Short-Time Work and Employment Stability: Evidence from a Policy Change

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Abstract

This paper investigates whether short-time work (STW) programmes achieve their stated goal of being devices intended to preserve jobs and keep workers employed in times of crisis. Our identification strategy exploits a change in the financial incentives provided to employers and employees for the temporary suspension of work contracts or the reduction of working time. We use longitudinal administrative data and estimate difference-in-differences regressions and instrumental variable bivariate probit models with endogenous covariates, which try to take account of the potential endogeneity of participation in STW. Our results suggest that discretionary policy changes in the incentives of STW schemes can be effective in the short run but they lose their ability when the decline in demand and the lack of work are more permanent.

1. Introduction

Short-time work (STW) schemes are intended to preserve jobs at firms experiencing temporarily low demand by encouraging work-sharing, while also providing income support to workers whose hours are reduced due to a shortened work week or temporary layoffs. A crucial aspect of STW programmes is that the contract of an employee with the firm is maintained during the period of reduced hours or the suspension of work. Although scarcely studied in the past, with few empirical studies devoted to this issue, there was a relative upsurge of interest in STW during the last recession. This was the result of several changes.

First, most governments in OECD countries took specific measures in response to the crisis to promote its use, by weakening eligibility and conditionality requirements and increasing generosity, while others

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established new programmes (Eurofound 2010; Panteia 2012). Second, take-up rates increased substantially at the beginning of the recession, although differences across countries were large (Hijzen and Martin 2013). Third, the resources devoted to STW schemes were substantial: expenditure amounted to some 5000 million euros in Germany, 5500 million euros in Italy and nearly 6000 million euros in Japan in 2009, that is between 0.1 per cent and 0.3 per cent of GDP in these countries (Boeri and Bruecker 2011). And finally, some analysts and policymakers attributed the good performance of some national labour markets (for instance, the German one) to the role played by STW schemes to prevent the adjustment along the extensive margin (employment) and to favour instead the adjustment along the intensive margin (hours of work per employee).[1]

Therefore, given the size and prominence of STW schemes during crises in general and during the last recession in particular, it would be important to evaluate their impact. Both country-level and firm-level approaches have been used in the otherwise scarce empirical literature.[2] Macro estimates are designed to exploit the country and time variation in take-up rates to analyse the quantitative impacts of STW schemes on labour market outcomes (Abraham and Houseman 1994; Van Audenrode 1994). Focusing on the last recession, Arpaia *et al.* (2010) and Hijzen and Venn (2011) estimate a model in which the impact of STW is captured with an interaction term between one dummy signalling the 2008–2009 recession and another dummy signalling countries with STW programmes in place. Micro estimates are based on establishment- or firm-level data and exploit the variation between participating and non-participating employers within countries (Calavrezo *et al.* 2009, 2010; Crimmann *et al.* 2012; Dietz *et al.* 2011; Duhautois *et al.* 2009). One shortcoming of most studies is that they do not usually take account of the potential endogeneity of STW schemes: in the case of the macro approach, that STW may be endogenous with respect to the role of macroeconomic conditions that affect both employment and STW but is not captured by the change in output; in the case of the micro approach, that there is a selection problem that arises because participant firms in STW schemes also tend to be less competitive than other firms that can be used as a control group.

However, subsequent works have improved on the econometric analysis. On the macro side, they do this by instrumenting the STW take-up rate: Cahuc and Carcillo (2011) use the permissible reductions in weekly working hours that can be compensated before 2008 and the STW take-up rate in 2007 as instrument, while Boeri and Bruecker (2011) and Hijzen and Martin (2013) instrument using the age of the corresponding national programme. On the micro side, Boeri and Bruecker (2011) and Bellmann *et al.* (2012) use an instrument based on the experience of firms with STW before the crisis. In general, macro studies have found short-time positive impacts of STW on employment during the 2008–2009 recession (although deadweight costs seem to be sizable as well, especially in the medium run), whereas empirical evidence

from micro studies which control for STW endogeneity are not so clear-cut, showing either small positive effects or no effect at all.[3]

We contribute to this literature by using a worker-level approach, examining whether STW programmes (and their changes) achieve their stated goal of being devices intended to preserve jobs and keep workers employed. This is relevant since, as Kruppe and Scholz (2014) point out, when using either country- or firm-level data, one is unable to evaluate the effect of STW on individual labour market biographies, so that it may well be that (even when no employment preserving effect of STW on either the country or the establishment level is found) STW might contribute to prevent individual unemployment. To do so we exploit the fact that in March 2009, about one year after the onset of the Great Recession, the Spanish government decided to increase the financial incentives provided to employers and employees for the temporary suspension of work contracts or the reduction of working time. All this is novel since, as far as we know, no previous study, first, has focused attention on the impact of participation in STW on the subsequent labour market status or trajectories of workers, or, second, has taken advantage of changes implemented in the programmes during the recession in order to evaluate their impact.[4]

This paper uses longitudinal administrative data, defines treatment and control groups before and after the policy change and examines the labour market transitions of both groups. To assess the robustness of our evaluation exercise we consider two alternative control groups; we also define and use a subsample of workers in manufacturing, since this is the industry that contains the majority of STW arrangements. Instrumental variable (IV) recursive bivariate probit models with endogenous covariates are estimated. The dependent variable (the stability of employment) is measured as the probability that an individual who was already employed at the time of the implementation of such an arrangement remains employed with the same employer in the future (one, two and three years after implementation). A key feature of our setting is that participation in STW is instrumented, since single-equation estimates of its effect will be biased if this chance is correlated with unobserved factors that determine both STW eligibility and employment permanence. This allows us to identify the effect of participation on the difference-in-difference between pre- and post-reform performance of treatment and control groups, thereby controlling for unobserved sources of heterogeneity that may create a spurious correlation between treatment status and labour market outcomes.

The structure of the article runs as follows. Section 2 provides a description of the STW regulations in Spain and the changes adopted in 2009. Section 3 presents the data and some descriptive analysis. Section 4 outlines the empirical specifications used to investigate the effects of STW schemes and their changes on individuals' outcomes. Section 5 reports and discusses the results. Finally, Section 6 concludes.

2. Institutional setting

The Spanish STW scheme is the legal instrument used to protect employment in cases of exceptional circumstances by allowing firms to reduce temporarily employees' activities below the legal working time duration or suspend business activities. The rules regulating STW have remained unchanged since 1994 until recently. The financial incentives to engage in and use STW schemes changed substantially in 2009. Needless to say, the regulation applies to permanent workers only. Temporary workers, whose labour contract has a limited and well-defined duration, can be discharged by simply not having their contracts renewed. This is an important caveat, since about a quarter of all employees in Spain work under a contract of this type.

Any action taken by employers concerning workforce adjustment (affecting about 10 per cent of their employees) had to be preceded by a procedure called an 'employment regulation process' (*Expediente de Regulación de Empleo*, ERE).[5] This procedure basically considered three possible courses of action: permanent dismissal of workers, temporary layoff of workers and temporary reduction of working time for some or all employees, the latter two belonging to the same category (STW). To initiate an ERE, the employer had to offer a minimum 30-day period of consultation and negotiation with the workers' representatives, and at the same time it had to notify the labour authority, providing the necessary information to justify the measures proposed. In the case of firms with over 50 workers, a 'social plan' was to be added to the initial documents provided. The ERE needed administrative authorisation. If it was rejected, it was sent back to the firm with the recommendation that an agreement be reached with the workers. This procedure was common to all types of ERE; the only difference was that, in the case of STW, the time allowed to the labour market authorities to decide on an ERE was half of that allowed in the case of permanent dismissals. This also was the case for firms with fewer than 50 employees and when the ERE involved less than 5 per cent of the current workforce of the establishment.

The workers dismissed, temporarily laid off or under short-time working were entitled to receive unemployment benefits, provided that they had contributed to the unemployment insurance system long enough. The only proviso added in the case of short-time workers was that the benefit was proportional to the amount of working time reduction; thus, participants maintained their wages in proportion to the working time cut agreed in the ERE, receiving their (reduced) wages and the partial-unemployment benefit. In addition, a threshold for working time reduction of one-third of normal hours was established to be entitled to receive benefits. These benefits were against their total benefits: partial-unemployment benefits received during STW were 'discounted' from the total benefits accumulated by workers and not reinstated later, so workers affected by an STW scheme suffered a loss in unemployment benefits rights.[6]

As said above, a major modification of the regulation (due to its potential impact on the behaviour of employers and workers) took place in March

2009 (Royal Decree Law 2/2009, from March 6), later maintained with Law 27/2009 (from December 30) and with the labour market reforms of 2010 and 2012. In March 2009, the government decided to increase the financial incentives provided for STW. In particular, two related measures were adopted benefiting those employers and workers involved in STW schemes agreed since the passing of the RDL-2009.[7]

First, firms would benefit from a 50 per cent bonus in the employers' social security contributions in the case of STW (80 per cent if training activities are carried out), with a limit of 240 days per worker and the obligation to keep the workers involved employed for a minimum of one year after the conclusion of the STW programme. And second, workers affected by an STW scheme would not suffer from any loss in unemployment benefits rights, with some limits, since there would be a partial reinstatement of their benefits: up to 120 days if the worker is subsequently dismissed, and up to 90 days if he/she is involved again in an STW scheme and does not have enough unemployment benefits to use.

In terms of use, the Spanish STW take-up rate changed substantially after the onset of the last recession. It increased from 0.2 per cent in 2007 and 0.6 per cent in 2008 to 3.1 per cent in 2009, declined to about 1.7 per cent in 2010–2011 and rose again to 2.7 per cent in 2012 after the economy relapsed in recession.[8] For the sake of comparison, take-ups amounted to 7 per cent in Belgium, 4–5 per cent in Germany and Japan, 1–2 per cent in Austria, France, Italy, Czech Republic, the Netherlands and Slovakia, and less than 1 per cent in other countries in 2009 (Hijzen and Martin 2013). Moreover, our own estimates based on the administrative database we use in this paper suggest that expenditure amounted to nearly 600 million euros in 2009 and 1000 million in 2012, which represents about 0.05 per cent and 0.1 per cent of GDP, respectively.[9]

3. Data and descriptive statistics

Data

We use data from the Continuous Sample of Working Life (*Muestra Continua de Vidas Laborales*; hereinafter, MCVL), an administrative data set built upon the computerized records of the Spanish Social Security and the Continuous Municipal Register and the tax data of the National Revenue Agency. This database is formed by a 4 per cent sample (selected by means of a simple random sampling system) representative of the reference population which includes both workers who are registered with the Social Security as working and recipients of contributory and non-contributory pensions and unemployment benefits. The resulting database thus provides annual information on more than one million people who have had any kind of relationship with the Social Security in a given year.[10]

This administrative data set provides information on individual, job and employer attributes as well as on the unemployment benefits received by

each worker in the event that he/she was separated from his/her job and eligible to receive them; in particular, whether each individual was receiving unemployment benefits when out of work, the types of benefit received (unemployment insurance, UI, or unemployment assistance, UA) and the number of days of benefit receipt. This also applies to spells of benefit receipt while in STW as a consequence of either suspension of contract or reduction of working time, as the Spanish unemployment compensation system has a specific unemployment benefit for those workers participating in STW and the MCVL contains a variable with a category which allows their identification (although it does not enable one to distinguish between temporary layoff and reduction of working hours).

The MCVL has a longitudinal design and the sample is refreshed with new sample members, remaining representative of the population in each edition. This means that an individual who is present in an edition of the sample and subsequently remains registered with the Social Security stays as a sample member. Therefore, its longitudinal nature makes it possible to know the subsequent labour market status of a given individual after an STW scheme has been adopted in a company or a job separation has taken place. Moreover, since each establishment owns an identification code in each province in which it operates, the database contains an anonymous identification number for the employer associated with every single spell of employment. For our purposes, this feature of the MCVL is extremely important, since it allows one to identify STW participants and non-participants within the same firm and to know whether workers remain with the same employer in the future. Both aspects are essential for the empirical analysis we pursue: the first one for the design of the comparison group; the second one for the construction of the dependent variable measuring the labour outcome of interest.

In sum, this database presents at least three advantages for the analysis of STW schemes and their impact on the labour market outcomes of workers: first, the information is available, accurate and detailed on the jobs held by individuals and on the spells of receipt of unemployment benefits; second, it makes it possible to identify the participation in STW not only of workers but also of employers; and third, it allows us to know the subsequent labour market status of individuals, distinguishing between (participants and non-participants) workers who remain in the same firm, those who are employed in a new job with a different firm and those who are jobless.[11]

Main Variables

Our aim is to estimate the effects of participation in an STW scheme on the probability that an individual who was already employed at the time of the implementation of such an arrangement remains employed within the same firm in the future and to evaluate the impact of the increased financial incentives. The outcome variable is, therefore, the labour market status of the individual several quarters/years after the 'treatment' has taken place, while the measure of exposure to an STW scheme is a variable indicating

whether or not the worker was involved in an arrangement any time during a given quarter. Furthermore, as we want to quantify the causal effect of the policy change that took place in March 2009, the point is to know not only whether workers involved in STW schemes after the passing of the new financial incentives are more or less likely to remain employed within the same firm when compared with participants before the change but also whether this happens when participating workers are compared to otherwise similar non-participating workers. This will allow us to gauge whether the change in the incentives had some differential positive effect on the permanence of those workers in their jobs.

We take advantage of the characteristics of the database and of the STW programme implementation to construct treatment and control groups. We explore the existence of data for participants (treated individuals) and non-participants (controls) in STW schemes for the pre- and post-2009 periods. The available sample has pre- and post-reform treatment observations and pre- and post-reform control observations, and we will use this repeated cross-section feature to implement difference-in-differences class estimators.

In order to carry out this analysis, we define two observation windows or periods of entry. The first one refers to a period before the passing of the Royal Decree Law 2/2009 (RDL-2009) in March that year. The individuals starting a spell of receipt of STW in this period form the *pre-reform treatment group*. In fact, we select workers who began STW spells anytime during the fourth quarter of 2008 (2008Q4): individuals can start a benefit anytime between October 1, 2008 and December 31, 2008, while their successive entries into the unemployment compensation system (UCS) or into other jobs may occur until December 31, 2013 (the last day available in our data set). The second window is located after the aforementioned RDL-2009. We select individuals starting an STW spell during the second quarter of 2009 (2009Q2), between April 1, 2009 and June 30, 2009, while their successive entries into the UCS or into other jobs may occur until 31st December 2013. They belong to the *post-reform treatment group*.

These 'starting periods' were chosen due to two reasons. First, they are very close in time, corresponding to moments just before and after the reform took place; in fact, both samples are separated out by one quarter only. And second, both quarters belong to a rather severe recession beginning after the summer of 2008, so the conditions faced by workers in both periods are characterized by a strong declining economic activity; thus, this choice avoids the comparison of periods with quite different macroeconomic and labour market conditions. The latter advised against going back further to, say, the beginning of 2008 or even 2007, which in addition implied that the number of observations dropped substantially.

As regards the controls, this is a group of employed individuals who are not involved in STW schemes. We also analyse a group with these characteristics before and after the RDL-2009: these are the *pre-* and *post-reform control groups*. In fact, we consider two different comparison groups: the first one (control A) comprises all employed individuals not participating in STW

programmes; the second one (control B) narrows the definition of the control group by including only non-participating workers in firms that have made use of STW schemes.

We have limited our sample to wage and salary individuals aged 16–59 years who work in the non-agriculture private economy (the individuals are registered with the General System of Social Security in their current job), hold an open-ended contract, work full-time and have job tenure for longer than one year. The purpose is to exclude those workers with a marginal attachment to firms who cannot be potentially (and legally) eligible in the event that an employer decided to run an STW arrangement.

Descriptive Evidence

Table 1 provides summary statistics, separately, for the sample of treated (participants) and non-treated workers (non-participants) in pre- and post-reform periods and for the two comparison groups. The control variables are individual, job and employer attributes (gender, age, nationality, region of residence, job category, job tenure, industry affiliation and size) measured when implementation of the programme occurs (2008Q4 for the treatment and control groups in the pre-reform period and 2009Q2 for the treatment and control groups in the post-reform period).

When compared either with other employees (control A) or with non-participating workers in firms that have run STW schemes (control B), certain categories of workers, jobs and firms are over-represented among STW participants. These are male, Spanish and older workers, long-tenured employees, and jobs in blue-collar occupations (except the least skilled), in medium-sized and large employers, in manufacturing industries and in the regions of Catalonia and the Basque Country.[12] At the same time, STW participants exhibit a certain change between the pre- and post-reform periods, with a higher presence of older workers, white-collar jobs and small firms (with less than 50 employees) in sectors other than 'Manufacture of machinery and equipment' in the latter as compared to the former.[13] Whether this alteration is related to the change in the STW regulation is difficult to grasp. One cannot rule out the varying impact of the deepening of the downturn on the characteristics of workers and jobs.

In the empirical analysis, for each group of workers defined according to their treatment status (treated and non-treated) and period regime (before and after the RDL-2009), period one covers the quarter in which treatment (participation in STW) takes place, while period two corresponds to one, two and three years later. In period two, outcome is measured by the individual's permanence in employment with the same firm. This means that outcomes are observed in 2009Q4, 2010Q4 and 2011Q4 for the pre-reform sample and in 2010Q2, 2011Q2 and 2012Q2 for the post-reform sample.

Table 2 presents descriptive evidence on labour market outcomes by treatment and period regime after one, two and three years of treatment. The first rows show the mean proportion of employees who are still working

TABLE 1
Summary Statistics (Means): Treatment and Control A and B Groups in Pre- and Post-Policy Change Periods; Spain (MCVL, 2008–2013)

	Pre-reform period					Post-reform period				
	Contr. A	*Contr. B*	*Treatment*	*Diff. A-Tr*	*Diff. B-Tr*	*Contr. A*	*Contr. B*	*Treatment*	*Diff. A-Tr*	*Diff. B-Tr*
Gender (men)	0.664	0.632	0.803	−0.139	−0.171	0.661	0.644	0.799	−0.138	−0.155
Nationality (Spanish)	0.940	0.947	0.981	−0.041	−0.034	0.939	0.951	0.973	−0.034	−0.022
Age groups										
<30	0.385	0.377	0.181	0.204	0.196	0.380	0.347	0.123	0.257	0.224
30–39	0.350	0.351	0.397	−0.047	−0.046	0.353	0.364	0.346	0.007	0.018
40–49	0.195	0.195	0.257	−0.062	−0.062	0.197	0.210	0.285	−0.088	−0.075
≥50	0.070	0.077	0.165	−0.095	−0.088	0.070	0.079	0.246	−0.176	−0.167
Job category										
WCHS	0.130	0.088	0.035	0.095	0.053	0.133	0.093	0.063	0.070	0.030
WCMS	0.127	0.101	0.055	0.072	0.046	0.128	0.101	0.091	0.037	0.010
WCLS	0.302	0.328	0.063	0.239	0.265	0.303	0.316	0.147	0.156	0.169
BCHS	0.245	0.238	0.345	−0.100	−0.107	0.243	0.243	0.343	−0.100	−0.100
BCMS	0.115	0.150	0.457	−0.342	−0.307	0.113	0.159	0.298	−0.185	−0.139
BCLS	0.080	0.094	0.045	0.035	0.049	0.080	0.088	0.056	0.024	0.032
Industry										
Manufacture of food products, textiles, wood and paper	0.089	0.085	0.040	0.049	0.045	0.089	0.081	0.113	−0.024	−0.032
Extraction, energy, chemicals and manufacture of metals	0.108	0.103	0.326	−0.218	−0.223	0.105	0.129	0.378	−0.273	−0.249
Manufacture of machinery, electrical and electronic or transport equipment	0.076	0.121	0.529	−0.453	−0.408	0.074	0.137	0.333	−0.259	−0.196
Construction	0.049	0.035	0.007	0.042	0.028	0.048	0.036	0.016	0.032	0.020
Trade and tourism	0.310	0.293	0.021	0.289	0.272	0.311	0.277	0.081	0.230	0.196
Transport	0.154	0.127	0.046	0.108	0.081	0.157	0.112	0.041	0.116	0.071
Rest of services	0.214	0.236	0.030	0.184	0.206	0.216	0.228	0.038	0.178	0.190
Region										
Balearic Is., Canary Is., Andalusia, Ceuta, Melilla & Murcia	0.199	0.137	0.034	0.165	0.103	0.199	0.132	0.065	0.134	0.067
Aragon, Navarre & La Rioja	0.056	0.037	0.126	−0.070	−0.089	0.056	0.038	0.105	−0.049	−0.067

(*Continued*)

TABLE 1
Continued

| | Pre-reform period | | | | | Post-reform period | | | | |
	Contr. A	Contr. B	Treatment	Diff. A-Tr	Diff. B-Tr	Contr. A	Contr. B	Treatment	Diff. A-Tr	Diff. B-Tr
Asturias, Galicia & Cantabria	0.089	0.054	0.073	0.016	−0.019	0.090	0.060	0.088	0.002	−0.028
Extremadura, Castile-La Mancha & Castile–Leon	0.101	0.059	0.123	−0.022	−0.064	0.101	0.051	0.078	0.023	−0.027
Catalonia	0.206	0.312	0.310	−0.104	0.002	0.204	0.315	0.322	−0.118	−0.007
Valencia	0.099	0.116	0.137	−0.038	−0.021	0.096	0.121	0.121	−0.025	0.000
Madrid	0.194	0.262	0.072	0.122	0.190	0.197	0.239	0.040	0.157	0.199
Basque Country	0.056	0.022	0.124	−0.068	−0.102	0.056	0.043	0.181	−0.125	−0.138
Firm size										
1–10 workers	0.282	0.247	0.024	0.258	0.223	0.277	0.24	0.084	0.193	0.156
10–19 workers	0.125	0.110	0.001	0.124	0.109	0.124	0.108	0.001	0.123	0.107
20–49 workers	0.156	0.149	0.025	0.131	0.124	0.156	0.148	0.044	0.112	0.104
50–249 workers	0.217	0.225	0.703	−0.486	−0.478	0.218	0.236	0.595	−0.377	−0.359
250 +workers	0.220	0.269	0.247	−0.027	0.022	0.225	0.268	0.276	−0.051	−0.008
Tenure										
1 to <3 years	0.089	0.112	0.079	0.010	0.033	0.079	0.098	0.060	0.019	0.038
3 to <6 years	0.149	0.176	0.110	0.039	0.066	0.158	0.179	0.146	0.012	0.033
6 to <10 years	0.349	0.380	0.382	−0.033	−0.002	0.359	0.370	0.340	0.019	0.03
10 to <20 years	0.336	0.293	0.403	−0.067	−0.110	0.330	0.309	0.385	−0.055	−0.076
>20 years	0.077	0.039	0.026	0.051	0.013	0.075	0.043	0.069	0.006	−0.026
Worker's receipt of UB (yes)	0.491	0.813	0.690	−0.199	0.123	0.492	0.813	0.588	−0.096	0.225
Firm's previous use of STW (yes)	0.188	0.946	0.479	−0.291	0.467	0.202	0.908	0.280	−0.078	0.628
Individuals (sample)	188 954	14 577	1335			189 927	15 065	2377		

Note: 'Job category' is classified as white-collar high-skilled occupations, WCHS (managers, workers with university degree, technical engineers and qualified assistants); white-collar medium-skilled occupations, WCMS (clerical and workshop heads and assistants); white-collar low-skilled occupations, WCLS (administrative officials and other clerical workers); blue-collar high-skilled occupations, BCHS (first and second class officials); blue-collar medium-skilled occupations, BCMS (third class officials and specialists) and blue-collar low-skilled occupations, BCLS (labourers).

TABLE 2

Labour Market Performance of Individuals by Treatment Status and Control Groups (A and B) for Pre- and Post-Policy Change Periods: Proportion of Individuals Who Remain Employed with the Same Firm Several Years after Implementation of STW. Full Sample. Spain (MCVL, 2008–2013)

	Control A			Control B		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Pre-reform period						
Treatment	82.7	75.7	71.7	82.7	75.7	71.7
Control	86.9	79.5	72.8	85.6	77.6	70.9
Post-reform period						
Treatment	81.7	70.7	62.7	81.7	70.7	62.7
Control	87.7	80.2	73.3	86.8	78.4	71.5
Diff. treatment – control						
Pre-reform	−4.2	−3.8	−1.1	−2.9	−1.9	0.8
Post-reform	−6.0	−9.5	−10.6	−5.1	−7.7	−8.8
Diff. post-reform – pre-reform						
Treatment	−1.0	−5.0	−9.0	−1.0	−5.0	−9.0
Control	0.8	0.7	0.5	1.2	0.8	0.6
Difference-in-difference						
DID	−1.8	−5.7	−9.5	−2.2	−5.8	−9.6

with the same employer distinguishing between treatment and control groups for pre- and post-reform periods. The next rows offer the differences in that indicator across groups of workers and periods. Finally, the bottom row displays the difference-in-difference by treatment status and period. This analysis is only descriptive and does not consider the potential endogeneity of the STW variable, which will be taken into account in the econometric estimation.

The data show that stability is lower in the treatment group than in both control groups after one year but the evolution of the difference varies between periods: it increases (in absolute terms) over time for the post-reform groups and declines (approaching zero) for the pre-reform groups. This diverging behaviour is due to the fact that, although the proportion of workers remaining in the same firm diminishes for all groups, it falls more in the case of the post-reform treated group. Therefore, for the treatment group, the indicator is always lower in the post-reform than in the pre-reform period and the difference tends to increase (in absolute terms) with time (from 1 pp (percentage point) one year later to 9 pp three years later).

Furthermore, the proportions of individuals still working with the same employer in the pre-reform control groups are quite similar to the corresponding ones in the post-reform control groups: differences amount to about 1 pp or less. This might be taken as evidence that the 'common trend' assumption appears to be satisfied reasonably well. This assumption is crucial for the validity of our results. In our setting, the common trend entails the probability of remaining employed with the same firm evolving equally for participants and non-participants in STW schemes in the absence of a

change in the rules or the financial incentives of the programme. Therefore, this evidence leads us to believe that this assumption is likely to hold.

Finally, since the difference in the indicator across policy change periods is negative (and increasing) for the treatment group and close to zero for the control groups, the difference-in-difference (DID) is negative (and increasing), ranging from about 2 pp one year later to nearly 10 pp three years later. This also holds when we use the manufacturing subsample, which suggests that the pattern of the differences for the treatment and control groups in relation to being employed in the same firm is similar.

4. Econometric models: specifications

Estimation Assuming Exogenous Exposure: Difference-in-Differences

Our purpose is to evaluate the effects of policy change (increased financial incentives) on some labour market outcomes of individuals, namely the probability of remaining employed with the same firm. As a starting point, let us consider the following single-equation model:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 R_i + v_i \tag{1}$$

for $i = 1, 2, \ldots, n$ participants in STW schemes. In this specification, R_i is a dummy variable for the policy change adopted in March 2009: it takes the value 1 for the population of workers assigned to STW schemes after the change took place (in 2009Q2) and 0 for those before the change (in 2008Q4). X_i represents a vector of exogenous control variables, which are measured at the moment of implementation of the STW arrangement, that is 2008Q4 in the pre-reform period and 2009Q2 in the post-reform period. Y_i is the variable measuring the labour outcome of interest (whether the worker is still employed within the same firm in the future), so that it takes value 1 if the individual is working with the same employer after one year and value 0 otherwise (outcomes are observed in 2009Q4 for the pre-reform sample and in 2010Q2 for the post-reform sample). As we repeat the same set of estimations for a time span of two and three years, Y_i takes value 1 if the worker remains in the same firm after two and three years, respectively, and value 0 otherwise (outcomes are thus observed in 2010Q4 and 2011Q4, respectively, for the pre-reform sample and in 2011Q2 and 2012Q2, respectively, for the post-reform sample). Finally, v_i is a measure of unobservables associated with the outcome variable Y_i and assumed to be uncorrelated with the policy change variable, that is we require that $E(v_i|R_i, X_i) = 0$. Therefore, the comparison summarized by the parameter β_2 (and estimated on a sample of STW participants before and after the change in the financial incentives) would identify the impact of the policy change on the stability of employment.

Since we are interested in quantifying the differentiated effect of the increased financial incentives established in March 2009 and the data-base we use has information not only on STW participants but also on

non-participants, a DID research design can be set out. The main idea of this design in our setting is to compare the labour market outcomes of four distinct groups: STW participants and non-participants before and after the policy change has taken place, respectively. Therefore, the previous equation has to be modified, adopting the following form:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 R_i + \beta_3 D_i + \beta_4 R_i D_i + v_i \tag{2}$$

for $i = 1, 2, \ldots, n$ participants and non-participants in STW schemes. In this specification, R_i is the dummy variable for the policy change and D_i is a dummy variable indicating exposure to an STW scheme (it takes the value 1 if the worker was involved in a programme in the corresponding 'starting period' and 0 otherwise). In this case, the estimate β_0 is the mean probability of non-participant workers admitted under pre-2009 rules remaining employed with the same firm; the estimate $\beta_0 + \beta_2$ is the mean probability of non-participant workers under post-2009 rules; the estimate $\beta_0 + \beta_3$ is the mean probability of participant workers; and $\beta_0 + \beta_2 + \beta_3 + \beta_4$ is the mean outcome of participant workers under post-2009 rules. Therefore, β_4 would capture the difference-in-differences effect of the policy change. In other words, this latter parameter identifies the causal effect under the 'common trend' assumption, namely that any time-varying unobservables have the same effect on treated and non-treated workers. The interpretation of this parameter is that it measures the higher or lower probability of remaining employed with the same company in the period after the 2009 change, relative to non-participating (full-time permanent) workers and relative to the period before the change.

Although the estimation of probit models to calculate DID estimates for binary outcomes has long been a standard methodology, several concerns have been raised about the appropriateness of interpreting β_4 as the true effect of the treatment in a DID framework (Ai and Norton 2003; Puhani 2012). There are some proposals to correct it, but none of them are straightforward (Athey and Imbens 2006; Blundell and Costa-Dias 2009; Karaca-Mandic *et al.* 2012). In our case, non-linear parametric approximations to predict the components of the conditional-on-X effects have been used, following Ai and Norton (2003) and Lechner (2011).

Estimation Controlling for Endogenous Exposure: Instrumental Variables Estimates

The previous approach has an evident key limitation, since it assumes that participation in an STW scheme is exogenous once personal and employer control variables are included in the model, so no other factors simultaneously determine both participation in a STW and labour market outcomes. However, participants may differ from non-participants in many unmeasured ways. In fact, when firms decide to start the procedure to carry out an STW arrangement, they have to communicate to (and bargain with)

the workers' representatives some key issues such as the number and types of jobs affected by the proposed measures, the details of these measures and the criteria used to designate the workers involved. Therefore, employers (and workers' representatives) choose which workers are to be included in the STW scheme.

Given that participation is not random, one must necessarily pay attention to this selection bias, since it may seriously affect the results of the estimation, leading to the over- or underestimation of β_4. In order to avoid this bias, we set out a model with two equations, extending equation (2) to include the possibility of endogenous regressors. Therefore, we employ an instrumental variable approach, which involves estimating an equation for STW participation, in addition to the equation that indicates the labour market outcomes of individuals:

$$D_i = \delta_0 + \delta_1 X_i + \delta_2 Z_i + \mu_i \qquad (3)$$

where X_i represents an overlapping vector of exogenous control variables that also affect the outcome Y_i in equation (2); Z_i is a non-overlapping vector of variables that are correlated with D_i but not Y_i; and μ_i is the error term (a vector of unobservables associated with D_i but also potentially correlated with ν_i in equation (2)).

The aim of forming a consistent estimator for β_3 and β_4 in equation (2) becomes manageable if one is able to construct instruments for D_i. A set of variables Z_i would be valid instruments for D_i if they were strongly correlated with participation in STW schemes ($\mathrm{cov}[Z_i,D_i] \neq 0$) but uncorrelated or minimally associated with the error term of the employment outcome equation ($\mathrm{cov}[Z_i,\nu_i] = 0$) (see Greene 2008; Wooldridge 2006). In the empirical setting, we have chosen two variables that are hypothesized to affect participation in STW schemes but not the labour market outcomes, as we will see below.

The set of equations (2) and (3) can be estimated simultaneously using a bivariate probit model for two binary outcomes. The model is motivated using a continuous underlying latent variable specification for both the participation in STW and the employment outcome, whose discrete realizations are given as above by D_i and Y_i, respectively. In both equations, to allow for the possibility that the unobserved determinants of exposure to an STW scheme and the unobserved determinants of individuals' labour market outcomes are correlated, the disturbance terms ν_i and μ_i are assumed to have bivariate normal distribution, with $E[\nu_i] = E[\mu_i] = 0$, $\mathrm{var}[\nu_i] = \mathrm{var}[\mu_i] = 1$ and $\mathrm{cov}[\nu_i,\mu_i] = \rho$. Because the events are dichotomous, there are four possible states of the world: ($Y_i = 1$, $D_i = 1$), ($Y_i = 1, D_i = 0$), ($Y_i = 0, D_i = 1$) and ($Y_i = 0, D_i = 0$). The likelihood function is constructed as the product of the four mutually exclusive outcomes, being a bivariate probit.

The recursive bivariate probit model we have sketched above is identified. The identification strategy relies on the fact that we are modelling sequential decisions or events. It can be consistently and efficiently estimated via the

method of Full Information Maximum Likelihood (see Greene and Henster 2010; Jones 2007).

5. Empirical analysis

This section reports the estimate results of the models presented above. All estimations (save the first one) have been carried out using two control groups (all employed individuals not participating in STW programmes and non-participating workers in firms that have made use of STW) and two samples (full sample and subsample of manufacturing workers). The manufacturing results have been omitted in the tables but they are reported in the text as a robustness check (available upon request). After that, we discuss the results and give them an economic interpretation.

Results

First of all, we provide the results from various probit models based on the selection on observables for the probability of remaining in a job in the future (after one, two and three years). We first estimate equation (1) using information only on participating workers in STW (treated individuals). The covariates consist of an indicator variable for policy change (increased financial incentives) and a set of explanatory variables (personal, job and employer attributes such as gender, age, nationality, region, job category, job tenure, industry affiliation and firm size). The estimates of R (not shown) are not significant for the first year and negative and significant for the second and third year after treatment. They imply that the likelihood of being still working with the same employer for individuals affected by the policy change is not significantly different after one and two years and about 5 pp lower after three years, when compared with individuals not affected by the policy change. This result is similar if we consider the manufacturing subsample.

Next, Table 3 presents estimate results of probit models of equation (2) when participation in STW is deemed exogenous and the sample of STW participants and non-participants is used. It reports the results from an estimation procedure which controls for R, D, the interaction of these two variables, and the same set of variables as previously. For the sake of brevity, we present only the marginal effects (using the non-linear parametric approximations to predict the components of the conditional-on-X effects – on the mean values) of those variables related to the policy change and the participation in STW, being the whole set of results available upon request.[14] Our main interest focuses on the estimate of the parameter β_4 from equation (2), that is the interaction term of being involved in an STW after the change in financial incentives, which is intended to capture the DID effect of the policy change.

We find a non-significant impact of participation on STW programmes after the policy change after one year and a negative impact after two and three years. The DID estimates suggest that the probability of remaining employed

TABLE 3

Estimate Results of Probit Models for Equation (2): Marginal Effects and Standard Errors (in Brackets). Full Sample. Spain (MCVL, 2008–2013)

	Control A group			Control B group		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Policy change (*R*)	0.008***	0.007***	0.005***	0.010***	0.005	0.001
	(0.001)	(0.001)	(0.001)	(0.004)	(0.005)	(0.005)
STW particip. (*D*)	−0.043***	−0.060***	−0.055***	−0.048***	−0.075***	−0.080***
	(0.008)	(0.011)	(0.012)	(0.010)	(0.013)	(0.014)
R·D	−0.016	−0.047***	−0.085***	−0.018*	−0.043***	−0.078***
	(0.010)	(0.013)	(0.015)	(0.011)	(0.015)	(0.016)
LR χ^2(28)	17250.64	32429.8	43279.71	1930.04	3463.61	4699.97
Prob > χ^2	0.000	0.000	0.000	0.000	0.000	0.000
Pseudo-R^2	0.059	0.084	0.096	0.071	0.09	0.11
Individuals	382 589	382 589	382 589	33 311	33 311	33 311

Note: The estimated marginal effects (standard errors) of each column come from a different regression on equation (2). They report probit estimates with an indicator variable for policy change, an indicator variable for exposure to a STW scheme, the interaction term of both variables and additional explanatory variables which include personal, job and employer attributes such as gender, nationality, age, industry, region, job category, job tenure and firm size. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

with the same firm was about 4 pp lower two years later and 8 pp lower three years later for participants in STW schemes after the 2009 change relative to non-participants and relative to the period before the change. Results are virtually identical for the manufacturing subsample. Furthermore, the policy change seemed to be only slightly successful in achieving the attachment of (all) jobs and workers to firms, as can be deducted from the parameter on the *R* variable (β_2), although its effect is pretty small (lower than 1 pp in most cases and sometimes not statistically significant). At the same time, the impact of the treatment irrespective of the policy change (β_3) is significant, exerting a negative effect on the labour market outcomes of participant workers.

Next, Table 4 provides results from the IV bivariate recursive probit model (equations (2) and (3)). The full IV bivariate recursive probit model estimates with coefficients and explanatory variables are shown in Tables A1 and A2. These coefficients are used to calculate the average marginal effects displayed in the top panel and the average treatment effects (ATE) and average treatment effects on the treated (ATET) for the *R·D* parameter provided in the bottom panel. In the estimation of these models, STW participation is instrumented using whether the employer was previously involved in STW and the worker's receipt of unemployment benefits prior to the participation in STW.[15]

The first variable is a dummy variable that captures whether firms were exposed to an STW scheme prior to the starting of the observation period (either fourth quarter of 2008 or second quarter of 2009, for the pre- and post-reform sample, respectively), thanks to the availability of the anonymous identification codes of workers and firms. This variable tries to take account of the fact that the first important factor explaining the individual probability of

TABLE 4
Estimate Results of IV Recursive Bivariate Probit Models (Equations (2) and (3)): Full Sample.
Spain (MCVL, 2008–2013)

(a) Marginal effects

	Control A group			Control B group		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Policy change (R)	0.008***	0.007***	0.006***	0.011***	0.006	0.003
	(0.001)	(0.001)	(0.001)	(0.004)	0.005	(0.020)
STW particip. (D)	−0.078***	−0.084***	−0.114***	−0.080***	−0.154***	−0.178***
	(0.022)	(0.028)	(0.035)	(0.014)	0.017	(0.020)
$R·D$	−0.015	−0.046***	−0.083***	−0.016	−0.037***	−0.070***
	(0.010)	(0.013)	(0.015)	(0.011)	0.014	(0.016
Wald χ^2 (46)	22267.47	35742.57	45859.65	7532.20	8963.38	10061.74
Prob > χ^2	0.000	0.000	0.000	0.000	0.000	0.000
Individuals	382 589	382 589	382 589	33 311	33 311	33 311

(b) DID effects: ATE and ATET

	Control A group			Control B group		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
$R·D$ (ATE)	−0.016***	−0.048***	−0.083***	−0.017***	−0.037***	−0.067***
	(0.006)	(0.017)	(0.023)	(0.007)	(0.014)	(0.022)
$R·D$ (ATET)	−0.018***	−0.053***	−0.087***	−0.020***	−0.044***	−0.074***
	(0.006)	(0.015)	(0.019)	(0.007)	(0.011)	(0.015)

Note: The estimated marginal effects (standard errors) in each column come from a different regression on equations (2) and (3). For each regression, the estimates of an indicator variable for policy change (R), an indicator variable for exposure to a STW scheme (D) and the interaction term of both variables ($R·D$) on equation (2) are reported. Additional explanatory variables include personal, job and employer attributes (gender, nationality, age, industry, region, job category, job tenure and firm size). Worker's participation on STW is instrumented in equation (2). ATE and ATET refer to the average treatment effect and the average treatment effect on the treated, respectively, for the $R·D$ parameter. Standard errors are shown in brackets. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

being in STW is the employer decision of implementing such a programme. Obviously, this may be related to the firm's financial situation. Here we follow the approach used in Boeri and Bruecker (2011). That variable can be deemed a valid instrument as long as two conditions hold. First, prior use is a good predictor of contemporaneous usage due to learning: past experience may have increased knowledge about STW design features and created more acceptance of the adoption of this instrument among bargaining agents. And second, past experience affects its current use but does not directly affect contemporaneous employment outcomes. Although some firms might systematically face more volatile demand conditions than others, in the empirical exercise we control for some employers' attributes (industry and size), so the assumption still seems to be reasonable.

The second variable refers to the worker's receipt of unemployment benefits prior to the participation in STW. This indicator may be proxying relevant aspects that firms and workers' representatives take into account when they

have to choose which workers will participate in a given STW arrangement. From a theoretical point of view, one may argue that there are at least three channels through which the decision of the employer on which employees are to work short-time is influenced (see Scholz 2012). First, the cost of STW: depending on the design features of STW schemes, they may imply that employing low-skilled workers in STW results in lower costs due to their lower wage rates or they can be neutral with regards to the group of workers involved. Second, expectations and labour hoarding: firms which expect the lack of work to end soon may be more prone to apply the STW arrangement to all groups of employees; however, if firms consider that the economic activity will decline for longer, they may use STW to hoard workers they value (or those whose hiring and/or firing costs are higher) and layoff those who do not (or those who can be replaced rapidly and without costs when recovery resumes). And third, fairness considerations: employers may have good reasons to ensure that their behaviour is perceived as fair and thus face incentives to select a broad range of employees into STW (this may be further favoured by the mandatory process of negotiation with unions or workers' representatives); however, these considerations related to justice may be modelled by firms' attributes, such as the industry affiliation or the firm size, and the composition of the workforce.

Therefore, worker's receipt of unemployment benefits prior to the participation in STW may be a marker for different types of workers/jobs, proxying their stability/productivity or the hiring/firing costs associated with them. In practice, this indicator has been transformed into a dummy, taking the value 1 if the worker has received unemployment benefits prior to the moment of implementation of STW (i.e. previously to either 2008Q4 or 2009Q2) and 0 otherwise.[16]

One obvious caveat of the IV analysis carried out is whether the variables used are valid instruments for STW participation. The estimation of the participation model (equation (3)) with the inclusion of the instruments provides the result that individuals working in firms who previously used STW and who received unemployment benefits in the past exhibit significant higher chances of being involved in an STW programme. This means that one of the key identification conditions of the IV model holds. Regarding the second condition, we have used the residuals of the previous model in order to estimate an auxiliary model where residuals are the dependent variable against instruments as covariates. The results indicate that the instruments are not correlated with the unobserved variables. Furthermore, we have estimated equation (2) using instruments as covariates and found that they do not affect the outcome variable. Therefore, in the light of these results, we might conclude that the variables we have chosen are good instruments. Despite that, one cannot completely rule out the possibility that there are channels through which they affect the individual propensity of remaining employed in the future. For instance, if previous usage by firms is related to less competitiveness or more instability, and this influences subsequent outcomes; or if aspects associated with previous receipt of unemployment benefits affect the decisions

of employers not only to choose workers in STW but also to dismiss them later.

The results provided in Table 4 are remarkably similar to those obtained previously. The marginal effect of the *R* variable is lower than 1 pp in all cases. As above, this finding may be interpreted in the sense that the policy change exerted a very limited but positive impact to achieve the maintenance of workers and jobs. Regarding the combined impact of the policy change and participation in STW, it was negative for the group of participants, at least in the medium run, since the DID parameter, which is negative but not significant in the estimation corresponding to one year, becomes significant and larger in magnitude over time. Using either the control A or the control B group, the results indicate that the likelihood of continuing working with the same employer was the same one year later and about 4–5 pp and 7–8 pp lower two and three years later, respectively, for participants in STW schemes after the 2009 change relative to non-participants and relative to the period before the change.

The estimated impacts are quite similar if one calculates the ATET and ATE parameters.[17] The same happens when using either the manufacturing subsample or other potential instruments and even when restricting the sample to a group of STW firms in both the pre- and post-reform periods to examine how the experience of those workers compare in the same firm.[18] This latter robustness check provides smaller impacts though: participants in STW schemes after the 2009 change were equally likely to remain working with the same employer one and two years later and about 5.5 pp less likely three years later as compared to participants before the change.

Heterogeneity

Previous results may hide the existence of substantial heterogeneity. One dimension in which heterogeneity should be relevant is the skill content of jobs or the human capital of workers. The hiring/firing costs associated with jobs or the employees' human capital may influence the likelihood to participate in STW. From a theoretical point of view, the direction of this influence is not clear-cut (as we have commented on above). The same happens when we look at the empirical literature (see, among others, Büchel and Pannenberg 1992; Fuchs and Jacobsen 1991; Koumakhov and Najman 2001; Scholz 2012). Regarding the impact on the probability that jobs/workers survive once participation has been taken into account, there are two possibilities. One is that firms use STW to preserve jobs that are not economically viable; in this case, short-time workers (holding less productive jobs) are more likely to be laid-off after the end of the programme. The other is that employers put valuable jobs/workers in STW; thus, the preservation of these jobs/workers once the programme ends would not be surprising.

Consequently, we have performed the analysis carried out previously for various groups of workers broken down by job category. Average marginal effect results from the IV bivariate recursive probit model using the full

TABLE 5

Estimate Results of IV Recursive Bivariate Probit Models (Equations (2) and (3)) by Job Category: Marginal Effects and Standard Errors (in Brackets). Full Sample and Control B Group. Full Sample. Spain (MCVL, 2008–2013)

(a) Type of job

	Workers in white-collar jobs			Workers in blue-collar jobs		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Policy change (*R*)	0.009*	0.004	0.006	0.013**	0.009	−0.001
	(0.005)	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)
STW particip. (*D*)	−0.100***	−0.188***	−0.187***	−0.084***	−0.158***	−0.199***
	(0.029)	(0.036)	(0.042)	(0.017)	(0.020)	(0.023)
R·D	0.034	0.053*	0.021	−0.029**	−0.059***	−0.087***
	(0.025)	(0.031)	(0.037)	(0.013)	(0.016)	(0.019)

(b) Type of job and skill level

	Workers in white-collar high-qualified jobs			Workers in blue-collar high-qualified jobs		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Policy change (*R*)	−0.004	−0.017	−0.029	0.002	0.008	0.019
	(0.013)	(0.016)	(0.018)	(0.011)	(0.014)	(0.016)
STW particip. (*D*)	0.095	0.070	0.130	−0.137***	−0.144**	−0.128*
	(0.062)	(0.089)	(0.100)	(0.047)	(0.060)	(0.068)
R·D	−0.037	−0.098	−0.126	0.049	0.032	−0.002
	(0.051)	(0.072)	(0.081)	(0.041)	(0.053)	(0.061)

	Workers in white-collar medium/low-qualified jobs			Workers in blue-collar medium/low-qualified jobs		
	1 year	*2 years*	*3 years*	*1 year*	*2 years*	*3 years*
Policy change (*R*)	0.011*	0.008	0.013	0.012	0.009	−0.001
	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)	(0.011)
STW particip. (*D*)	−0.130***	−0.232***	−0.243***	−0.043*	−0.099***	−0.114***
	(0.032)	(0.039)	(0.046)	(0.025)	(0.030)	(0.034)
R·D	0.053*	0.090***	0.059	−0.041**	−0.080***	−0.095***
	(0.028)	(0.035)	(0.041)	(0.019)	(0.024)	(0.026)

Note: See Table 4. Significance levels *p < 0.10, **p < 0.05, ***p < 0.01.

sample and the control B group are provided in Table 5. The upper panel simply differentiate between white-collar and blue-collar jobs, while the bottom panel also considers the skill/qualification dimension within these groups. The results indicate that manual jobs are more likely to be negatively affected by the layoff decisions of employers after the reform, since the probability of remaining working in the same firm one, two and three years later is significantly lower for STW participants in the post-reform period as compared to non-participants and to the pre-reform period. Moreover, although the point estimates lack precision, it seems that this occurs preponderantly for less-skilled workers and less-qualified jobs. Taken as a whole, this evidence points out to the existence of 'displacement effects'

associated with the use of STW in the sense that firms maintain jobs (at least in the short run) that appear to be less productive and put workers holding this sort of jobs in STW.

Discussion

The reading of the results should not be to consider that STW schemes cause the destruction of jobs and the unemployment of workers. Surely enough, that would be misleading. Quite on the contrary, our interpretation is that discretionary policy changes in the design or in the incentives of STW schemes can be effective in the short run but they lose their ability when the decline in demand and the lack of work are more permanent. In other words, they cannot avoid the layoff of workers in the medium run the when the shock is persistent. This fits nicely with the empirical evidence of the micro literature based on firm-level data, which, in general, has not found significant (short-run) effects of STW on employment and with the results of some macroeconomic studies.

From a macro perspective, the theoretical explanation would be that a discretionary loosening of the STW eligibility criterion subsidizes worker-firm matches that would not have been destroyed even in the absence of the intervention. If the discretionary intervention is used in a transitory way, firms' future expectations remain unaffected and no additional jobs are saved. In contrast, rules both have a direct effect on unemployment through a reduction of the firing threshold and indirectly affect firms' hiring and firing decisions via future expectations. This explanation has been put forward by Balleer *et al.* (2014), who use a macro approach (SVAR model) to disentangle two components of STW: its role as an automatic stabilizer and the impact of discretionary policy changes. Their evidence points to powerful effects of the former but no effects of the latter on unemployment, using data from Germany for the post-reunification period. In the same vein, Hijzen and Martin (2013), although estimate that STW schemes had a significant impact on preserving jobs during the 2008–2009 crisis (amounting to 2 per cent in Germany and about 1 per cent in Japan and Italy in the short run), obtain that the *net* effect of STW on employment declined over time, becoming slightly negative in some countries (Germany and Italy) and strongly negative in others (Japan) eight quarters after the beginning of the crisis. These findings are further confirmed when they perform simulations based on their macro estimations under different assumptions about the persistence of the shock and the use of STW, with net impacts of zero after six/eight quarters when the downturn is more prolonged. They also find that deadweight costs associated with STW tend to diminish with the persistence of the recession, being above 70 per cent in the case of a very short downturn.

Furthermore, at the microeconomic level, one may argue that firms (and workers) who use the financial incentives of STW schemes may benefit from them in the short run, in the sense that they would allow them to maintain their level of employment (and to avoid unemployment), thereby benefiting not only the participating workers but also the non-participants. This would

happen at least during the first year, which coincide with the obligation to keep the workers involved employed. However, if the downturn continues much longer, the flexibility buffers become exhausted, with unavoidable layoffs as a consequence, affecting more intensely those jobs and workers involved in STW, who may be the first candidates to extend the list of lost jobs and permanent layoffs. This interpretation would be in line with the results obtained by Kruppe and Scholz (2014) using firm data from the German IAB establishment panel. They conduct an IV regression and find a significant influence of the share of short-time workers on the log employment change between June 2008 and June 2009 (when the crisis was still in full effect, thus supporting the results of Boeri and Bruecker 2011, who conducted a similar analysis for that period) that are not maintained when the period of analysis is extended until June 2010 (thus, contradicting those results).

This chain of events may happen not only because of the characteristics of workers (age, education or seniority) but also due to the attributes of jobs (skill content). If this interpretation is correct, the 'weakest' jobs and workers will be the least likely to survive. To be precise, our results suggest that workers employed in blue-collar less-skilled occupations are the ones who are least likely to continue working with the same employer after the firm has implemented an STW arrangement. Therefore, firms would use the flexibility devices at their disposal (and STW is one of them) to protect their core staff during crises and to avoid brain drains (Crimmann *et al.* 2012).

6. Conclusions

This article has investigated whether STW schemes were successful in their objective of preserving jobs and keeping workers employed in Spain by exploiting a policy change that took place in March 2009 that increased financial incentives to use the temporary suspension of workers' contracts or the reduction of their working time. We have contributed to the relatively small body of literature (mainly from a macroeconomic perspective but also based on establishment-level data) on the effects of STW on employment by using a worker-level approach and longitudinal administrative data. The dependent variable has been measured as the likelihood that an individual remains employed with the same employer in the future (one, two and three years after implementing the STW measure). This is relevant since, when using either country- or firm-level data, one is unable to evaluate the effect of STW on individual labour market trajectories. IV recursive bivariate probit models with endogenous covariates (in which participation in STW is instrumented) have been estimated, so we have tried to control for unobserved sources of heterogeneity that may create a spurious correlation between treatment status and labour market outcomes.

Our estimates suggest that the policy change was slightly successful in achieving the attachment of (all) jobs and workers to firms, although its effect was small or null (either lower than 1 pp or statistically insignificant). In

this regard, the alteration of incentives of STW schemes is deemed to have accomplished its goal. This result approximates the empirical evidence of the micro literature based on firm-level data which in general has not found any significant net effects of STW on employment (Bellmann *et al.* 2012; Calavrezo *et al.* 2009), departing from the large positive impacts found by others (Boeri and Bruecker 2011).

Regarding the combined impact of the policy change and participation in STW (the difference-in-differences effect), we find that it was null in the short run (after one year) for the group of participants and negative in the medium run (after two to three years). This impact threshold coincides with the employers' obligation to keep the workers involved employed for a minimum of one year after the conclusion of the STW arrangement. But the result might be related to the duration of the economic downturn as well, considering therefore that STW schemes and their discretionary policy changes can be effective in the short run in times of crisis but they tend to lose their ability when the weakness of economic activity is more permanent (as some studies underscore – see Hijzen and Martin 2013; Kruppe and Scholz 2014). Whether our findings can be considered correlations rather than causal effects is open to question. Although the results seem to be quite robust to various sensitivity analyses, such as different comparison groups, samples and estimation strategies, one cannot completely rule out the presence of endogeneity and unobserved heterogeneity that plagues this sort of studies. In fact, other explanations are also possible: workers who were chosen for STW may have unobservables that make them more prone to be laid off in the future; or firms with greater financial difficulties are more likely to take advantage of the incentives, so the economic problems (rather than the STW programme) lead to more layoffs in the future.

Finally, the present findings also complement the literature, which mainly highlight the 'deadweight costs' of the measures, by pointing out to the existence of potential 'displacement effects'. We find that the previous effect, taken at face value, seems to be concentrated on certain types of workers/jobs (in particular, workers employed in manual, less-qualified jobs), affecting more intensely those categories involved in STW, who may be the first candidates to extend the list of permanent layoffs. This means that firms would use STW to stabilize employment but also to protect their core jobs and employees during crises, bringing about 'displacement effects' as they preserve jobs that are not economically viable and leave workers that hold less productive jobs in STW schemes.

Acknowledgements

Notes

1. See, for instance, Krugman (2009). For some nuances on this perspective, see Brenke *et al.* (2013) and Balleer *et al.* (2014).
2. This scarcity contrasts with the large body of empirical works in other strands of active labour market policy, such as training, employment subsidies and the like, as the meta-analysis studies underscore (see, for instance, Card et al. 2009; Greenberg et al. 2003).
3. The estimates of Boeri and Bruecker (2011) range from less than 0.1 per cent of employment in Portugal and Norway to close to 2 per cent in Ireland, Italy and Finland and nearly 3 per cent in Belgium. However, the number of jobs saved is smaller than the number of workers involved in the programmes, so deadweight costs are potentially important: for some countries that difference is close to 100 per cent of full-time employment (FTE) workers, although the mean amounts to 66 per cent. These results are larger than those calculated in Hijzen and Venn (2011), who found that deadweight losses would account for over one-third of the subsidy. On the other hand, Hijzen and Martin (2013) estimate that STW schemes had a significant impact on preserving jobs during the crisis (2 per cent in Germany and about 1 per cent in Japan and Italy), but also with large deadweight costs, especially in the short run and when the downturn is transitory. In the case of studies using firm-level micro data, some of them show positive effects of STW on employment (Boeri and Bruecker 2011), while others find no effects on saving jobs or avoid dismissals (Bellmann *et al.* 2012; Kruppe and Scholz 2014).
4. The only exception is Gonthier (2012), who considers the change in regulation of STW in France, although focusing on firms. The main goal of Gonthier's work is to draw up a multidimensional description of the establishments that used short-time work, taking into account the interaction between short-time work and other labour flexibility tools. In chapter 7, she analyses the impact of the 2009 French STW reform. She exploits the discontinuity between firms with less than 250 employees and firms with 250 employees or more regarding the level of compensation they are eligible for, using a difference-in-difference approach. Her results suggest a slightly positive impact of the reform on the propensity to use STW but no effects on the number of STW hours consumed.
5. In fact, this proportion was not fixed but variable, depending on the number of workers affected and the size of the firm. The thresholds were as follows: ten workers in firms with less than 100 employees; 10 per cent of the workers in firms with 100–300 employees; and 30 workers in firms with more than 300 employees.
6. The firm might agree to supplement the workers' income as part of an ERE, so that no actual loss of labour income would be experienced during the period of STW.

In the case of both temporary layoffs and reduction of working hours, employers have to pay social contributions for those workers affected.

7. In fact, STW arrangements agreed several months before the passing of the legal change (from October 1, 2008) might benefit from the new financial incentives, but this was only known *a posteriori*, that is, employers and workers did not know at the time of adopting the agreed STW scheme that the legal change was going to take place.

8. To calculate the take-up rate, we have related the number of workers effectively affected by STW (making use of the *Estadística de Regulación de Empleo* database published by the Ministry of Employment and Social Security) and the total number of workers that could potentially be affected (the number of wage and salary employees in the economy, this latter information coming from the Labour Force Survey).

9. This sum includes not only the unemployment benefits paid to workers under STW but also the social contributions paid by the Public Employment Service to the Social Security.

10. More information on the database can be found in the Social Security website: http://www.seg-social.es/Internet_1/Estadistica/Est/Muestra_Continua_de_Vid as_Laborales/index.htm. The database used in this manuscript is available to allow replication studies. For more details on the characteristics of the dataset, see Arranz and García-Serrano (2011, 2014) and Arranz *et al.* (2013).

11. Furthermore, from a macro perspective, the data from the MCVL are fully comparable to those obtained with other sources measuring STW such as the *Estadística de Regulación de Empleo* database (published by the Ministry of Employment and Social Security). For instance, it reflects properly the large increase of STW spells in the second half of 2008 and in 2009, the reduction in 2010–2011 and the subsequent increase in 2012. It also takes account of the changes in the entries into and exits from the unemployment compensation system (see Arranz *et al.* 2013). These facts reassure us of the use of the MCVL in order to analyse STW issues.

12. The estimate results of a probit model on the probability of participating in STW for both pre- and post-reform periods confirm that male, Spanish, older workers are more likely to be chosen to participate and that the type of job, the industry affiliation and the employer size play an important role in determining that probability: being employed in blue-collar medium- and low-skilled jobs, in large firms (at least 250 employees) and in manufacturing increases that likelihood. The resulting effect on industry is substantial and reflects the fact that STW tends to be concentrated in the goods-producing sector and that incentives to hoard labour may be stronger there due to the greater importance of firm-specific skills. Furthermore, persons who work in the Basque Country (followed by those in Catalonia) are more likely to participate than workers in other regions. Both the Basque Country and Catalonia are characterized by exhibiting not only a productive structure where the shares of manufacturing and large firms are higher than in other Spanish regions but also a system of industrial relations more prone to the use of internal adjustment instruments.

13. In order to capture if treated workers and jobs are different before and after the reform, we have estimated a probit model on the probability of participating in STW in the post-reform period (value 1) versus the pre-reform period (value 0). The estimate results confirm that individuals aged 50 years or more and working in white-collar occupations, and jobs in sectors different from manufacturing of

    machinery and equipment and in small firms (with less than 50 employees) are more likely to participate in STW schemes in the post-reform period than in the pre-reform period.

14. Marginal effects are evaluated as the means of the data once personal, job and employer characteristics have been taken into account.

15. The estimates of ρ (the correlation between the unobservables in equations (2) and (3), see Tables A1 and A2) show that it is statistically significantly different to 0 in all models. This can be interpreted as indicating that the assumptions are satisfied and estimating two probits simultaneously (in a recursive manner) is a better option; otherwise, estimating two probits separately would have been the preferred option.

16. We have tried other instruments: job tenure and worker's family responsibilities (proxied by the number of people living in the individual's household). These variables may be linked to fairness considerations, if employers and workers' representatives are worried about future prospects of certain individuals in the event of permanent dismissals and try to protect them from that risk. However, one may argue that the information conveyed by these variables also affects the decision to lay off workers later.

17. With regard to other covariates (see Tables A1 and A2), the probability of a given person keeping on working with his/her employer is negatively associated with the following characteristics: being non-Spanish, being older than 50, having been in the company for less than 10 years, working in blue-collar low-skilled jobs, working in some specific industries (especially in building and services) and working in small firms (with less than 50 employees).

18. By using job tenure instead of previous receipt of unemployment benefits, the impacts are somewhat smaller: the likelihood of continuing working was the same one year later and about 3 pp and 6 pp lower two and three years later, respectively.

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Appendix

TABLE A1
Estimate Results of IV Recursive Bivariate Probit Models: Treatment and Control A Group (One Year, Two and Three Years Later); Spain (MCVL, 2008–2013)

	1 year later			2 years later			3 years later		
	Coeff.	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*
Equation (2) (outcome)									
Policy change (*R*)	0.044	0.005	***	0.029	0.005	***	0.018	0.005	***
Participation in STW (*D*)	−0.407	0.117	***	−0.322	0.107	***	−0.362	0.110	***
R-D	−0.076	0.053		−0.176	0.049	***	−0.263	0.047	***
Gender (men)	−0.050	0.006	***	−0.055	0.005	***	−0.070	0.005	***
Nationality (Spanish)	0.171	0.010	***	0.243	0.009	***	0.272	0.009	***
Age groups									
<30	0.172	0.010	***	0.279	0.009	***	0.324	0.009	***
30–39	0.191	0.010	***	0.308	0.009	***	0.371	0.009	***
40–49	0.178	0.011	***	0.281	0.010	***	0.322	0.010	***
≥50	–	–	–	–	–	–	–	–	–
Job category									
WCHS	0.018	0.012		0.016	0.011		0.048	0.010	***
WCMS	0.051	0.012	***	0.028	0.011	***	0.006	0.010	
WCLS	0.046	0.010	***	0.027	0.009	***	0.019	0.009	**
BCHS	0.094	0.011	***	0.092	0.010	***	0.099	0.009	***
BCMS	0.015	0.012		0.017	0.011		0.021	0.010	**
BCLS	–	–	–	–	–	–	–	–	–
Industry									
Manufacture of food products, textiles, wood and paper	−0.067	0.014	***	−0.067	0.013	***	−0.067	0.012	***
Extraction, energy, chemicals and manufacture of metals	−0.051	0.013	***	−0.084	0.012	***	−0.099	0.011	***
Manufacture of machinery, electrical and electronic or transport equipment	–	–	–	–	–	–	–	–	–

(*Continued*)

TABLE A1
Continued

	1 year later			2 years later			3 years later		
	Coeff.	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*
Trade and tourism	−0.035	0.013	***	−0.086	0.011	***	−0.140	0.011	***
Transport	−0.049	0.012	***	−0.077	0.011	***	−0.086	0.011	***
Building and rest of services	0.001	0.013		−0.032	0.012	***	−0.142	0.012	***
Regions									
Baleares, Canarias, Andalucía, Ceuta, Melilla & Murcia	−0.103	0.014	***	−0.090	0.012	***	−0.131	0.011	***
Aragón, Navarra & La Rioja	−0.081	0.017	***	−0.036	0.015		−0.080	0.014	***
Asturias, Galicia & Cantabria	−0.014	0.015		−0.021	0.014		−0.055	0.013	***
Extremadura, Castilla-La Mancha & Castilla-León	−0.072	0.015	***	−0.043	0.013	***	−0.078	0.012	***
Cataluña	−0.221	0.013	***	−0.183	0.012	***	−0.210	0.011	***
Valencia	−0.105	0.015	***	−0.098	0.013	***	−0.127	0.012	***
Madrid	−0.194	0.014	***	−0.183	0.012	***	−0.219	0.011	***
País Vasco	−	−	−	−	−	−	−	−	−
Firm size									
<50 workers	−	−	−	−	−	−	−	−	−
50–249 workers	0.049	0.007	***	0.099	0.006	***	0.144	0.006	***
250 +workers	0.144	0.007	***	0.222	0.007	***	0.317	0.006	***
Job tenure (≥10 years)	0.677	0.006	***	0.814	0.006	***	0.853	0.005	***
Intercept	0.698	0.023	***	0.181	0.021	***	−0.090	0.020	
Equation (3) (participation)									
Policy change (R)	0.268	0.015	***	0.268	0.015	***	0.267	0.015	***
Gender (men)	0.072	0.018	***	0.072	0.018	***	0.072	0.018	***
Nationality (Spanish)	0.128	0.043	***	0.128	0.044	***	0.130	0.044	***

(Continued)

TABLE A1
Continued

	1 year later			2 years later			3 years later		
	Coeff.	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*
Age groups									
<30	−0.953	0.026	***	−0.952	0.026	***	−0.957	0.026	***
30–39	−0.558	0.022	***	−0.557	0.022	***	−0.560	0.022	***
40–49	−0.474	0.023	***	−0.473	0.023	***	−0.474	0.023	***
≥50	—	—	—	—	—	—	—	—	—
Industry									
Manufacture of food products, textiles, wood and paper	−0.631	0.027	***	−0.631	0.027	***	−0.629	0.027	***
Extraction, energy, chemicals and manufacture of metals	−0.158	0.019	***	−0.158	0.019	***	−0.157	0.019	***
Manufacture of machinery, electrical and electronic or transport equipment	—	—	—	—	—	—	—	—	—
Trade and tourism	−1.225	0.029	***	−1.225	0.029	***	−1.223	0.029	***
Transport	−1.096	0.028	***	−1.097	0.028	***	−1.094	0.028	***
Building and rest of services	−1.131	0.030	***	−1.131	0.030	***	−1.133	0.030	***
Firm size									
+50 workers	0.807	0.021	***	0.807	0.021	***	0.807	0.021	***
Instruments									
Worker's receipt of UB (1 = yes)	0.057	0.015	***	0.058	0.016	***	0.055	0.015	***
Firm's previous use of STW (1 = yes)	0.169	0.016	***	0.169	0.016	***	0.169	0.016	***
Intercept	−2.111	0.054		−2.111	0.054		−2.110	0.054	
Rho	0.079	0.047		0.040	0.043		0.080	0.045	
Log likelihood	−152832.26			−192016.01			−217221.98		
Sample	382 589			382 589			382 589		

Note: Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A2

Estimate Results of IV Recursive Bivariate Probit Models: Treatment and Control B Group (One Year, Two and Three Years Later); Spain (MCVL, 2008–2013)

	1 year later			2 years later			3 years later		
	Coeff.	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*
Equation (2) (outcome)									
Policy change (R)	0.053	0.019	***	0.024	0.017	***	0.009	0.016	***
Participation in STW (D)	−0.400	0.070	***	−0.558	0.063	***	−0.551	0.062	***
$R·D$	−0.078	0.057		−0.134	0.052	***	−0.215	0.051	***
Gender (men)	−0.070	0.020	***	−0.068	0.018	***	−0.078	0.017	***
Nationality (Spanish)	0.167	0.039	***	0.224	0.036	***	0.225	0.035	***
Age groups									
<30	0.177	0.033	***	0.247	0.030	***	0.301	0.029	***
30–39	0.219	0.031	***	0.278	0.029	***	0.338	0.028	***
40–49	0.219	0.033	***	0.249	0.030	***	0.294	0.029	***
≥50	—	—	—	—	—	—	—	—	—
Job category									
WCHS	0.053	0.042		0.045	0.038		0.018	0.037	
WCMS	0.077	0.042	*	0.051	0.037		0.034	0.036	
WCLS	0.061	0.033	*	0.071	0.030	**	0.022	0.029	
BCHS	0.160	0.035	***	0.182	0.031	**	0.136	0.030	***
BCMS	0.024	0.036		0.088	0.033	***	0.090	0.032	***
BCLS	—	—	—	—	—	—	—	—	—
Industry									
Manufacture of food products, textiles, wood and paper	−0.076	0.041	*	−0.132	0.037	***	−0.206	0.036	***
Extraction, energy, chemicals and manufacture of metals	−0.059	0.035	*	−0.112	0.032	***	−0.134	0.030	***
Manufacture of machinery, electrical and electronic or transport equipment	—	—	—	—	—	—	—	—	—

(Continued)

TABLE A2
Continued

	1 year later			2 years later			3 years later		
	Coeff.	S.E.	Sign.	Coeff.	S.E.	Sign.	Coeff.	S.E.	Sign.
Trade and tourism	−0.059	0.035	*	−0.187	0.031	***	−0.284	0.030	***
Transport	−0.077	0.034	**	−0.173	0.031	***	−0.255	0.030	***
Building and rest of services	−0.096	0.039	***	−0.136	0.036	***	−0.250	0.034	***
Regions									
Baleares, Canarias, Andalucía, Ceuta, Melilla & Murcia	−0.113	0.053	**	−0.062	0.047		−0.067	0.045	
Aragón, Navarra, La Rioja	−0.077	0.062		−0.006	0.055		0.013	0.053	
Asturias, Galicia & Cantabria	0.041	0.060		0.027	0.052		0.024	0.050	
Extremadura, Castilla-La Mancha & Castilla-León	−0.117	0.058	**	−0.062	0.052		−0.045	0.050	
Cataluña	−0.156	0.049	***	−0.050	0.043		−0.035	0.041	
Valencia	−0.136	0.053	***	−0.102	0.047	**	−0.103	0.045	**
Madrid	−0.122	0.051	**	−0.068	0.045		−0.042	0.043	
País Vasco	—	—	—	—	—	—	—	—	—
Firm size									
<50 workers	—	—	—	—	—	—	—	—	—
50–249 workers	0.102	0.023	***	0.181	0.021	***	0.197	0.020	***
250 +workers	0.183	0.024	***	0.292	0.021	***	0.359	0.020	***
Job tenure (≥10 years)	0.808	0.023	***	0.941	0.020	***	1.002	0.019	***
Intercept	0.626	0.080		0.146	0.073	**	−0.058	0.070	
Equation (3) (participation)									
Policy change (R)	0.027	0.026	***	0.025	0.026		0.027	0.026	
Gender (men)	0.207	0.031	***	0.205	0.030	***	0.202	0.030	***
Nationality (Spanish)	0.303	0.081	***	0.315	0.081		0.312	0.081	***

(Continued)

TABLE A2
Continued

	1 year later			2 years later			3 years later		
	Coeff.	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*	*Coeff.*	*S.E.*	*Sign.*
Age groups									
<30	−1.425	0.047	***	−1.435	0.047	***	−1.440	0.046	***
30–39	−0.749	0.041	***	−0.756	0.041	***	−0.760	0.041	***
40–49	−0.551	0.043	***	−0.548	0.043	***	−0.548	0.043	***
≥50	—	—	—	—	—	—	—	—	—
Industry									
Manufacture of food products, textiles, wood and paper	−0.613	0.048	***	−0.606	0.048	***	−0.608	0.048	***
Extraction, energy, chemicals and manufacture of metals	−0.200	0.032	***	−0.199	0.032	***	−0.200	0.032	***
Manufacture of machinery, electrical and electronic or transport equipment	—	—	—	—	—	—	—	—	—
Trade and tourism	−1.523	0.049	***	−1.514	0.049	***	−1.519	0.049	***
Transport	−1.201	0.048	***	−1.199	0.048	***	−1.199	0.048	***
Building and rest of services	−1.009	0.052	***	−1.017	0.052	***	−1.021	0.052	***
Firm size									
+50 workers	0.949	0.035	***	0.952	0.035	***	0.953	0.035	***
Instruments									
Worker's receipt of UB (1 = yes)	−0.271	0.030	***	−0.273	0.029	***	−0.275	0.029	***
Firm's previous use of STW (1 = yes)	−1.575	0.028	***	−1.573	0.028	***	−1.569	0.028	***
Intercept	0.312	0.101	***	0.303	0.101	***	0.309	0.100	***
Rho	0.120	0.037	***	0.212	0.033	***	0.228	0.033	***
Log likelihood	−18654.142			−21969.708			−23713.558		
Sample	33 311			33 311			33 311		

Note: Significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.