Self FT | Self PT | ZHC | Total |
| Inactive | 85.18 | 32.05 | 1.29 | 8.40 | 1.25 | 12.37 | 15.31 | 24.71 [2,641] |
| Unempl | 2.55 | 37.18 | 1.38 | 2.69 | 0.25 | 1.55 | 6.70 | 2.96 [316] |
| Emp FT | 4.41 | 16.99 | 91.52 | 11.59 | 10.51 | 5.93 | 18.18 | 43.94 [4,697] |
| Emp PT | 4.75 | 8.65 | 3.62 | 74.13 | 1.63 | 6.96 | 26.79 | 16.25 [1,737] |
| Self FT | 0.80 | 1.28 | 1.45 | 0.39 | 81.35 | 14.43 | 4.78 | 7.62 [814] |
| Self PT | 1.48 | 1.60 | 0.22 | 1.18 | 4.26 | 57.73 | 2.87 | 3.17 [339] |
| ZHC | 0.84 | 2.24 | 0.53 | 1.62 | 0.75 | 1.03 | 25.36 | 1.36 [145] |
| Total | 100.00 [2,632] | 100.00 [312] | 100.00 [4,563] | 100.00 [1,786] | 100.00 [799] | 100.00 [388] | 100.00 [209] | 100.00 [10,689] |

**Notes:** For each type of economic activity today, the table reports the percentage of respondents by working arrangement 5 quarters before. The data is pooled from the LFS panel survey, from January 2015 to March 2018. For all those in some form of employment, their primary job is reported. Sample sizes reported in square brackets.
**Source:** LFS.

TABLE 4.7: SAMPLE OF ZHC WORKERS IN LSE-CEP SURVEY

| | Mean | S.D. |
|---|---|---|
| Female | 0.53 | 0.50 |
| Age | 36.28 | 13.21 |
| Age 18-24 | 0.26 | 0.44 |
| Age 25-34 | 0.25 | 0.43 |
| Age 35-44 | 0.19 | 0.39 |
| Age 45-54 | 0.18 | 0.38 |
| Age 55-65 | 0.13 | 0.33 |
| No qualifications | 0.02 | 0.13 |
| Some GCSE/O levels | 0.10 | 0.30 |
| 5 or more GCSE/O levels | 0.13 | 0.34 |
| Trade/technical/vocational training | 0.11 | 0.31 |
| A levels | 0.23 | 0.42 |
| Bachelor's degree | 0.27 | 0.45 |
| Master's degree | 0.11 | 0.31 |
| Doctorate degree | 0.03 | 0.16 |
| North East | 0.05 | 0.22 |
| North West | 0.12 | 0.32 |
| Yorkshire and Humberside | 0.06 | 0.23 |
| East Midlands | 0.08 | 0.27 |
| West Midlands | 0.09 | 0.29 |
| Eastern England | 0.08 | 0.26 |
| London | 0.19 | 0.40 |
| South East | 0.12 | 0.33 |
| South West | 0.08 | 0.27 |
| Wales | 0.04 | 0.20 |
| Scotland | 0.07 | 0.26 |
| Northern Ireland | 0.02 | 0.15 |
| Married/Cohabiting | 0.44 | 0.50 |
| Widow/Separated/Divorced | 0.10 | 0.30 |
| Never married | 0.45 | 0.50 |
| Children | 0.55 | 0.50 |
| White | 0.84 | 0.37 |
| Mixed/Multiple ethnic group | 0.04 | 0.20 |
| Asian/Asian British | 0.06 | 0.23 |
| Black/African/Caribbean/Black British | 0.06 | 0.23 |
| Arab | 0.00 | 0.06 |
| Observations | 1,167 | |

TABLE 4.7 CONTINUED: SAMPLE OF ZHC WORKERS IN LSE-CEP SURVEY

| | Mean | S.D. |
|---|---|---|
| Multiple employers (ZHC jobs) | 0.42 | 0.49 |
| Non-ZHC job holder | 0.34 | 0.47 |
| Agriculture, forestry and fishing | 0.01 | 0.08 |
| Mining and quarrying | 0.01 | 0.08 |
| Manufacturing | 0.07 | 0.25 |
| Electricity, gas, steam and air conditioning supply | 0.02 | 0.15 |
| Water supply, sewerage, waste management | 0.01 | 0.10 |
| Construction | 0.06 | 0.24 |
| Wholesale and retail trade, repair of motor vehicles | 0.09 | 0.29 |
| Transportation and storage | 0.06 | 0.24 |
| Accommodation and food service activities | 0.11 | 0.32 |
| Information and communication | 0.05 | 0.22 |
| Financial and insurance activities | 0.03 | 0.18 |
| Real estate activities | 0.01 | 0.07 |
| Professional, scientific and technical activities | 0.03 | 0.16 |
| Administrative and support service activities | 0.05 | 0.23 |
| Public administration and defense | 0.01 | 0.10 |
| Education | 0.09 | 0.29 |
| Human health and social work activities | 0.15 | 0.36 |
| Arts, entertainment and recreation | 0.06 | 0.24 |
| Other service activities | 0.06 | 0.23 |
| Activities of households as employers of domestic personnel | 0.01 | 0.12 |
| Activities of extraterritorial organizations | 0.00 | 0.07 |
| Other | 0.01 | 0.07 |
| Hourly wage | 11.63 | 8.16 |
| Hourly Wage (median) | 8.64 | |
| Hours worked in previous week | 18.62 | 13.67 |
| Different days worked per week | 4.06 | 1.71 |
| Proportion doing unpaid hours | 0.32 | 0.47 |
| Average weekly unpaid hours | 7.08 | 9.02 |
| Less than one year of working experience | 0.05 | 0.23 |
| 1-3 years of working experience | 0.17 | 0.38 |
| 3-5 years of working experience | 0.15 | 0.36 |
| More than 5 years of experience | 0.62 | 0.48 |
| Less than one year of working experience in ZHC | 0.52 | 0.50 |
| 1-3 years of working experience in ZHC | 0.21 | 0.41 |
| 3-5 years of working experience in ZHC | 0.14 | 0.35 |
| More than 5 years of experience in ZHC | 0.13 | 0.34 |
| Received work-related training in the last year | 0.55 | 0.50 |
| Observations | 1,167 | |

**Notes:** The table reports the mean and standard deviation of a set of individual characteristics for the sample of respondents who declared to be on a ZHC in the week prior to taking the survey.
**Source:** LSE-CEP survey.

TABLE 4.8: TRAINING OF WORKERS ON ZHC

| | Received last year (1) | Most useful (2) |
|---|---|---|
| Technical or technology training | 0.18 | 0.23 |
| Quality training | 0.30 | 0.24 |
| Skills training | 0.54 | 0.50 |
| Continuing education | 0.13 | 0.20 |
| Professional training and legal training | 0.22 | 0.24 |
| Managerial training | 0.15 | 0.23 |
| Safety training | 0.56 | 0.27 |
| Other | 0.01 | 0.02 |
| Observations | 644 | 1,167 |

**Notes:** The table reports answers to the question "What type of training [did you receive last year]?" in column (1) and to the question "What type of training would you find most useful to improve your job prospects?" in column (2). The table reports the proportion of respondents who ticked each of the preset options.
**Source:** LSE-CEP survey.

TABLE 4.9: WHO PAYS FOR THE TRAINING OF WORKERS ON ZHC

| | Who pays |
|---|---|
| Me or a family member | 0.16 |
| A contractor or customer | 0.11 |
| My employer | 0.59 |
| Someone else | 0.02 |
| No one, it was free | 0.12 |
| Observations | 644 |

**Notes:** The table reports answers to the question "Who paid for the cost of the training?". The table reports the proportion of respondents who ticked each of the preset options.
**Source:** LSE-CEP survey.

TABLE 4.10: NMDS-SC SUMMARY STATISTICS

| | All firms | | Care homes | | Domiciliary care | |
|---|---|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. | Mean | S.D. |
| Number of employees | 45.22 | 46.26 | 38.99 | 31.16 | 65.97 | 74.00 |
| Proportion under 25 | 0.12 | 0.09 | 0.12 | 0.09 | 0.12 | 0.09 |
| Hourly wage | 7.57 | 1.09 | 7.53 | 1.11 | 7.67 | 1.01 |
| Weekly hours | 25.61 | 8.90 | 28.56 | 5.17 | 15.75 | 11.31 |
| Weekly earnings | 189.42 | 79.01 | 212.80 | 54.35 | 111.59 | 96.54 |
| Hourly wage carer | 7.10 | 0.93 | 7.01 | 0.97 | 7.43 | 0.68 |
| Weekly hours carer | 24.49 | 10.30 | 27.98 | 6.25 | 12.41 | 12.25 |
| Prop. on ZHC | 0.12 | 0.23 | 0.05 | 0.10 | 0.38 | 0.33 |
| Prop. on permanent contract | 0.88 | 0.17 | 0.90 | 0.11 | 0.82 | 0.27 |
| Prop. on temporary contract | 0.02 | 0.08 | 0.02 | 0.04 | 0.05 | 0.15 |
| Prop. on bank contract | 0.06 | 0.10 | 0.06 | 0.08 | 0.05 | 0.13 |
| Prop. on agency contract | 0.01 | 0.08 | 0.00 | 0.02 | 0.04 | 0.16 |
| Female | 0.85 | 0.13 | 0.84 | 0.13 | 0.87 | 0.11 |
| Age | 42.60 | 4.63 | 42.71 | 4.53 | 42.21 | 4.92 |
| Prop. carer | 0.61 | 0.19 | 0.56 | 0.16 | 0.75 | 0.23 |
| Prop. with nursing qualification | 0.03 | 0.06 | 0.04 | 0.07 | 0.00 | 0.01 |
| Occupancy rate | 0.77 | 0.33 | 0.92 | 0.14 | 0.27 | 0.30 |
| Proportion paid below NLW | 0.48 | 0.34 | 0.52 | 0.32 | 0.34 | 0.36 |
| Observations | 4,680 | | 3,599 | | 1,081 | |

**Notes:** The table reports the mean and standard deviation of a set of firm-level variables for the balanced sample of firms used in the analysis. The statistics refer to March 2016, and are shown for the full sample, and for the sample of care homes and domiciliary care agencies separately.
**Source:** NMDS-SC.

TABLE 4.11: WAGE EQUATIONS

Change in log average hourly wage

March 2016 to March 2017

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | 0.053*** | 0.054*** | 0.056*** | 0.056*** |
| | (0.002) | (0.003) | (0.003) | (0.003) |
| Initial low-paid proportion x Domiciliary | | -0.001 | | -0.001 |
| | | (0.006) | | (0.006) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes |
| F-stat | 519.52 | 280.43 | 410.41 | 203.22 |
| Mean of dep. var.: | | | | |
| All firms | 0.041 | | | |
| Care homes | 0.043 | | | |
| Domiciliary care | 0.036 | | | |

**Notes:** The table reports the estimated coefficient $\hat{\beta}_3$ from model 4.3. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables.
**Source:** NMDS-SC.

TABLE 4.12: ZERO HOURS CONTRACTS EQUATIONS

Change in proportion of employees on ZHCs

March 2016 to March 2017

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | 0.001 | 0.006* | 0.014** | 0.012** | | |
| | (0.006) | (0.004) | (0.007) | (0.006) | | |
| Initial low-paid prop x Domic | | 0.039** | | 0.033* | | |
| | | (0.019) | | (0.019) | | |
| Change in log avg wage | | | | | 0.257** | 0.219** |
| | | | | | (0.126) | (0.101) |
| Change in log avg wage x Domic | | | | | | 0.596* |
| | | | | | | (0.350) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Mean of dep. var.: | | | | | | |
| All firms | 0.019 | | | | | |
| Care homes | 0.006 | | | | | |
| Domiciliary care | 0.061 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\hat{\beta}_3$ from model 4.3 in columns (1)-(4), and the estimated IV coefficient $\hat{\beta}_4$ from model 4.4 in columns (5)-(6), using the change in the share of workers on ZHC as outcome variable. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables.
**Source:** NMDS-SC.

TABLE 4.13: ZERO HOURS CONTRACTS EQUATIONS (LFS SAMPLE)

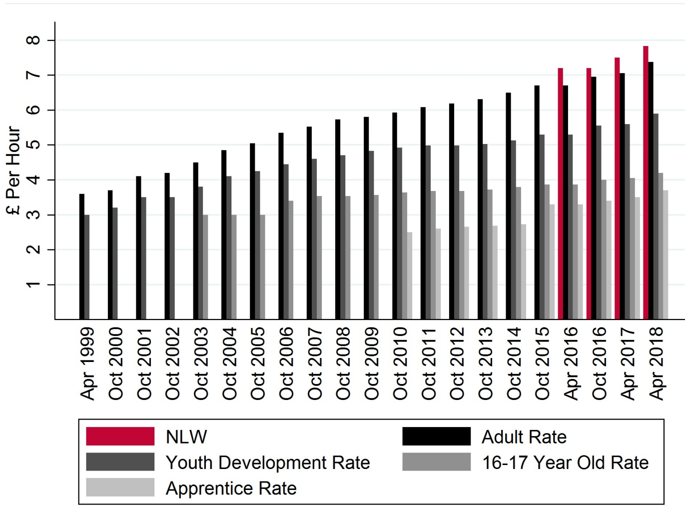
Probability of being on a ZHC

| | Social care | | Low-pay industries | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Post NLW | 0.011*** | 0.010*** | 0.008*** | 0.010*** |
| | (0.003) | (0.003) | (0.001) | (0.001) |
| Observations | 25,191 | 25,191 | 91,362 | 91,362 |
| Controls | No | Yes | No | Yes |
| Pre-NLW mean of dep. var. | 0.042 | 0.042 | 0.041 | 0.041 |

**Notes:** The table reports the estimated reduced-form coefficient $\hat{\beta}_5$ from model 4.5. The sample for the first two columns is workers employed in the social care industry, and for the second pair of columns is workers employed in low-pay industries (defined in Low Pay Comission, 2017). The samples contain 4 pre-NLW quarters (2014-2015 quarter 2 and quarter 4) and 3 post-NLW quarters (2016 quarter 2 and quarter 4, and 2017 quarter 2). Controls include age, education, gender, a dummy for white ethnicity, a dummy for British nationality, a dummy for working in the public sector and twelve regional dummies. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1.
**Source:** NMDS-SC.

# 4.A Appendix Figures

FIGURE 4.A1: MINIMUM WAGE RATES IN THE UK BETWEEN 1999 AND 2018




**Notes:** The graph reports the various minimum wage rates in the UK between 1999 and 2018. The apprentice rate applies to apprentices. The 16-17 year-old rate to workers aged 16 and 17. The youth development rate to workers aged 18-20. The adult rate applied to workers aged 21 and over until March 2016. From April 2016, the adult rate applies to workers aged 21-24 and the NLW to those aged 25 and over.
**Source:** Low Pay Commission.

## 4.B Appendix Tables

TABLE 4.B1: SAMPLE OF SURVEY RESPONDENTS OF LSE-CEP SURVEY

| | Mean | S.D. |
|---|---|---|
| Female | 0.53 | 0.50 |
| Age | 40.93 | 13.04 |
| Age 18-24 | 0.14 | 0.35 |
| Age 25-34 | 0.21 | 0.41 |
| Age 35-44 | 0.22 | 0.41 |
| Age 45-54 | 0.25 | 0.43 |
| Age 55-65 | 0.19 | 0.39 |
| No qualifications | 0.04 | 0.19 |
| Some GCSE/O levels | 0.12 | 0.32 |
| 5 or more GCSE/O levels | 0.13 | 0.34 |
| Trade/technical/vocational training | 0.12 | 0.33 |
| A levels | 0.22 | 0.41 |
| Bachelor's degree | 0.26 | 0.44 |
| Master's degree | 0.09 | 0.28 |
| Doctorate degree | 0.02 | 0.12 |
| North East | 0.05 | 0.22 |
| North West | 0.11 | 0.32 |
| Yorkshire and Humberside | 0.09 | 0.29 |
| East Midlands | 0.08 | 0.27 |
| West Midlands | 0.09 | 0.29 |
| Eastern England | 0.07 | 0.26 |
| London | 0.12 | 0.33 |
| South East | 0.15 | 0.35 |
| South West | 0.08 | 0.27 |
| Wales | 0.05 | 0.22 |
| Scotland | 0.08 | 0.27 |
| Northern Ireland | 0.02 | 0.14 |
| Employed by government | 0.17 | 0.38 |
| Employed by private company | 0.49 | 0.50 |
| Employed by non-profit organization | 0.07 | 0.26 |
| Self-employed, with or without employees | 0.11 | 0.32 |
| Working in the family business | 0.01 | 0.11 |
| Only work last week was filling out surveys | 0.03 | 0.17 |
| Did not have a job last week | 0.12 | 0.32 |
| Observations | 18,831 | |

**Notes:** The table reports the mean and standard deviation of a set of individual characteristics for the full sample of respondents to the LSE-CEP Survey of Self-Employment and Alternative Work Arrangements.
**Source:** LSE-CEP survey.

TABLE 4.B2: CEP-LSE SURVEY REPRESENTATIVENESS BASED ON LFS 2017

| | All individuals | | ZHC workers | |
|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. |
| Female | 0.52 | 0.50 | 0.60 | 0.49 |
| Age | 42.78 | 13.34 | 37.85 | 14.91 |
| Age 18-24 | 0.11 | 0.32 | 0.28 | 0.45 |
| Age 25-34 | 0.19 | 0.40 | 0.19 | 0.39 |
| Age 35-44 | 0.22 | 0.41 | 0.16 | 0.37 |
| Age 45-54 | 0.24 | 0.43 | 0.18 | 0.38 |
| Age 55-65 | 0.24 | 0.43 | 0.19 | 0.39 |
| No Qualifications | 0.08 | 0.26 | 0.06 | 0.24 |
| GCSE/O levels | 0.20 | 0.40 | 0.22 | 0.41 |
| Trade/Technical/Other | 0.09 | 0.28 | 0.10 | 0.30 |
| A Levels | 0.23 | 0.42 | 0.28 | 0.45 |
| Bachelor's Degree | 0.30 | 0.46 | 0.23 | 0.42 |
| Master's Degree | 0.05 | 0.21 | 0.03 | 0.17 |
| Doctorate Degree | 0.01 | 0.10 | 0.00 | 0.06 |
| North East | 0.04 | 0.20 | 0.05 | 0.22 |
| North West | 0.11 | 0.31 | 0.09 | 0.29 |
| Yorkshire The Humber | 0.09 | 0.28 | 0.08 | 0.28 |
| East Midlands | 0.07 | 0.26 | 0.08 | 0.27 |
| West Midlands | 0.09 | 0.28 | 0.08 | 0.26 |
| East of England | 0.09 | 0.29 | 0.09 | 0.29 |
| London | 0.11 | 0.32 | 0.12 | 0.32 |
| South East | 0.13 | 0.34 | 0.15 | 0.35 |
| South West | 0.09 | 0.29 | 0.11 | 0.32 |
| Wales | 0.04 | 0.21 | 0.01 | 0.11 |
| Scotland | 0.08 | 0.27 | 0.08 | 0.27 |
| Northern Ireland | 0.05 | 0.21 | 0.05 | 0.23 |
| Employed by Public Sector | 0.17 | 0.38 | 0.16 | 0.36 |
| Employed by Private Sector | 0.58 | 0.49 | 0.84 | 0.37 |
| Self-employed, with or without employees | 0.11 | 0.31 | 0.09 | 0.29 |
| Does not have a job | 0.24 | 0.43 | 0.00 | 0.00 |
| Hourly Wage | 14.82 | 11.42 | 9.70 | 7.12 |
| Hourly Wage (median) | 11.55 | | 8.0 | |
| Observations | 108,983 | | 1,686 | |

**Notes:** The table reports summary statistics of individual level characteristics for all working age respondents and ZHC workers. Wage data only appears in two waves of the LFS, thus wage statistics are based off approximately one third of the number of observations.
**Source:** LFS.

TABLE 4.B3: NMDS-SC SURVEY REPRESENTATIVENESS (CARE WORKERS)

| | LFS | | NMDS-SC | |
|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. |
| | (1) | (2) | (3) | (4) |
| Prop. female | 0.85 | 0.36 | 0.85 | 0.13 |
| Age | 42.62 | 13.58 | 42.60 | 4.63 |
| Hourly rate | 7.91 | 1.50 | 7.10 | 0.93 |
| Weekly hours | 28.38 | 16.14 | 24.49 | 10.30 |
| Proportion on ZHC | 0.11 | 0.31 | 0.12 | 0.23 |
| North East | 0.07 | 0.25 | 0.05 | 0.23 |
| North West | 0.13 | 0.34 | 0.13 | 0.34 |
| Yorkshire and Humberside | 0.12 | 0.32 | 0.10 | 0.31 |
| East Midlands | 0.08 | 0.28 | 0.09 | 0.28 |
| West Midlands | 0.11 | 0.31 | 0.12 | 0.33 |
| East England | 0.12 | 0.32 | 0.13 | 0.34 |
| London | 0.09 | 0.28 | 0.06 | 0.24 |
| South East | 0.15 | 0.36 | 0.15 | 0.36 |
| South West | 0.13 | 0.34 | 0.15 | 0.36 |
| Observations | 2,025 | | 4,680 | |

**Notes:** The table reports the mean and standard deviation for a set of individual-level characteristics for care workers in the LFS (columns (1) and (2)). The table also reports the mean and standard deviation for the same set of characteristics at the firm level in NMDS-SC (columns (3) and (4)). The LFS data refer to 2015Q4 and 2016Q1, and the NMDS-SC data to March 2016. The ZHC indicator only appears in April-June and October-December quarters of the LFS. Thus the proportion of ZHC reported in column (1) is based on 2015Q4 data only. Wage data only appears in two waves of the LFS, thus wage statistics in columns (1) and (2) are based off approximately one fifth of the number of observations.
**Source:** LFS and NMDS-SC.

TABLE 4.B4: EMPLOYMENT EQUATIONS

Change in log number of employees

*March 2016 to March 2017*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Initial low-paid proportion | -0.000 | -0.010 | -0.001 | -0.009 |
| | (0.011) | (0.011) | (0.014) | (0.013) |
| Initial low-paid proportion x Domiciliary | | 0.036 | | 0.024 |
| | | (0.032) | | (0.033) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes |
| F-stat | 519.52 | 280.43 | 410.41 | 203.22 |
| Mean of dep. var.: | | | | |
| All firms | 0.013 | | | |
| Care homes | 0.013 | | | |
| Domiciliary care | 0.012 | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\hat{\beta}_3$ from model 4.3, using the change in log headcount employment as outcome variable. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables.
**Source:** NMDS-SC.

TABLE 4.B5: EMPLOYMENT CONTRACT EQUATIONS

Change in proportion of employees by contract type between March 2016 and March 2017

*Panel A - Temporary contract*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | -0.002 | -0.003 | -0.002 | -0.000 | | |
| | (0.003) | (0.002) | (0.003) | (0.002) | | |
| Initial low-paid prop x Domic | | -0.003 | | -0.001 | | |
| | | (0.010) | | (0.010) | | |
| Change in log avg wage | | | | | -0.038 | -0.008 |
| | | | | | (0.060) | (0.046) |
| Change in log avg wage x Domic | | | | | | -0.129 |
| | | | | | | (0.167) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Mean of dep. var.: | | | | | | |
| All firms | -0.002 | | | | | |
| Care homes | -0.001 | | | | | |
| Domiciliary care | -0.005 | | | | | |

*Panel B - Bank*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | 0.002 | -0.001 | 0.002 | -0.001 | | |
| | (0.003) | (0.003) | (0.003) | (0.003) | | |
| Initial low-paid prop x Domic | | 0.008 | | 0.011 | | |
| | | (0.006) | | (0.007) | | |
| Change in log avg wage | | | | | 0.037 | -0.024 |
| | | | | | (0.056) | (0.063) |
| Change in log avg wage x Domic | | | | | | 0.193 |
| | | | | | | (0.118) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Mean of dep. var.: | | | | | | |
| All firms | -0.004 | | | | | |
| Care homes | -0.004 | | | | | |
| Domiciliary care | -0.005 | | | | | |

TABLE 4.B5 CONTINUED: EMPLOYMENT CONTRACT EQUATIONS

*Panel C - Agency contract*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Initial low-paid prop | 0.001 | -0.001* | 0.001 | 0.000 | | |
| | (0.002) | (0.001) | (0.002) | (0.002) | | |
| Initial low-paid prop x Domic | | 0.000 | | 0.001 | | |
| | | (0.007) | | (0.008) | | |
| Change in log avg wage | | | | | 0.017 | 0.001 |
| | | | | | (0.040) | (0.027) |
| Change in log avg wage x Domic | | | | | | 0.023 |
| | | | | | | (0.137) |
| Observations | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 | 4,680 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Mean of dep. var.: | | | | | | |
| All firms | -0.002 | | | | | |
| Care homes | -0.000 | | | | | |
| Domiciliary care | -0.009 | | | | | |

**Notes:** The table reports the estimated reduced-form coefficient $\hat{\beta}_3$ from model 4.3 in columns (1)-(4), and the estimated IV coefficient $\hat{\beta}_4$ from model 4.4 in columns (5)-(6), using the change in the share of workers on a given contract as outcome variable. The sample is a balanced panel of adult social care providers active between March 2015 and March 2017. Robust standard errors are reported in parentheses. P-value: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables are the initial proportion female, proportion with nursing qualification, proportion of care assistants, average age (all workers), occupancy rate and local authority district dummies. When data on firm-level covariates is missing, such missing information is controlled for via a set of dummy variables. *Temporary contract*: the worker is employed for a limited duration, normally either on a fixed term contract or for a fixed task, or on a spell of casual or seasonal employment as a "temp". *Bank worker*: the worker is retained by the organisation as a whole, but deployed on a casual or short term basis. *Temporary agency work*: the worker is supplied by an outside employment agency/bureau; this category includes staff employed by NHS professionals, and workers supplied on contract e.g. by outside catering and cleaning companies.
**Source:** NMDS-SC.

TABLE 4.B6: ZERO HOUR CONTRACTS EQUATION, LOW PAY INDUSTRIES (LFS SAMPLE)

Probability of being on a ZHC

| | Retail | | Hospitality | | Social care | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post NLW | 0.001 | 0.002 | 0.0118** | 0.014** | 0.011*** | 0.010*** |
| | (0.002) | (0.002) | (0.006) | (0.006) | (0.003) | (0.003) |
| Observations | 27,058 | 27,058 | 12,446 | 12,446 | 25,191 | 25,191 |
| Controls | No | Yes | No | Yes | No | Yes |
| Pre-NLW mean of dep. var.: | 0.017 | 0.017 | 0.102 | 0.102 | 0.042 | 0.042 |
| | Employment agency | | Cleaning and maintenance | | Leisure, travel and sport | |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| Post NLW | 0.013 | 0.013 | 0.013*** | 0.014*** | 0.024** | 0.025** |
| | (0.013) | (0.013) | (0.004) | (0.004) | (0.011) | (0.010) |
| Observations | 1,701 | 1,701 | 5,729 | 5,729 | 3,541 | 3,541 |
| Controls | No | Yes | No | Yes | No | Yes |
| Pre-NLW mean of dep. var.: | 0.072 | 0.072 | 0.019 | 0.019 | 0.099 | 0.099 |
| | Food processing | | Wholesale of food | | Childcare | |
| | (13) | (14) | (15) | (16) | (17) | (18) |
| Post NLW | 0.011* | 0.013** | 0.003 | 0.004 | 0.006 | 0.006 |
| | (0.006) | (0.006) | (0.005) | (0.005) | (0.006) | (0.006) |
| Observations | 2,885 | 2,885 | 1,915 | 1,915 | 3,246 | 3,246 |
| Controls | No | Yes | No | Yes | No | Yes |
| Pre-NLW mean of dep. var.: | 0.025 | 0.025 | 0.010 | 0.010 | 0.031 | 0.031 |
| | Agriculture | | Security | | Textiles | |
| | (19) | (20) | (21) | (22) | (23) | (24) |
| Post NLW | 0.001 | 0.001 | -0.024 | -0.019 | 0.018** | 0.019** |
| | (0.003) | (0.004) | (0.019) | (0.019) | (0.008) | (0.008) |
| Observations | 3,084 | 3,084 | 1,057 | 1,057 | 996 | 996 |
| Controls | No | Yes | No | Yes | No | Yes |
| Pre-NLW mean of dep. var.: | 0.010 | 0.010 | 0.115 | 0.115 | 0.009 | 0.009 |
| | Hairdressing | | Pooled | | | |
| | (25) | (26) | (27) | (28) | | |
| Post NLW | 0.010* | 0.010** | 0.008*** | 0.010*** | | |
| | (0.005) | (0.005) | (0.001) | (0.001) | | |
| Observations | 2,513 | 2,513 | 91,362 | 91,362 | | |
| Controls | No | Yes | No | Yes | | |
| Pre-NLW mean of dep. var.: | 0.013 | 0.013 | 0.041 | 0.041 | | |

**Notes:** The table reports the estimated reduced-form coefficient $\hat{\beta}_5$ from model 4.5, using different Low Paying Industry samples, as defined in Low Pay Comission (2017). The samples contain 4 pre-NLW quarters (2014-2015 quarter 2 and quarter 4) and 3 post-NLW quarters (2016 quarter 2 and quarter 4, and 2017 quarter 2). Controls include age, education, gender, a dummy for white ethnicity, a dummy for British nationality, a dummy for working in the public sector and twelve regional dummies. Robust standard errors are reported in parentheses. P-value: *** p<0.01, ** p<0.05, * p<0.1.
**Source:** LFS.

## 4.C LSE-CEP Survey of Self-employment and Alternative Work Arrangements: Survey Questionnaire

R1 What is the highest degree or level of school you have completed?

- No qualifications
- Some GCSE/O levels.
- 5 or more GCSE/O levels
- Trade/technical/vocational training
- A levels
- Bachelor's degree
- Master's degree
- Doctorate degree

R2 Are you?

- Male
- Female

R3 What is your age? [ALLOW INTEGER NUMBERS BETWEEN 15 AND 99]

___________

R4 Which region do you usually live in?

- North East
- North West
- Yorkshire and Humberside
- East Midlands
- West Midlands
- Eastern England
- London
- South East
- South West
- Wales
- Scotland
- Northern Ireland

S1 On your main job last week, were you employed by government, by a private company, a nonprofit organization, or were you self-employed or working in the family business? Or were you not working at all last week?

- ○ Employed by government GO TO S2
- ○ Employed by private for-profit company GO TO S2
- ○ Employed by nonprofit organization including tax exempt and charitable organizations GO TO S2
- ○ Self-employed, with or without employees GO TO S3
- ○ Working in the family business GO TO S3
- ○ Only work last week was filling out surveys SCREENS OUT
- ○ Did not have a job last week SCREENS OUT

S2 Many people work in self-employment, on either a part-time or full-time basis, doing things such as working on construction jobs, selling goods or services in their businesses, or working through a digital platform or intermediary, such as Uber, Upwork, Deliveroo or Avon. Last week, were you working or self-employed as an independent contractor, an independent consultant, or freelance worker? That is, someone who obtains customers on their own to provide a product or service.

- ○ Yes
- ○ No

S3 Last week, were you on a zero hours contract? Zero hours contracts are also known as casual contracts or "on call" work. Under such contracts, people agree to be available for work as and when required, but have no guaranteed hours or times of work.

- ○ Yes GO TO QUESTION Q1
- ○ No GO TO QUESTION D1

Q1 In your employment as a zero hours contract or on-call worker last week, did you have more than one employer or contract? Please consider only jobs on zero hours contracts or on-call jobs when answering this question.

- ○ Yes
- ○ No

Q2 Last week, did you do any paid work as self-employed or on employment contracts other than zero hours contracts or on-call jobs?

- ○ Yes
- ○ No

Q3 In your zero hours contract or on-call job, how many hours did you work last week? Please, consider only hours you are paid for.
Please enter: ___________ hours last week

Q4 In your zero hours contract or on-call job, how many hours do you work on average in a week? Please, consider only hours you are paid for.
Please enter: ___________ hours on average in a week

Q5 On how many (different) days per week do you usually work?
Please enter: ___________ days per week

Q6 How much did you earn per hour in your zero hours contract or on-call job last week? Please, consider only hours you are paid for.
Please enter earnings: £___________ per hour

Q7 Did you do any hours of unpaid work in your zero hours contract or on-call job last week? E.g. travel time from one customer to another.

- ○ Yes
- ○ No

IF Q7 = Yes
Q7a How many hours of unpaid work did you do in your zero hours contract or on-call job last week?
Please enter: ___________ hours of unpaid work last week

Q8 Would you have preferred to work more or fewer hours last week in your zero hours contract or on-call job at that wage rate? Or were you satisfied with the number of hours you worked?

- ○ More hours last week
- ○ Fewer hours last week
- ○ Satisfied with number of hours

IF Q8 = More hours last week
Q8a Why were you not able to work more last week?

- ○ I am not qualified for the available work
- ○ There isn't enough available work
- ○ I have domestic commitments that prevent me from working more
- ○ I am ill or disabled
- ○ Other

IF Q8 = Fewer hours last week
Q8b Why would you want to work fewer hours?

- ○ I am a student
- ○ I am ill or disabled and do not feel I can take on more hours
- ○ I have domestic commitments that prevent me from working more
- ○ I want to spend more time on leisure or other unpaid activities
- ○ I want to do other types of work
- ○ Other

Q9 Would you have preferred to work a pattern of more regular hours last week on your zero hours contract or on-call job at that wage rate? Or were you satisfied with the pattern of hours you worked?

- ○ More regular hours last week
- ○ Less regular hours last week
- ○ Satisfied with pattern of hours

Q10 How satisfied are you with working on a zero hours contract or on-call job?

- ○ Very satisfied
- ○ Satisfied
- ○ Neither satisfied not dissatisfied
- ○ Dissatisfied
- ○ Very dissatisfied

Q11 Which of the following are reasons why you work on a zero hours contract or on-call job? Tick all that apply.

- ☐ Could not find employment in a job with a guaranteed number of hours
- ☐ Pay is better than other available jobs
- ☐ To complement pay from other jobs
- ☐ To earn money while going to school
- ☐ Gives me flexibility to perform other activities
- ☐ Other

Q11a Which is the most important reason why you work on a zero hours contract or on-call job?

- ○ Could not find employment in a job with a guaranteed number of hours
- ○ Pay is better than other available jobs

- ○ To complement pay from other jobs
- ○ To earn money while going to school
- ○ Gives me flexibility to perform other activities
- ○ Other

IF Q11a = Could not find employment in a job with a guaranteed number of hours
Q11b Please indicate which of the following reasons contributed to you not finding employment in a job with a guaranteed number of hours:

- ○ Lack of jobs near where I live
- ○ I faced discrimination
- ○ I am overqualified for the available jobs
- ○ I am underqualified for the available jobs
- ○ Other

Q12 For how long have you been working on a zero hours contract or on-call job?

- ○ Less than one month
- ○ 1 – 6 months
- ○ 7 – 12 months
- ○ 1 – 2 years
- ○ 3 – 4 years
- ○ 5 years or more

Q13 How much longer do you expect to remain in your zero hours contract or on-call job?

- ○ Less than one month
- ○ 1 – 6 months
- ○ 7 – 12 months
- ○ One year or more

Q14 Have you received any work-related training in the last year?

- ○ Yes SKIP TO Q14a
- ○ No SKIP TO Q14c

Q14a What type of training? (Mark all that apply) [LIST IN RANDOM ORDER, BUT OTHER IS LAST]

- ☐ Technical or technology training
- ☐ Quality training
- ☐ Skills training
- ☐ Continuing education
- ☐ Professional training and legal training
- ☐ Managerial training
- ☐ Safety training
- ☐ Other (please specify: ___________)

Q14b Who paid for the cost of the training?

- ○ Me or a family member
- ○ A contractor or customer
- ○ My employer
- ○ Someone else
- ○ No one, it was free

Q14c What type of training would you find most useful to improve your job prospects? (Mark all that apply) [LIST IN RANDOM ORDER, BUT OTHER IS LAST]

- ☐ Technical or technology training
- ☐ Quality training
- ☐ Skills training
- ☐ Continuing education
- ☐ Professional training and legal training
- ☐ Managerial training
- ☐ Safety training
- ☐ Other (please specify: ___________)

Q15 In your job on a zero hours contract or on-call job, what kind of work do you do, that is, what is your occupation? (For example: plumber, typist, farmer)
Please enter your occupation: ___________

Q15a What are your usual activities or duties at this job? (For example: typing, keeping account books, filing, selling cars, operating printing press, laying brick)
Please enter your usual activities or duties: ___________

Q15b What kind of business or industry are you in at this job?

- ○ (A) Agriculture, Forestry and Fishing
- ○ (B) Mining and Quarrying
- ○ (C) Manufacturing
- ○ (D) Electricity, Gas, Steam and Air Conditioning Supply
- ○ (E) Water Supply, Sewerage, Waste Management and Remediation Activities
- ○ (F) Construction
- ○ (G) Wholesale and Retail Trade, Repair of Motor Vehicles and Motorcycles
- ○ (H) Transportation and Storage
- ○ (I) Accommodation and Food Service Activities
- ○ (J) Information and Communication
- ○ (K) Financial and Insurance Activities
- ○ (L) Real Estate Activities
- ○ (M) Professional, Scientific and Technical Activities
- ○ (N) Administrative and Support Service Activities
- ○ (O) Public Administration and Defense, Compulsory Social Security
- ○ (P) Education
- ○ (Q) Human Health and Social Work Activities
- ○ (R) Arts, Entertainment and Recreation
- ○ (S) Other Service Activities
- ○ (T) Activities of Households as Employers of Domestic Personnel, Undifferentiated Goods and Services Producing Activities of Households for Own Use
- ○ (U) Activities of Extraterritorial Organizations and Bodies
- ○ Other (please specify: ___________)

Q15c In your zero hours contract or on-call job, what is the main company you work for?
Please specify name: ___________

D1 Which country were you born in?
Please specify: ___________

D2 What is your nationality?
Please specify: ___________

D3 Which category or categories below best describe your ethnic group? (Mark all that apply)

☐ White

☐ Mixed / Multiple ethnic group

☐ Asian / Asian British

☐ Black / African / Caribbean / Black British

☐ Chinese

☐ Arab

☐ Other (please specify: ___________)

D4 How many years of working experience have you got?

○ Less than one year

○ 1 – 3 years

○ 3 – 5 years

○ 5 years or more

D5 Are you now married, widowed, divorced, separated or never married?

○ Married

○ Widowed

○ Divorced

○ Separated

○ Never Married

○ Other (please specify: ___________)

D6 How many children do you have?

○ 0

○ 1

○ 2

○ 3 or more

D7 Which category represents your total individual income (before taxes) during the past 12 months? This should include money from all jobs, net income from a business or farm, and any rent, pensions, dividends, interest, social security payments or other money income you received.

○ Less than £5,000

- ○ £5,000 to 9,999
- ○ £10,000 to 19,999
- ○ £20,000 to 39,999
- ○ £40,000 to 69,999
- ○ £70,000 or more

D8 Which category represents total income (before taxes) of your household during the past 12 months? This should include money from all jobs, net income from a business or farm, and any rent, pensions, dividends, interest, social security payments or other money income that all members of your household received, including you.

- ○ Less than £5,000
- ○ £5,000 to 9,999
- ○ £10,000 to 19,999
- ○ £20,000 to 39,999
- ○ £40,000 to 69,999
- ○ £70,000 or more

D9 Do you use services such as Uber, TaskRabbit, Airbnb or Deliveroo?

- ○ Yes
- ○ No

D10 Could you tell us how interesting or uninteresting you found the questions in this survey?

- ○ Very interesting
- ○ Interesting
- ○ Neither interesting nor uninteresting
- ○ Uninteresting
- ○ Very uninteresting

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