Appendix

A.1 Estimations: Additional Results

Wage Estimations. We estimate a wage equation on FLFS data to impute wages in the Census used for labor supply estimations. We specify the wage equation as:

$$\log w_i = \theta(A_i) + \zeta.EDUC_i + \kappa.Z_i + \rho\lambda_i + \nu_i$$

assuming a normally distributed residual ν_i and including the following explanatory variables: a smooth function of age $\theta(A_i)$, a set of education categories $EDUC_i$ and additional controls Z_i (gender). We follow the standard Heckman approach and introduce an inverse Mills ratio λ_i, estimated on the basis of a reduced form employment probability. The latter includes disposable income at zero hours $C(0;A_i)$ as an instrument, relying again on the discontinuity at age 25 for identification. Log hourly wage estimations are performed on FLFS data. According to Chemin and Wasmer (2012), the FLFS is a robust dataset that contains detailed information on earnings and that can be used for reliable wage estimation. Moreover, all explanatory variables, and in particular the education categories in vector $EDUC_i$, are available in both the FLFS and the Census according the exact same definition. Using estimates, we predict wages for all individuals in the Census, drawing wage residuals ν_i in a normal distribution with zero mean. Since, in principle, workers cannot receive wages below the minimum wage, we discard ν_i draws leading to wages below this wage floor for employed individuals in the Census.

Estimates are reported in Table A.1 (left panel) together with the reduced-form participation equation for the Heckman correction. Focusing on the estimates used to predict wages for models A-C, we observe a significant gender gap, in line with the existence of a "sticky floor" effect in France, as well as a regular wage progression with the level of education. In the participation equation, disposable income when out of work is negative, as expected, and statistically significant.[i] The right panel shows wage and participation equations for wage imputation in model D, i.e. using a less parsimonious definition of education whereby we distinguish only between HS dropouts and those with any form of education. With model D, we aim to check if our results are sensitive to the implicit restriction on education in our baseline (the wage equation uses detailed education categories while the preference parameters only vary by broad education group). As discussed in the main text, this is not the case.

Table A.1: Wage Estimation with Selection on LFS Data

	Wage Estimations for Models A-C				Wage Estimations for Model D			
Variables	Log wage		Employment		Log wage		Employment	
Age	0.011	(0.055)	0.339	(0.221)	0.138	(0.060)	0.306	(0.217)
Age square / 100	0.000	(0.001)	-0.006	(0.004)	-0.002	(0.001)	-0.006	(0.004)
Female	-0.101	(0.016)	-0.004	(0.059)	-0.078	(0.018)	-0.007	(0.059)
Education	(omitted: HS dropouts)				(omitted: any education)			
HS dropouts					-0.206	(0.024)		
Junior vocational qualification	0.066	(0.026)						
Highschool diploma	0.120	(0.037)						
Vocational highschool dipl.	0.137	(0.029)						
Graduate qualification	0.344	(0.024)						
Disposable income 0 hours/100			-0.065	(0.037)			-0.058	(0.035)
Inverse Mills ratio	0.131	(0.067)			0.218	(0.034)		
Constant	3.290	(0.701)	-3.767	(2.823)	1.791	(0.764)	-3.382	(2.771)
Observations	1,425		2,040		1,425		2,040	

Note: estimations are performed on the French Labor Force Survey (FLFS) for the year 1999. Standard errors in brackets.

We check the robustness of our wage imputation in Figures A.1 (men) and A.2 (women). The upper graphs show that actual and predicted log wage distributions for workers in the FLFS are relatively comparable, with the exception of the few observations below the minimum wage, a situation that we rule out in our predictions. The bottom-left graph of each Figure shows that the distribution of predicted (log) wages for workers in the Census is very comparable to the one obtained in the FLFS (top right graph). This confirms that distributions of socio-demographics in both surveys are similar enough (see Table 1) and allow comparable predictions of the wage distribution. The last graph shows the distributions of predicted (log) wages for the whole Census selection (workers and non-workers), as used in the labor supply estimations. Moving from wages to disposable incomes, we show in the next sub-section that predicted disposable incomes, calculated using tax-benefit simulations and gross incomes (actual ones in the FLFS or work duration $\times$ imputed wages in the Census), line up quite closely in the two datasets. Figure A.3 shows the distribution of in-work income calculated imputed wages in the Census: it reflects the fact that most of the variation in wage rates occurs for young workers.

Figure A.1: Comparing Actual and Predicted Log Wage Distributions in FLFS and Census Data (Men)

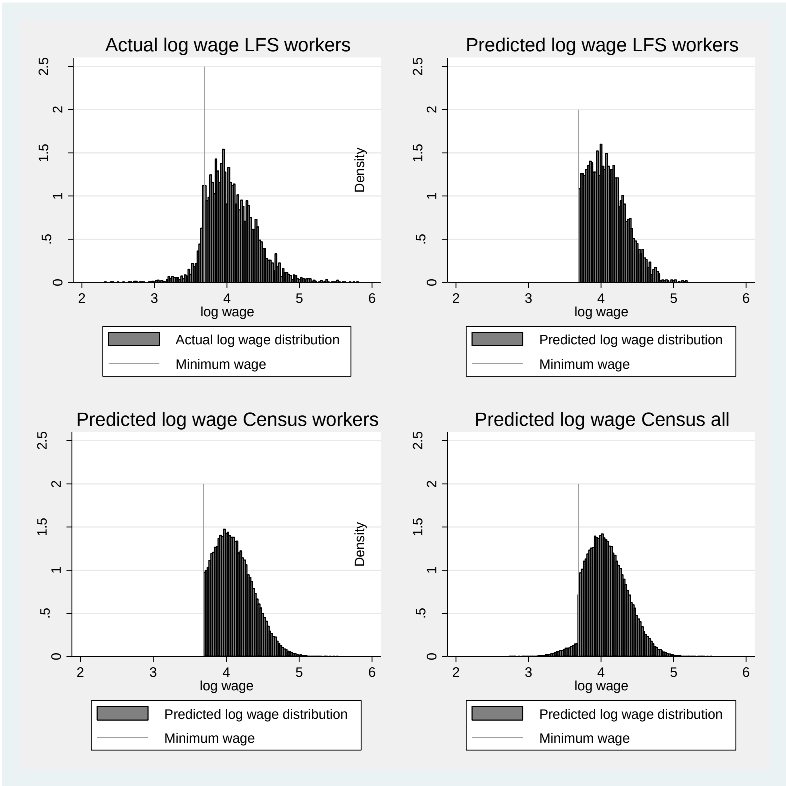


Figure A.2: Comparing Actual and Predicted Log Wage Distributions in FLFS and Census Data (Women)

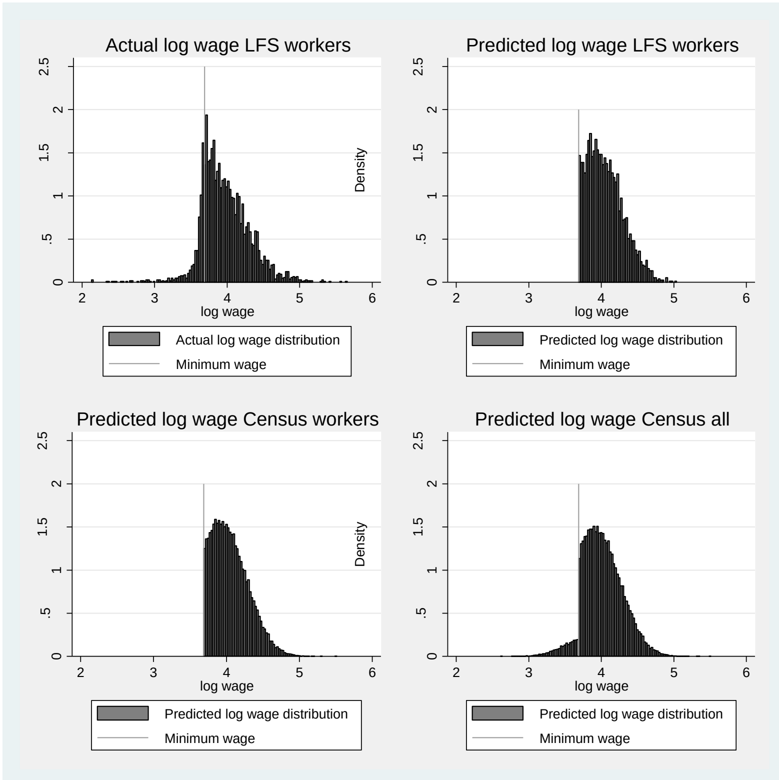
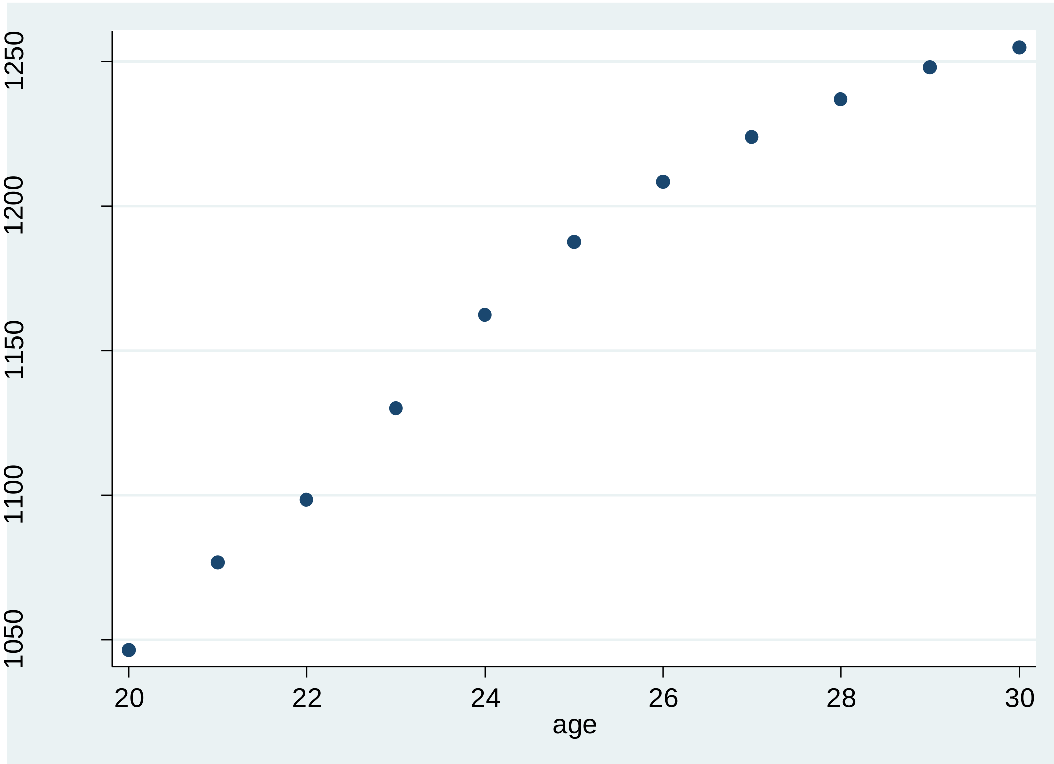


Figure A.3: Mean Predicted In-work Income by Age (Census data)



Labor Supply Estimates. Table A.2 shows the estimates of the RD model and of the participation model. Looking at the constant in the coefficients on in-work and out-of-work income in the participation model, the marginal effect of 1 additional EUR on participation is very different whether we consider in-work or out-of-work income. The effect of income at zero hours is roughly six times smaller than that of income at 39 hours for uneducated (HS dropout) females with model A. This could reflect (i) the fact that financial incentives depend primarily on income prospects on the labor market, (ii) the negative effects attached to welfare payments (e.g., stigma), (iii) other reasons including the lack of variability in $C(0, A_i)$ for the identification of a differentiated effect

Table A.2: Estimates: RD and Participation Models on Census Data

	RD		Model A		Model B		Model C		Model A2		Model A3	
	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
Non-monetary Propensity to work												
Coefficients α^o for RD and a^o for Structural Models:												
Constant	-5.816	1.805	-26.524	9.967	-27.255	9.997	-25.043	10.594	-29.103	9.789	-29.103	9.789
Educated	3.188	1.994	10.108	11.649	9.081	11.679	5.807	12.256	15.100	11.488	15.100	11.488
Male	0.068	0.008	0.748	0.145	0.732	0.146	0.734	0.146	0.751	0.145	0.751	0.145
Male x Educated	-0.040	0.009	-0.164	0.160	-0.238	0.160	-0.242	0.160	-0.167	0.160	-0.167	0.160
Coefficients α^1 for RD and a^1 for Structural Models:												
Age	0.716	0.221	2.742	1.219	2.791	1.220	2.649	1.240	0.751	0.293	0.250	0.098
Age2 / 10	-0.265	0.089	-0.991	0.493	-1.005	0.493	-0.996	0.493	-0.067	0.029	-0.007	0.003
Age3 / 1000	0.326	0.119	1.181	0.656	1.201	0.657	1.251	0.661	0.020	0.010	0.001	0.000
Age x Educated	-0.361	0.244	-1.112	1.422	-1.147	1.424	-0.968	1.442	-0.429	0.344	-0.143	0.115
Age2 x Educated / 10	0.145	0.098	0.469	0.574	0.501	0.575	0.512	0.575	0.044	0.034	0.005	0.004
Age3 x Educated / 1000	-0.192	0.131	-0.646	0.764	-0.660	0.765	-0.778	0.773	-0.015	0.011	-0.001	0.000
Policy Effect : Treatment Effect (RD coefficients β on Age >=25)												
Constant	-0.027	0.013										
Educated	0.019	0.014										
Male	-0.020	0.010										
Male x Educated	0.013	0.010										
Policy Effect : Financial Incentives to Work (Behavioral models parameters)												
Income Out of Work (coefficients b0) / 100			0.038	0.022	0.039	0.022	0.039	0.022	0.033	0.021	0.033	0.021
x Educated			-0.014	0.025	-0.011	0.025	-0.011	0.025	-0.004	0.024	-0.004	0.024
x Male			0.025	0.016	0.024	0.016	0.024	0.016	0.026	0.016	0.026	0.016
x Male x Educated			-0.020	0.019	-0.026	0.019	-0.027	0.019	-0.020	0.019	-0.020	0.019
Income In Work (coefficients b^o1) / 100			0.217	0.011	0.270	0.057	-0.027	0.474	0.217	0.011	0.217	0.011

x Educated		-0.067	0.012	0.166	0.063	0.661	0.528	-0.067	0.012	-0.067	0.012
x Male		-0.052	0.013	-0.051	0.013	-0.051	0.013	-0.052	0.013	-0.052	0.013
x Male x Educated		0.012	0.014	0.015	0.014	0.015	0.014	0.012	0.014	0.012	0.014
Income In Work (coefficients $b^1$1) / 100											
x Age				-0.0020	0.0021	0.0214	0.0372				
x Age2 / 10						-0.0046	0.0072				
x Age x Educated				-0.0088	0.0023	-0.0476	0.0412				
x Age2 x Educated / 10						0.0075	0.0080				
Log Likelihood		-91,613		-91,557		-91,557		-91,610		-91,610	
prob > chi2		0		0		0		0		0	
Observations	202,093	202,093		202,093		202,093		202,093		202,093	

RD estimates are obtained by OLS. The participation models are estimated by simulated ML with conditional probabilities averaged over ten wage x unobserved heterogeneity draws. Model (A) omits age in the marginal utility of income while the latter vary linearly and quadratically with age in models (B) and (C) respectively. Models (A2) and (A3) are similar to model (A) but use age in quarters and months respectively rather than age in years. All estimates are based on the 1999 Census data (for behavioral models, wages are imputed using estimations on the Labor Force Survey).

A.2 RD Analysis: Additional Checks

Confounding Institutional Factors at Age 25. We discuss possible confounding factors regarding the age discontinuity under study. Among all institutional features that could also be responsible for a sharp change in employment patterns at age 25, we first investigated other tax-benefit policies. The only relevant benefit policy in terms of age conditions appeared to be the RMI itself, i.e., parents receiving the RMI obtain an increment for children aged 21-24. However, this applies only if the child is a student, and hence does not concern our target group of HS dropouts. On the tax side, tax deductions are linked to the legal obligation of parents to financially support their children, which stops at the child's 25th birthday. Hence children may expect a double income effect when they turn 25 (transfers received from their parents may simply decrease as this obligation stops, and this effect is accentuated by the fact that parents become poorer as they no longer benefit from tax deductions). If leisure is a normal good, tax policy cannot explain a *drop* in employment at age 25. Finally, we have checked all the labor market policies targeted at young workers that may affect their labor supply (by decreasing job search costs) or the labor demand if youth employment is subsidized by the state. For year 1999, relevant schemes (i.e. with an age condition) included subsidized training programs in the private sector (with part-time work paid below the minimum wage) and subsidized public-sector jobs for the youth. Importantly, both schemes concerned youths under 26 -- or even under 30 in some cases. Hence, we confirm that there is no other factor at work at the 25 year-old threshold, except the RMI (see Bargain and Doorley, 2011, for more details).

Sensitivity Checks and Placebo. First, we have checked whether results were sensitive to the distance of observations from the discontinuity. The parametric estimation provides global estimates of the regression function over all values of the forcing variable, while the RD design depends instead on local estimates of the regression function at the cutoff point. Thus we verify whether the treatment effect varies in a linear spline model for an increasingly small window around age 25. We find very stable estimates, which are additionally confirmed by non-parametric estimations with varying bandwidths (not reported). Second, Figure A.4 reports non-parametric trends obtained using age expressed in days rather than years. This leads to the same qualitative conclusions as results with age in years in the main text. Third, in this Figure, we also compare the RD effect to the changes in employment at age 25 for a number of placebo control groups, not affected by the discontinuity. The first group is uneducated workers with children, i.e.not affected by the age condition. We find no significant employment change at 25 for this group. A second set of comparison groups consists of uneducated workers in 1982 (before the introduction of the RMI) and in 1990 (only one year after its introduction, i.e., a time when the program was not yet well publicized and concerned a much smaller population). As shown in Figure A.4, there is no sign of a discontinuity at 25 for these two placebo groups.

Graphical RD for the 2009 RSA Reform. In Figure A.5, we plot actual employment rates for our population of HS dropouts at ages around 25 for the years prior to the RSA reform (2004-08) and the available data years just after (2010-2011). We first notice that employment levels have declined compared to the period for which we carry out our estimations, i.e. the year 1999. Note that the data collection process has also changed in the meantime, so that two years of data are now necessary to obtain the same sample size as in the year 1999. This is what we have for the post 2009 period. For the period just before the reform, we pool four years of data so that our estimates of the

RMI effect for 2004-08 are more precise than for 1999. Graphically, the latter show the same type of drop at 25 as for the year 1999, yet slightly smaller in magnitude (see RD estimates in Table 3 and the related discussion in section 5.2). The post 2009 years show no sign of an employment effect, indicating the possible incentivizing effect of the in-work component of the RSA. We also notice a marked decline in employment levels post 2009, reflecting the impact of the Great Depression, which is of course something that the structural model cannot predict. Nonetheless, as far as the external validity check is concern, our attempt is to correctly predict the change in *relative* employment levels under and above 25 due to the RSA reform.

Figure A.4: Employment Rates of Childless Singles (Census, Age in Days: Nonparametric Fit)

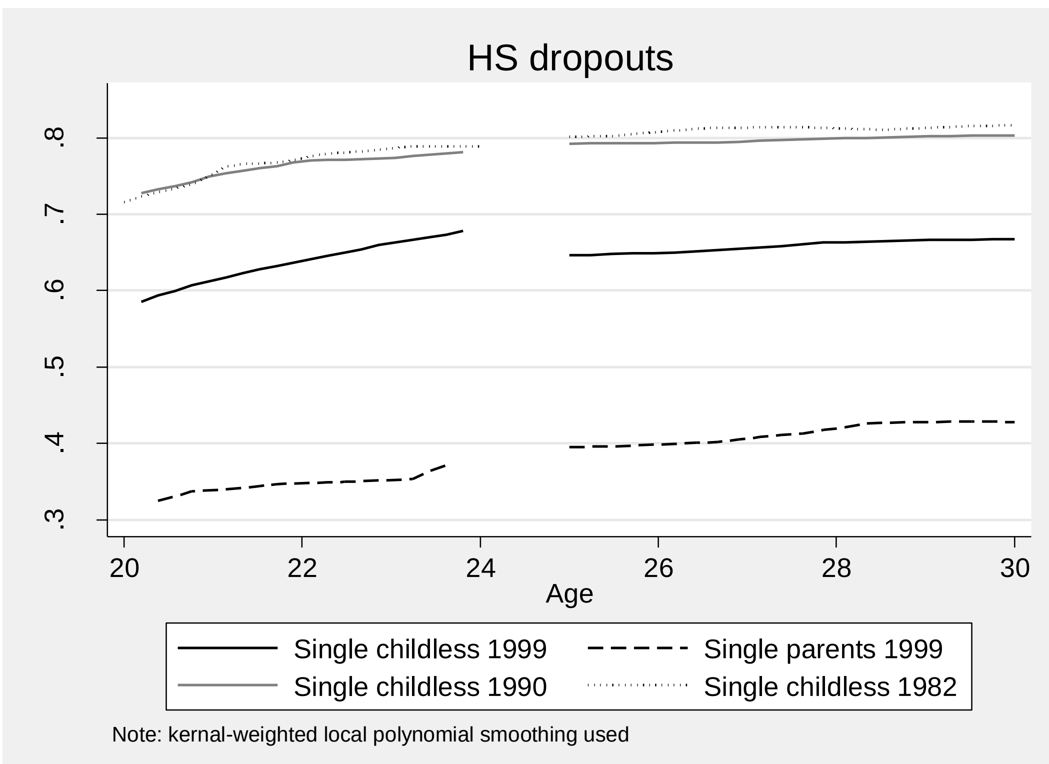
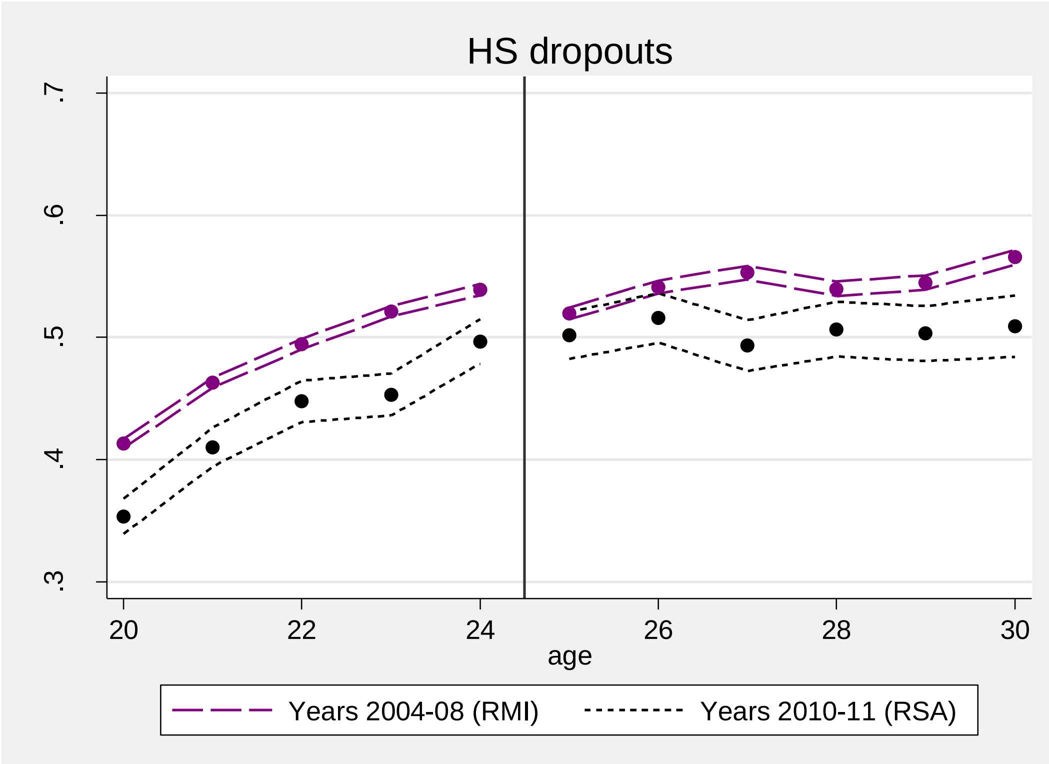


Figure A.5: Employment Rates of Childless Singles (Census, Age in Years)



A.3 Treatment Effect in the Structural Framework

We explain here how the structural model can be used to assess the RMI employment effect at the discontinuity. The differential in employment levels between 24 and 25 is not exactly equal to the treatment effect. Indeed we need to account for employment trends on both sides of the cutoff. Ignoring individual heterogeneity and assuming we use a linear probability model to ease notation, we can write the treatment effect in the RD design as:

$$\beta = \bar{Y}_{25} - \bar{Y}_{24} - [\alpha^1(25) - \alpha^1(24)]$$

with $\bar{Y}_A$ the average participation level at age A. By analogy, we can define the treatment effect in the structural model as:

$$\bar{Y}_{25} - \bar{Y}_{24} - [a^1(25) - a^1(24)].$$

When assuming b parameters independent from age A (model A), this corresponds to

$$\{b_1 C(wH;25) - b_0 C(0;25)\} - \{b_1 C(wH;24) - b_0 C(0;24)\},$$

or with $b_1 = b_0 = b > 0$:

$$b\{[C(wH;25) - C(0;25)] - [C(wH;24) - C(0;24)]\}.$$

This illustrates well the fact that the policy effect affects employment levels by changing the financial gains to work between 25 and 24 years old. Yet, this expression would be correct if the 24 and 25 years old had the same wage w and the same marginal utility of income b. In other words, equation (treatart) fails to account for the continuous effects of age other than through term a^1. With structural models, the correct measure of the policy effect requires the evaluation of the employment gap at age 25 using a counterfactual employment level for the 25 years old in the absence of RMI (i.e. as if they were 24). That is, with $b_1 = b_0 = b$, the correct policy effect is written:

$$b(25)\{[C(\widetilde{w}_i(25)H;25) - C(0;25)] - [C(\widetilde{w}_i(25)H;24) - C(0;24)]\},$$

where we highlight the impact of age on term b and on wage levels. Then in the general case:

$$\{b_1(25)C(\widetilde{w}_i(25)H;25) - b_0(25)C(0;25)\}$$
$$- \{b_1(25)C(\widetilde{w}_i(25)H;24) - b_0(25)C(0;24)\},$$

Using this expression and the equality between (treatart) and (treatart2), the policy effect becomes:

$$\bar{Y}_{25} - \bar{Y}_{24} - [a^1(25) - a^1(24)]$$
$$+ \{b_0(25)C(0;24) - b_0(24)C(0;24)\}$$
$$- \{b_1(25)C(\widetilde{w}_i(25)H;24) - b_1(24)C(\widetilde{w}_iH(24);24)\}.$$

Out-of-work income does not vary with age on the same side of the cutoff so that we must impose b_0 independent from age. Finally, we have:

$$\bar{Y}_{25} - \bar{Y}_{24} - [a^1(25) - a^1(24)]$$
$$- \{b_1(25)C(\widetilde{w}_i(25)H;24) - b_1(24)C(\widetilde{w}_i(24)H;24)\}.$$

The policy effect at the cutoff is therefore the age variation in employment rates corrected by the differential age effect on employment trends due to wages and behavioral parameters a and b .

Additional References in the Appendix

Arulampalam, Wiji, Alison Booth and Mark Bryan. 2007. "Is There a Glass Ceiling over Europe? Exploring the Gender Pay Gap across the Wage Distribution," *Industrial and Labor Relations Review*, 60(2), 163-186.

Eklof, Matias and Hans Sacklén. 2000. “The Hausman-MaCurdy Controversy: Why do Results Differ Between Studies?” *Journal of Human Resources*, 35(1), 204-220

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[i] Note that we have also run a similar wage model using the pooled 1997-01 sample to give a larger sample size (unreported). The coefficient estimates are similar, and subsequent results (wage predictions and labor supply estimations on Census data) are also very similar regardless of the choice of the wage estimation sample.