n° 2016-12 September 2016

WORKING PAPERS

The Effect of Social Benefits on Youth Employment: Combining RD and a Bahavioural Model

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The Effect of Social Benefits on Youth Employment: Combining RD and a Behavioral Model*

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May 2016, forthcoming in the *Journal of Human Resources*

Abstract

Natural experiments provide robust identifying assumptions for the estimation of policy effects. Yet their use for policy design is often limited by the difficulty of extrapolating on the basis of reduced-form estimates. In this study, we exploit an age condition in the eligibility for social assistance in France, which lends itself to a regression discontinuity (RD) design. We suggest to make the underlying labor supply model explicit, i.e. to translate the reduced-form discontinuity in terms of discontinuous changes in disposable incomes. This exercise shows the potential of combining natural experiments and behavioral models. In particular, we can test the external validity of the combined approach. We find that it predicts the effect of a subsequent reform, which extends transfers to the working poor, remarkably well. The model is then used to simulate the extension of social assistance to young people and finds that a transfer program with an in-work component would not create further disincentives to work in this population.

Key Words: behavioral model, regression discontinuity, labor supply
JEL Classification : C52, H31, J22.

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1 Introduction

Recent debates in the economic literature tend to compare and contrast the different approaches existing for policy evaluation (Angrist and Pischke, 2010, Deaton, 2009, Heckman and Urzua, 2010). A reasonable approach, however, seems to try to combine them optimally (Blundell, 2012). In particular, the economic literature should attempt to reconcile the methods based on randomized or natural experiments (ex post policy evaluation) with those relying on structural, behavioral models (ex ante evaluation). As stated by Imbens (2010), "much of the debate ultimately centers on the weight researchers put on internal validity versus external validity". Critics of the structural approach generally argue that it is difficult to identify all the primitive parameters in an empirically compelling manner because of selection effects, simultaneity bias and omitted variables. Thus, for causal inference of actual policy effects, it is hard to dispute that the experimental and quasi-experimental approaches are preferable. Yet, their external validity is often limited, given the reduced-form nature of the estimated statistics and the fact that these statistics are not policy invariant parameters of economic models. This explains why structural models are still broadly used, allowing analysts to perform ex ante simulations for policy design as well as welfare analyses.

In this study, we combine the two approaches, focusing on the labor supply effect of tax-benefit policies. We first rely on an age condition leading to a discontinuity in eligibility for the main social assistance program in France. We focus on the welfare program in place before 2009, a transfer to the workless poor (the *Revenue Minimum d'Insertion*, RMI). We exploit the fact that childless single individuals under 25 years of age are not eligible for this transfer. Estimates of the negative employment effect of social assistance are identified at the threshold using an RD design. We then suggest to make explicit the underlying labor supply model, i.e. to translate the reduced-form discontinuity in terms of discontinuous changes in disposable incomes. Bringing in structure related to short-term financial incentives allows us to perform simulations of policy reforms directly.

This exercise shows the potential of combining natural experiments and behavioral models. The discontinuity guarantees credible identification of the structural model while the behavioral model allows us to answer some of the questions at the core of the French political debate: What is the effect of an in-work transfer reform that extends RMI payments to the working poor (the *Revenue de Solidarité Active*, RSA, introduced in 2009)? Does an extension of welfare programs to under-25 year-olds generate greater unemployment and, possibly, long-term poverty among the youngest workers?

The first question allows us to test the external validity of the combined approach. That is, we subject the model to a real test by simulating the 2009 reform and comparing our results to what really happened. We find that the 2009 RSA has restored work incentives among the over-25 year olds. Most importantly, the magnitude of the simulated effect is remarkably close to the actual effect measured by an ex post analysis – and this is true overall and for specific sub-groups (men, women, high school dropouts). Thus, estimating a structural model on (quasi)experimental data seems to deliver on the promise that such a model can be used for credible predictions of policy effects. Moreover, the policy studied in our extrapolation check is particularly relevant in the current policy debate, characterized by an increasing tendancy of countries to opt for in-work benefits rather than blanket universal transfers for those out of work. The suggested reform combines generous in-work and out-of-work payments to combat poverty while maintaining financial incentives to work, so it is particularly suited to the young and unskilled workers.[1]

From there, we can address the second question and simulate the extension of either the RMI or RSA systems to the youth. This is of particular importance in the present context of increasing youth unemployment. The group of $16 - 24$ year olds has been hit particularly hard by the crisis (unemployment in this group has increased steadily in recent years in France, from 22.9% in 2011 to 25.5% in 2013). The youth also have limited access to welfare programs, which results in a poverty rate twice as large as that of the 25-30 year-olds. At a time of benefit cuts in several European countries, the extension of social assistance to the youth is under debate. The at risk of poverty and social exclusion rate has been steadily increasing for young people in Europe over the course of and subsequent to the Great Recession (+11% between 2008-2014 for the under 25s in the EU-27 compared to +3% for all age groups, source: EUROSTAT). An age condition limiting the availability or the level of welfare payments to the under 25s exists, not only in France, but also in Spain, Luxembourg and Denmark (see also Lemieux and Milligan, 2008, for Canada). In April 2016, the French government suggested the creation of a "mini-RSA" for the youth, which was criticized for its potential to increase non-employment in this population. Given this, we suggest a series of simulations based on our structural model. In essence, we find that while traditional social assistance (RMI) would indeed increase the inactivity of the under-25 population, the RSA program – either partial or full – should not reduce participation significantly in this population. Overall, it seems possible to reduce poverty in this vulnerable group without further weakening their attachment to the labor market.

The paper is structured as follows. Section 2 explains the contribution of the paper while reviewing the existing literature. Section 3 presents the institutional background and the

[1] This reform relates to recent debates on the optimal design of tax-benefit systems (see Immervoll et al., 2007) and on the efficiency of in-work transfers such as those in place in the US and the UK (i.e., the Earned Income Tax Credit, EITC, and the Working Family Tax Credit, WFTC).

data while section 4 presents the empirical strategy. Section 5 reports and analyzes the results while section 6 concludes.

2 Literature and Contribution

2.1 (Quasi-)Experiments

There is a strong history of using natural experiments – notably US/UK tax-benefit reforms – to quantify labor supply responses. For example, Eissa and Liebman (1996) use a difference-in-difference approach to identify the impact of the EITC on the labor supply of US single mothers. They find compelling evidence that single mothers joined the labor market in response to this incentive. In the UK, Francesconi and Van der Klaauw (2007) use changes in the generosity of the WFTC for the same purpose. Closest to us, Lemieux and Milligan (2008) exploit the fact that, prior to 1989 in Quebec, unattached persons younger than 30 years old received substantially less in welfare payments than similar individuals aged 30 years old or more. Using a RD design, they find that more generous transfers reduce employment. A similar analysis is conducted for Denmark by Jonassen (2013), with an age condition at 25.

We exploit a similar discontinuity, drawing on the detailed RD analysis conducted in Bargain and Doorley (2011) for the year 1999. It pertains to the fact that childless single individuals under 25 years of age were not eligible for the main social assistance program in France (RMI).[2] Interestingly, this policy feature concerns a group which is rarely studied in the literature. Childless singles are seldom concerned by welfare reforms in the US or the UK (changes in the EITC or the WFTC most often concerned couples or single individuals with children). It is an important group, however, given the increase in its relative population share. Young single individuals also form a group particularly at risk of poverty in France (a rate of almost 11% when the poverty line is half the median income, compared to 6% on average in the population). In general, they have not contributed enough to receive unemployment benefits, they are not eligible for social assistance and their employment rates are relatively low. It is therefore crucial to evaluate the potential increase in inactivity that may follow an extension of social transfers to the under 25s, as motivated in the introduction.

[2] In the same line of research, Chemin and Wasmer (2012) use the French labor force survey (LFS) and a triple-difference approach to exploit the fact that the Alsace region in France already had a system of social assistance before the RMI was introduced all over the country. Their estimates of the disincentive effect corroborate those in Bargain and Doorley (2011).

2.2 Structural Labor Supply Models

A very large number of policy studies have relied on structural models estimated on cross-sectional data to analyze current or hypothetical tax-benefit policies (see for instance the discussion in Blundell and MaCurdy, 1999). As argued above, the internal validity of these models is not, however, guaranteed. The main identification issue concerns omitted variables (e.g., being a "hard working" person) that can positively affect gross wage rates and consumption-leisure preferences simultaneously. If cross-sectional variation in gross wages is endogenous to preferences, it cannot be directly used to infer potential responses to tax and benefit incentives. In early models, identification was obtained from exclusion restrictions and hinged on the validity of instruments (e.g., Hausman, 1981, for the US or Bourguignon and Magnac, 2001, for France).

A few studies have used grouped data estimations of the correlation between hours (or participation) and wages over a long period to address the problem of wage endogeneity (Pencavel, 2002, Devereux, 2003, 2004, Blundell et al., 1998). Most of the recent studies are more focused on the ability to perform simulations of topical reforms and, hence, still rely on cross-sectional data for recent years. Many of them have adopted discrete choice models that allow the incorporation of the complete effect of tax-benefit policies on household budget constraints. Thus, identification can be obtained from exogenous variation in tax-benefit rules across regions (e.g., across US states in Hoynes, 1996) or, when several years of data are available, over time (e.g., Blundell et al., 1998). In most cases, however, in the absence of time or spatial variation, models are identified by non-linearities and discontinuities introduced to budget curves by tax-benefit rules, combined with demographic variation (e.g. Laroque and Salanié, 2002, for France, van Soest, 1995, for the Netherlands). Two persons with identical gross wages and characteristics will face different *effective* tax schedules if, say, one has two children and the other has three, simply because child benefits or tax allowances vary with gross income. This type of identification is parametric (since demographics also affect labor supply directly) and relies on implicit exclusion restrictions (e.g., the number of children affects preferences linearly while the specific switch from two to three children only impacts the budget constraint through discontinuous child-related policies). In our study, the RMI age discontinuity plays a similar role: it identifies the effect of financial incentives while preference parameters vary *continuously* with age. Notice that this exclusion restriction seems more reasonable in our case. First, age is a dimension over which individuals have no control (in contrast to fertility or marital status). Second, there is no reason for preferences to vary discontinuously with age. As a matter of fact, this is exactly the identifying assumption used in related RD studies (as Lemieux and Milliagan, 2008) and, as far as we know, this link has never been made in the literature.

2.3 Comparing or Combining Approaches

Comparing methods is important. Lalonde's (1986) landmark paper studied the ability of econometric methods to replicate experimental results. In the same vein, comparisons of randomized or quasi-experiments with the predictions of structural models are useful. In the labor supply literature, a few studies have compared the predicted employment effect of tax-benefit policies using discrete choice models to the actual effect as evaluated by different techniques including difference-in-difference (Blundell, 2006, Cai et al., 2007, Pronzato, 2012, Geyer et al., 2015), tax responsiveness (Thoresen and Vattø, 2015), regression discontinuity (Hansen and Liu, 2011) or randomised experiments (Todd and Wolpin, 2006). Evidence is mixed: while most of these studies point to the satisfying performance of structural models, others do not (especially Choi, 2011 and Keane and Wolpin, 2007). Another way to look at it is to acknowledge that ex post and ex ante evaluation approaches can be fruitfully combined, as discussed in the introduction. A few studies have explored the benefits of (quasi)experiments to identify structural models.[3] In this study, we suggest to "add structure" to the RD design by making the underlying static labor supply logic explicit. Our structural model is identified using the same policy discontinuity but can also be used to simulate any policy reform and to perform out-of-sample prediction. Comparing the predicted employment effect of the RSA reform to the actual effect allows us to check the external validity of the model.[4]

3 Institutional Background and Data

3.1 Institutional Background

RMI and RSA. The policy we study, the RMI, acted until 2009 as a 'last resort' benefit for those who are ineligible for (or have exhausted their right to) other benefits in France.

[3] Imbens (2010) cites an early example, Hausman and Wise (1979), who estimate a model for attrition using a randomized income maintenance experiment. Recent examples include Card and Hyslop (2005), who estimate welfare participation using experimental data from Canada; Todd and Wolpin (2003), who analyze data from Mexico's Progresa program; Attanasio et al. (2011) who also analyze the effect of Progresa on education choices; or Imbens, Rubin and Sacerdote (2001) who estimate labor supply models, exploiting random variation in unearned income using data from lottery winners.

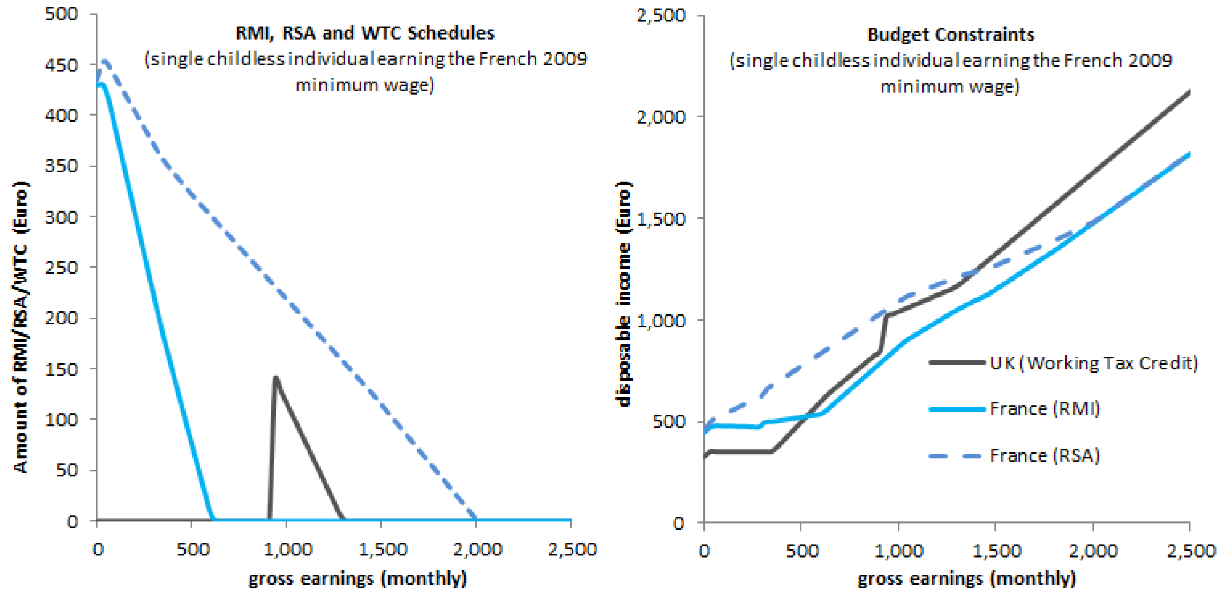
[4] Note that another advantage of the structural approach is the ability to perform welfare analysis (beyond the mere analysis of income distribution). In practice, welfare evaluation involves a certain degree of interpersonal comparability or the derivation of money metric measures, which pose several challenges (see for instance Eissa et al., 2008, Decoster and Haan, 2015, and Bargain et al., 2013, for recent welfare analyses using labor supply models). For this reason, and because our main exercise pertains to the comparison with a-theoretical RD, we do not undertake welfare analysis in the present study.

We describe here the relevant situation for the year studied, 1999, but the situation for the *workless* poor is almost unchanged by the 2009 RSA reform that we describe and simulate below (the RSA simply adds a substantial in-work transfer to the *working* poor). The RMI can be claimed by any French resident, aged at least 25 (or aged under 25 with a dependent child) and not in school. The RMI is often complemented by means-tested housing subsidies which, together with the RMI, almost lift a workless poor person to the poverty line at 40% of median equivalized income. Although, in theory, eligibility for the RMI is conditional on signing an integration contract, in practice, entitlement to the RMI does not include any obligation to actively seek work or to train. In addition, the RMI is time unlimited. Denote R the maximum amount of RMI that a single individual can obtain and $S(E)$ the amount of housing subsidy she can obtain as a function of her earnings, E. As a simplification, we can define this person's disposable income as $C(E;A) = S(E) + max(0, R - t.E).1(A \geq 25)$ with A denoting age in years and t the taper rate of RMI. Specifically around the age cutoff and for someone out of work, $C(0;25) = S(0) + R$ is around 150% larger than $C(0;24) = S(0)$. After a short period, during which it is possible to cumulate earnings and some RMI, the withdrawal rate t becomes 100%. This confiscatory implicit taxation on earnings is expected to discourage participation, especially among those with weak attachment to the labor market and low wage prospects (see Gurgand and Margolis, 2008, Bargain and Doorley, 2011, Wasmer and Chemin, 2012). The system prevailing after 2009, the RSA, introduces an in-work transfer by permanently reducing the taper rate t from 100% to 38%. The age condition is maintained as well as other characteristics like the absence of a time limit.

Graphical Illustration. Figure 1 aims to clarify the impact of these redistributive schemes on living standards and to compare them with an international reference point. We first compare the RMI schedule (2009 parameters), the RSA schedule (parameters after reform in 2009) and the schedule of the British Working Tax Credit (WTC), for a single childless individual paid at the French hourly minimum wage and assumed to be eligible for these transfers (i.e. above 24 years old). The WTC is used for comparison since it also targets *childless* single individuals in the UK (contrary to the US EITC or the pre-2003 British WFTC, which are both targeted at couples or individuals with children only). These counterfactual simulations are obtained using the tax-benefit microsimulation EUROMOD, which reproduces the tax-benefit rules for several European countries including France and the UK.

The left-hand side graph in Figure 1 shows the level of benefit received compared to gross earnings (before tax and employee social security contributions) for each of the three benefits. We note that the RSA schedule is particularly generous for a minimum wage

Figure 1: Schedules of Alternative Redistributive Schemes and Budget Constraints



worker at full-time (gross earnings of around EUR 1,400 per month). The WTC for single individuals without children is paid to those working at least 30 hours per week, which explains why it begins at just below 1,000 EUR per month in our example. Although its taper rate (37%) is comparable to that of the RSA (38%), a housing allowance is deductible from the RSA amount before the taper rate is applied, leading to an effective withdrawal rate lower than that of the WTC. The right-hand side graph of Figure 1 represents the level of disposable income (taking each country's tax-benefit system into account) against gross earnings. Compared to the RMI regime, the RSA reform clearly increases the disposable income differential between a full-time work and being out-of-work. Interestingly, in the range of EUR $1,000 - 1,500$ of gross earnings where many low-paid individuals are to be found, both the French RSA and the British WTC regimes provide a similar level of net resources (despite different levels of transfers and because of generous tax free allowances in the UK, which allow very low income people pay no tax).

3.2 Data and Sample Selection

Datasets. RD estimations must rely on very large samples. With standard survey data, age cells would become too small for meaningful analysis. For this reason, we pursue both the RD analysis and the structural model estimation using the French *Census Data* for the year 1999. Its coverage is universal and samples of 1/4 of the population are publicly available from INSEE, corresponding to around 14.5 million people. Previous Census, 1982 and 1990, cannot be used since they correspond to years before the introduction of the RMI (1989) or just after (a period with few recipients). Our data for 1999 corresponds to a peak year, with around one million RMI recipients, following a gradual expansion of the scheme over the 1990s (see Bargain and Doorley, 2011). We shall also check the external validity of our model using Census data for years 2004-11.[5]

The Census provides data on age (in days), employment, type of contract, work duration, marital status and household type. Data on income and receipt of RMI or other benefits is, unfortunately, not available. Wage estimations are, therefore, conducted using the *Enquête Emploi*, i.e. the French Labor Force Survey (FLFS hereafter). This is a panel survey conducted on an annual basis for the period 1990-2002. For cross-sectional use, the annual FLFS is a representative sample of the French population, with a sampling rate of 1/300, providing information on employment, labor income (base salary plus all bonuses, overtime payment and in-kind advantages), education and demographics. Hence, it is possible to calculate hourly wages and estimate wage equations on key variables.

Sample Selection. The sample selection is applied to both Census and FLFS data. We retain individuals aged 20-30 who are potential workers, i.e., not in school, in the army or living on a (disability) pension. Our analysis focuses on *singles without children*. This group is of primary interest as it represent the main group of RMI claimants. Contrary to couples, whose joint labor supply decision is a relatively complicated problem, they also allow for a clear interpretation of the potential labor supply effects. Discarding individuals with children is due to the fact that a parent is eligible for the RMI regardless of age. Finally, and differently from Bargain and Doorley (2011), we consider both female and male singles, as well as all education categories. We also present results for a specific group, the *high school (HS) dropouts*, who have the lowest financial gains to work in the short term and, possibly, weaker attachment to the labor market. They represent 22% of the population of young singles aged $25-30$ but are over-represented among single RMI recipients in this age range, accounting for 52% of this group.

Wage and Incomes. FLFS and Census data are used to estimate and predict wage rates respectively. Wage estimations and the robustness of wage predictions are extensively discussed in Appendix A.1. Both Census and FLFS data have identical definitions of the key variables used to estimate wage rates and, in particular, education categories.

[5] Census data collection became annual starting in 2004 and now covers the whole population over a five-year period. Because of limited data access, we could not carry out our main analysis on waves 2004-08 (before the RSA reform). We could only avail of employment rates by age for 2004-2011 Census data, which we use for external validity checks hereafter.

Table 1: Summary statistics for single childless 20-30 year olds in the Census and LFS

	All		Under 25		Over 25	
	Census	FLFS	Census	FLFS	Census	FLFS
Proportion of men	0.56	0.57	0.51	0.51	0.58	0.60
Age	26	27	23	23	27.5	27
Education:						
Junior vocational qualification	0.26	0.25	0.29	0.26	0.24	0.24
Highschool	0.06	0.07	0.07	0.08	0.05	0.06
Vocational highschool	0.13	0.13	0.17	0.17	0.12	0.11
Graduate qualification	0.39	0.38	0.28	0.29	0.43	0.41
Dropouts	0.16	0.17	0.19	0.19	0.15	0.17
Work hours	30	26	29	31	31	32
Employment rate	0.81	0.81	0.79	0.81	0.82	0.81
Employment income*	1,534	1,429	1,392	1,228	1,583	1,510
Disposable income*	**1,032**	**1,136**	**893**	**926**	**1,081**	**1,217**
Sample size	202,093	2,040	53,048	570	149,045	1,470

Note: selection of childless single individuals between 20-30 years old. Data sources are the 1999 Census and Labor Force Survey (FLFS). Disposable income is calculated using labor income and the EUROMOD tax-benefit simulator on the data. In Census data, we predict wages using estimations conducted on FLFS data. All monetary variables are expressed in 1999 EUR/month. Employment income excludes zeros. Disposable income is found to be positive for

* All monetary variables are expressed in 1999 EUR/month.

Table 1 provides descriptive statistics of both datasets. It shows that the two selected samples are comparable in terms of demographic and education structures, which gives us confidence in the wage imputation. We also report the mean levels of simulated disposable incomes, calculated for each individual in the data as a function $C(E;A)$ of gross income E (it is also conditional on age A given RMI rules).[6] This function accounts for social contributions and taxes paid on income E as well as benefits received, which we approximate by very detailed numerical simulation of the French tax-benefit rules.[7] Simulated transfers essentially consist of the RMI (a function of age A) and housing benefits for our selection of childless single individuals, and child-related benefits for the broader population. Table 1 shows that the average levels of simulated disposable incomes line up quite closely in the two datasets.[8]

[6] Capital income is ignored as very small amounts are reported in this age group, especially for the low-educated youths that we focus on. Hence, gross income E corresponds essentially to earnings, i.e. actual earnings as observed in the FLFS or predicted earnings for all observations in the Census (actual work duration$\times$predicted wages).

[7] As explained below, tax-benefit simulations are also used to calculate, for each individual, disposable incomes $C(wH;A)$ at different worked hours H (zero and full-time) using imputed wages, for the purpose of estimating the structural participation model.

[8] Additional material, available from the authors compares the employment-age patterns within the two data sources, using the ILO definition in both cases, for people aged 20-30 (see also Bargain and

4 Empirical Approach

We first discuss how the age discontinuity in the RMI program can be exploited to measure the disincentive effect of this welfare scheme on labor market participation using RD. Then we turn to the structural model. Note that we are especially interested in estimating specific participation effects of the RMI for HS dropouts. Indeed, uneducated workers not only have lower wage prospects but also have a weaker attachment to the labor market and, hence, larger search costs (see Beffy et al., 2006, and Gurgand and Margolis, 2008). Hence, all the coefficients of the models presented below, both RD and behavioral models, will vary with a "HS dropout" dummy (this will not be indicated in order to lighten the notation).

4.1 RD Design

We start from Rubin's framework, denoting Y_i^* the propensity to be in work and T_i the treatment variable for each unit i. Here, being treated refers to the possibility of availing of the welfare program. As in Lemieux and Milligan (2008), this is simply determined by the age eligibility condition for the program, that is, $T_i = I(A_i \geq \underline{A})$ with A_i the forcing variable (age) and $\underline{A}$ the age limit. We know exactly what age people are at Census day and their employment status at that date. Consequently, and because the treatment variable is a deterministic function of age, we are in the presence of a "sharp" RD design. We denote Y_{i1}^* the potential outcome (participation decision) if exposed to treatment, i.e. if in the eligible age range, and Y_{i0}^* the potential outcome otherwise. Considering age in days as a continuous variable, we can make the usual assumption:

Condition 1 *(local continuity) The mean values of Y_1^* and Y_0^*, conditional on A, are continuous functions of A at $\underline{A}$.*

Condition 1 leads to a measure of the average treatment effect of the program at $\underline{A}$ as captured by any discontinuity of the outcome at this threshold:

$$ATE(\underline{A}) = \lim_{A \to \underline{A}^+} E(Y_1^*/A = \underline{A}) - \lim_{A \to \underline{A}^-} E(Y_0^*/A = \underline{A}).$$

Note that RD graphical analyses usually require large enough cells, so that age in *years* is commonly used (e.g. Lemieux and Milligan, 2008). Another reason for not usually relying on age in days is that it it not clear when the potential labor supply response

Vicard, 2014). The FLFS shows larger employment rates (as reflected in the average employment figures in Table 1), a discrepancy that becomes smaller for older age groups. Given the smaller sample size of the FLFS, employment levels by age also show a slightly more erratic pattern in these surveys. The overall trends are, however, very similar.

would occur after turning 25. Individuals who were working before their 25th birthday may not be aware that the RMI is means tested on the income earned during the three months prior to the claim.[9] Hence, we adopt age in years but shall nonetheless provide sensitivity checks using age in quarters or months. With a discrete running variable A (like age in years), we cannot compare observations "close enough" on both sides of the cutoff point to be able to identify the effect. Parametric assumptions are required in this case. Hence, we specify the RD model as:

$$Y_i^* = \alpha_i^0 + \alpha^1(A_i) + \beta_i I(A_i \geq \underline{A}) + \varepsilon_i. \tag{1}$$

This also allows using a large sample strategy, i.e. observations further away from the cutoff, to gain precision. With employment $Y_i = 1$ for those with $Y_i^* > 0$ and 0 otherwise, this model is easily estimated by logit or probit techniques. The effect of age A_i on the outcome variable is captured by function $\alpha^1(A_i)$ and by $T_i = I(A_i \geq \underline{A})$. The parametric version of Condition 1 requires that $\alpha^1(A_i)$ *be a smooth function of age close to* $\underline{A}$. Under this condition, the treatment effect β is obtained by estimating the discontinuity in the empirical model at the point where the forcing variable switches from 0 to 1. Given the discrete nature of the forcing variable, we use alternative parametric forms for $\alpha^1(A_i)$ in order to balance the usual trade-off between precision and bias (see Lee and Lemieux, 2010). Note that coefficients α_i^0 and β_i bear a subscript i as they can vary linearly with individual characteristics other than age. In practice, there is little demographic variation left except gender.

4.2 Adding Structure

General Model. The interpretation of a potential disincentive effect of social assistance in the above RD design coincides with the rationality assumed in static labor supply models. In the discrete version of these models (for instance, van Soest, 1995), agents are supposed to choose a work option $j = 1, ..., J$ in a set of J common work durations (for instance non-participation, part-time, full-time and overtime). In this setting, we can write utility at choice j as:

$$U_{ij} = U_i(H_j, C(w_i H_j; A_i)) - F_i.1(H_j > 0) + \epsilon_{ij} \tag{2}$$

with disposable income $C(w_i H_j; A_i)$ (equivalent to consumption in this static framework) and worked hours H_j. The deterministic utility levels are completed by i.i.d. error terms ϵ_{ij}, assumed to follow an extreme value type I (EV-I) distribution and to represent possible

[9] Using panel data, Jonassen (2013) actually confirms that the employment drop at the age cutoff corresponds to transitions out of work – and that they occur within 6 months after the 25th birthday.

observational errors, optimization errors or transitory situations. Translog or quadratic utility functions in hours H_j and consumption C are typically used for function U_i (see Blundell and MaCurdy, 1999). Utility is reduced by a level F_i for positive hours choices. This term may capture fixed costs of working (transportation), job search costs, etc., so that it must vary with individual characteristics including age. Bargain (2006) and van Soest et al. (2002) show that it is not possible to identify preferences from other structural components like fixed (or variable) costs of work, unless strong parametric assumptions are made. Acknowledging this, we opt for a flexible specification:

$$U_{ij} = a_{ij}(A_i) + b_{ij}(A_i)C(w_iH_j; A_i) + c_{ij}(A_i)C(w_iH_j; A_i)^2 + \epsilon_{ij} \tag{3}$$

where preference parameters vary themselves with the choice j.[10] The term a_{ij} entering utility in an (additive) separable way may capture work preferences, fixed costs of work and search costs, all possibly varying with age. Contrary to the RD model in which β_i captures the average treatment effect through the age condition, parameters on income may also vary with age as they have a different, more structural interpretation as components of the marginal utility of income. Coefficients in (3) also bear subscript i as they shall vary linearly with basic taste-shifters (gender) and, possibly, random terms for unobserved heterogeneity. We detail the specification below.

Participation Model. This structural labor supply model is widely used for policy analysis (see Blundell and MaCurdy, 1999, for a survey). As previously discussed, identification often relies on nonlinearities/discontinuities in tax-benefit rules. In our setting, we use the age condition in social assistance eligibility, creating exogenous variation in financial incentives at the age cutoff, as the key source of identification. Since this discontinuity affects only the financial difference between working and not working, we shall focus on the *participation* margin. The choice of working ($j = 1$) rather than staying out of the labor market ($j = 0$) depends only on the difference $Y_i^* = U_{i1} - U_{i0}$. Then coefficients on consumption are identified but only the difference $a_i = a_{i1} - a_{i0}$ is identified for the constant. The propensity to be employed is written as:

$$Y_i^* = a_i(A_i) + b_{i1}(A_i)C(w_iH_1; A_i) - b_{i0}C(0; A_i) + \epsilon_i \tag{4}$$

with $\epsilon_i = \epsilon_{1i} - \epsilon_{0i}$. We exclude the quadratic term in consumption to simplify the model. That is, participation decisions are assumed to depend on gains to work, i.e. the difference between disposable income when employed, $C(w_iH_1; A_i)$, and disposable income when out

[10] In this way, the "disutility" of work or other components like work costs are specified through choice-specific terms a_{ij}, b_{ij} and c_{ij} so they are not forced to vary in a restricted way with H_j, as in standard functional forms.

of work, $C(0; A_i)$. Yet we do not impose $b_{1i} = b_{0i}$ for flexibility. The model resembles the RD equation (1). In particular, the first term is written:

$$a_i(A_i) = a_i^0 + a^1(A_i)$$

with $a^1(\cdot)$ a smooth function of age similar to $\alpha^1(\cdot)$ in the RD design. Yet, the model captures the discontinuity effect not through the age condition $I(A_i \geq \underline{A})$ but with more structure, through the specification of financial gains to work. The term b_{i0} does not vary with age because disposable income out of work $C(0; A_i)$ is identical for all individuals on the same side of the age threshold. Yet the marginal utility of income "in work" can vary with age, expressed as:

$$b_{i1}(A_i) = b_{i1}^0 + b_1^1(A_i)$$

with $b_1^1(\cdot)$ a smooth function We specify three models. In the first one, b_{i1} does not vary with age (model A) while it varies linearly in the second (model B) and quadratically with age in the third (model C). Coefficient b_{i0}, a_i^0 and b_{i1}^0 are indexed by i as they vary with gender and we make b_{i1} vary linearly with u_i, a random and normally distributed term accounting for unobserved preferences for work (with zero mean and variance σ_u^2).

Identification. First, notice that exclusion restrictions through heterogeneity other than age are very limited. Detailed education groups enter the wage equation (see Appendix A.1) while preference parameters vary only with broad education groups (HS dropouts versus others). Gender necessarily enters both the wage equation and preferences in the structural model. Thus, the main identification comes from variation of the forcing variable age. The model fits observed employment levels on the basis of (i) an additive constant $a_i(A_i)$, which captures non-monetary aspects, and coefficients of the marginal utility of income, both allowed to vary smoothly with age; (ii) heterogeneity in gains to work due to wage rates, which also vary smoothly with age; (iii) sharp variation in 'net' gains to work due to the age condition. Admittedly, the identification of the policy effect *at all ages* is parametric and requires a bit more than in the RD design, which assumed *local* smoothness of $\alpha^1(A_i)$ (around the discontinuity). Precisely, we specify behavioral parameters in (4) (or in the more general model (3)) as *globally* continuous in age:

Condition 2 *(global continuity) Behavioral parameters vary continuously with age.*

This condition allows us to use the model for extrapolation further away from the threshold (the RD treatment effect can be used to extrapolate at different ages only under the assumption of invariance of the RMI effect with respect to age, as in model A). We necessarily rely on parametric assumptions on the way b_i varies with age (linearly in model B, quadratically in model C), yet we shall check if results are sensitive to the specification.

Estimation. The model is estimated as follows. First, wages are imputed for all observations in the Census. This is done by estimating wage equations on FLFS data and predicting wages in the Census (see Appendix A.1, Table A.1 and Figures A.1-A.3). Second, disposable income is calculated for each observation and at each discrete labor supply choice. That is, we use detailed numerical simulation of tax-benefit rules to obtain disposable income when out-of-work, $C(0; A_i)$, and when working full-time, $C(w_i H_1; A_i)$ (we set H_1 to 39 hours per week, the institutionally set full time option in France in 1999). Third, the labor supply model of equation (4) is estimated by simulated maximum likelihood (see detailed estimates in Appendix A.1, Table A.2). Under the assumption that error terms, ϵ_{ij}, follow an EV-I distribution, the (conditional) probability for each individual of choosing a given alternative has an explicit analytical solution, i.e., a logistic function of deterministic utilities at all choices. This multinomial logit model boils down to a simple logit in our case. Because the model is nonlinear, the wage prediction errors (denoted ν_i) are taken explicitly into account for a consistent estimation. The unconditional probability is obtained by integrating out the disturbance terms (u_i and ν_i) in the likelihood. In practice, this is done by averaging the conditional probability over a large number of draws for these terms, recalculating disposable income each time. Finally, the discontinuity in gains to work provides a scaling of employment responses to changes in financial incentives and, hence, the possibility to directly gauge the effect of any reform that alters the gains to work. Precisely, counterfactual policy scenarios are implemented, giving alternative functions $C(\cdot; A)$ and hence new levels of disposable income used to predict the new optimal choice of each individual.

5 Results

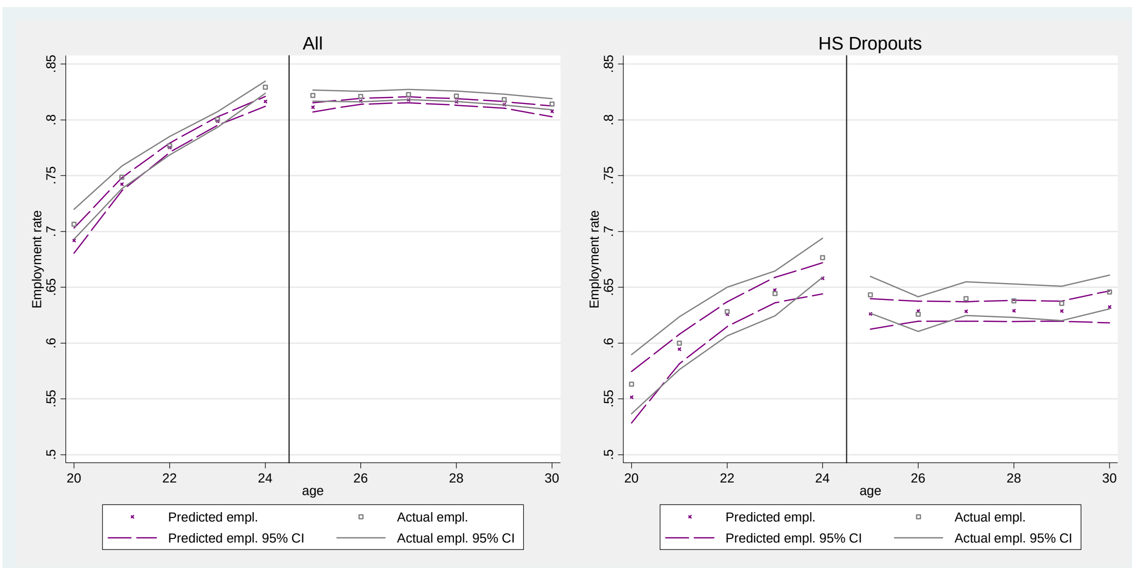
We first check the internal validity of the behavioral model. Then we compare out-of-sample predictions of a reform with the actual effects of this reform, suggesting an informal check of the model's external validity. Finally, we propose a series of policy relevant simulations.

5.1 Estimation Results and Internal Validity

There are two benchmarks against which we can assess the internal validity of the behavioral model: the prediction of actual employment rates at every age and the prediction of the RMI employment effect at the discontinuity.

Employment Rates. Figure 2 plots actual employment levels (squares) at all ages against predicted employment rates (crosses) and their confidence intervals. The solid lines represent the 95% confidence interval from sampling errors when calculating population mean employment rates per age. The dashed lines are the confidence intervals from model predictions, based on estimated preference parameters and standard errors. We rely here on structural model B which includes, in equation (4), a cubic function for $a^1(\cdot)$ and a linear function for $b_1^1(\cdot)$. We distinguish results for the whole selected sample and for HS dropouts, respectively. The model shows a good fit, with actual employment rates contained in the predicted confidence intervals at almost all ages, even further away from the cutoff. Sampling and model confidence intervals actually overlap at every point. We now move to the graphical inspection of the drop at age 25.

Figure 2: Employment Rate of Childless Singles: Discontinuity and Model Fit (Census, 1999)



Note: Actual employment rate from 1999 French Census compared to predicted employment rate using structural model B (sample of 20-30 year old men and women who are available for work).

RMI Employment Effect. With actual employment, we observe a small drop in employment at age 25 for the full sample but a larger decline for HS dropouts (the group combining both low wage prospects and little labor market attachment). Figure A.4 in the Appendix reports non-parametric trends obtained using age expressed in days rather than years, which leads to the same qualitative conclusions. Also, confidence intervals for actual employment in Figure 2 visually convey that the sharp fall in employment at age 25 is statistically significant for HS dropouts. The raw difference $\overline{Y}_{25} - \overline{Y}_{24}$ is -0.7 percentage points (ppt) in the broader group and -3.4 ppt among HS dropouts. At the same time, predicted employment also shows a small decrease at age 25 for all education groups and a significant drop for HS dropouts. Although employment rates are slightly underpredicted at both 24 and 25 years old, the predicted drop in employment levels looks very similar to the actual one.

We now address this comparison using parametric estimations. That is, we suggest a formal comparison of the RMI employment effects predicted by the RD approach and by the model. We use cubic age trends in both approaches, i.e. for both $\alpha^1(\cdot)$ and $a^1(\cdot)$. Starting with the RD estimation, the first column of Table 2 reports estimates of β_i of -1.6 ppt for the broader group and of -3.9 ppt for HS dropouts, both statistically significant. Hence, we confirm the substantial negative effect of the RMI on childless singles, especially in the case of unskilled workers. The next columns of Table 2 show the employment effect as predicted by the structural model. It corresponds to the drop in employment corrected by the age trends in wages and in behavioral components around the cutoff (see the characterization in Appendix A.3). Estimates of columns 2-7 are obtained for the different specifications previously described. Model A is the most comparable to the RD model as age is excluded from the marginal utility of consumption. That is, age smoothly affects "preferences" only through the additive term a_i, as in the RD design. Variants A2 and A3 use information on age in quarters and months respectively, rather than age in years.[11] In models B and C, the individual's valuation of the monetary gains from work varies linearly and quadratically with age, respectively. Finally, in model D, we rely on wage predictions based on a more parsimonious specification of the wage equation (see right panel of Appendix Table A.1). The aim is to check if results are sensitive to the implicit exclusion restriction made in our baseline, i.e. having detailed education categories in the wage equation but only a HS dropout dummy in the preference parameters. The RMI employment effects predicted with these different behavioral models are well in line with the RD results, i.e. around -1.5 to -1.6 and -3.6 to -3.9 ppt for the whole selected sample and for HS dropouts respectively.[12] This good fit was expected since the model is identified using the same discontinuity as the RD design.

[11] Note that the forcing variable, age, can be treated more as a continuous variable in this case, so extrapolations around the discontinuity are less dependent on the parametric form. It also generates more noise given smaller age cells, which is not a problem for the fit of the structural model.

[12] We observe slightly more homogenous results across gender groups for the whole sample compared to RD estimates. For HS dropouts, however, the structural model predicts the larger effects for men well. Alternative specifications for $\alpha^1(\cdot)$ and $a^1(\cdot)$, either quadratic or quartic, do not affect our conclusions qualitatively, even if small quantitative differences are observed. For HS dropouts, this can be seen in Table 3 below. Compared to baseline results using cubic forms, we observe larger effects for men, and larger (smaller) effects with the quadratic (quartic) form for women. Yet this is true with both approaches, i.e. comparing columns (1) and (2) confirms that RD estimates and model predictions are very similar in all specifications.

Table 2: Employment Effects of the RMI: RD vs. Structural Model

RMI Effect	Regression Discontinuity (RD)	Behavioral Model						Out-of-sample Predictions		Behavioral Model on a Larger Sample	
		A	A2	A3	B	C	D	RD	Model B	A' 20-24	A' 25-30
All Education Groups											
All	-1.6 ***	-1.5 ***	-1.6 ***	-1.6 ***	-1.5 ***	-1.5 ***	-1.5 ***	-1.1 *	-1.9 ***	-5.6 **	1.9
	(0.4)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.4)	(0.6)	(0.7)	(2.3)	(1.6)
Male	-0.7	-1.7 ***	-1.8 ***	-1.8 ***	-1.6 ***	-1.5 ***	-1.8 ***	-1.7 **	-2.0 ***	0.5	11.1 ***
	(0.6)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.7)	(0.8)	(3.3)	(2.9)
Female	-2.5 ***	-1.3 **	-1.4 **	-1.4 **	-1.5 **	-1.5 **	-1.1 **	-0.4	-1.8 **	-13.0 ***	-9.2 ***
	(0.7)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.5)	(0.7)	(0.8)	(2.0)	(0.8)
HS Dropouts											
All	-3.9 ***	-3.9 **	-3.6 ***	-3.6 **	-3.9 **	-3.9 **	-3.9 ***	-3.5 **	-4.1 *	-7.7 **	-0.2
	(1.4)	(1.5)	(1.4)	(1.5)	(1.5)	(1.5)	(1.4)	(1.6)	(2.1)	(3.9)	(2.7)
Male	-4.2 **	-4.5 ***	-4.2 **	-4.2 ***	-4.5 ***	-4.5 ***	-4.6 ***	-4.1 **	-4.9 **	-1.0	9.4 ***
	(1.8)	(1.6)	(1.7)	(1.6)	(1.6)	(1.6)	(1.5)	(1.7)	(2.2)	(3.5)	(2.9)
Female	-3.4	-2.9	-2.5	-2.5	-3.0	-3.0	-2.7	-2.5	-2.8	-18.7 ***	-15.8 ***
	(2.4)	(1.9)	(2.3)	(1.9)	(2.0)	(2.0)	(1.8)	(1.8)	(2.5)	(4.6)	(2.4)

The employment effect of the RMI is estimated using the RD design or the behavioral model. Both approaches rely here on a cubic age specification for the additive term (α in RD and a^1 in the model). RMI employment effects for different gender/education groups are predicted using demographic heterogeneity in the model (i.e. we do not estimate separate models for different gender/education groups). All figures are based on the 1999 Census data (for behavioral models, wages are imputed using estimations on the French Labor Force Survey). Behavioral model A omits age in the marginal utility of income while the latter varies 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). Model D is the same as model A but is estimated on data in which wages are imputed according to a less parsimonious definition of education categories. Out-of-sample predictions are performed on 50% of the sample using the other 50% for estimating the model. Models A' 20-24 and A' 25-30 are similar to model A but estimated on a larger sample of single individuals (with and without children) in the age range as indicated, hence not relying on the 25 year-old discontinuity for identification (note that the non-monetary propensity to work is allowed to vary with the number of children), and predictions reported here are for the usual sample of childless singles. Estimates significant at the 1%,5% or 10% levels are indicated using ***, ** and * respectively. Standard errors are reported in brackets.

Sensitivity Analysis. We suggest a series of checks regarding model specification and out of sample predictions. First, we perform a log-likelihood test between models A and B. With a LR statistic of 112 and a chi2 of 9 at the 1% level, we reject model A (the same is true for A versus C). Hence, age significantly alters the marginal utility of income. Admittedly, the magnitude of this age effect is small compared to the way age affects the additive term.[13] This explains why there are only small differences between estimates of models A, B and C in Table 2.

Second, previous results are obtained with a cubic form for both $\alpha^1(\cdot)$ and $a^1(\cdot)$. For HS dropouts, we check the sensitivity to other functional forms in Table 3. Column (1) shows that RD estimates of β_i range between 3.9 and 5.8 percentage points over all specifications: $\alpha^1(\cdot)$ as quadratic, cubic or quartic. Hence, there is some variation in the magnitude of the treatment effect according to the specification used. Fortunately, in columns (2), we observe the same variation in the model prediction across the different specifications of $a^1(\cdot)$ (and this is true for all three models A, B, and C).

[13] For an educated worker aged 24, the marginal effect of age on employment due to cubic $a^1(\cdot)$ is 6 times larger than the effect due to linear $b_1^1(\cdot)$ (in model B). The same conclusion is reached with quadratic rather than cubic $a^1(\cdot)$, or with quadratic rather than linear $b_1^1(\cdot)$.

Third, we also check that the structural model does not overfit the data, which would limit its external validity. We estimate the model on a random half of the selected sample (estimation sample), and use estimates to predict employment rates and treatment effects on the other half (holdout sample). Results in the columns 8-9 of Table 2 show that the treatment effect on the holdout sample, measured by RD, is again very similar to what was found for the full sample (-1.1 and -3.5 for the whole selection and for HS dropouts respectively). The participation model seems to perform relatively well, even if treatment effects are larger than the RD estimates (-1.9 and -4.1 respectively). In line with the RD results, the model points to larger responses by single men compared to single women, especially among HS dropouts.

Age Discontinuity versus Traditional Identification. Finally, we would like to stress the role of the discontinuity in estimating the RMI effect on the probability of working. As said in section 2.2, the more traditional type of cross-sectional identification pertains to nonlinearities from tax-benefit policies combined with demographic variation. In order to see how the structural model identified using the discontinuity alone performs in comparison to the traditional approach, we suggest the following. We estimate a model A' on the whole population of single individuals – including single parents – aged 20-24. Hence, this model is identified by typical cross-sectional variation like family composition, but not on the age discontinuity. Symmetrically, we replicate the estimation on all singles aged 25-30, again not using the age discontinuity.[14]

Results are displayed in the last two columns of Table 2, showing how estimates of the RMI effect differ when adopting a structural model identified without using the discontinuity but more typical cross-sectional exclusion restrictions. It appears that Model A'20-24 points to the correct sign overall and for women, i.e. a negative employment effect, yet with magnitudes that are far off the mark compared to the RD estimates and, most importantly, to the model estimated using the discontinuity. Model A'25-30 performs slightly better for women but predicts a positive effect for men and, as a result, an insignificant overall effect.[15] We conclude that, as suspected in the discussion of section 2.2., using the number of children to identify the structural model does poorly compared to using the discontinuity.

[14] Note that model A' resembles our baseline model A but is augmented with the number of children entering the non-monetary preferences for work (detailed coefficients available on request). Child-related policies also enter the picture by affecting the budget constraint of single parents in the enlarged sample.

[15] These bad performances are not due to the fact that model A' is estimated on a narrower age range. Additional comparisons – unreported but available from the authors – show that model A estimated on a sub-sample of childless singles aged 20-25, hence incorporating the age condition but fewer years, performs almost as well as the baseline models regarding the RMI employment effect.

5.2 External Validity: Predicting the Effect of the 2009 Reform

We now address the predictive power of the model. Extrapolations rest on the capacity of the discontinuity to capture the essential aspects of work preferences and on the assumption that these preferences do not change radically over time. External validity checks consist of comparing model predictions of policy reforms with what effectively happened after these reforms. More precisely, we simulate the 2009 RSA reform, which essentially reduced the withdrawal rate t from 100% to 38%, introducing a generous in-work-benefit component targeted at the working poor. This fundamental reform of the French redistributive system was broadly inspired by similar policies such as the EITC in the US and the WFTC in the UK (see Immervoll et al., 2007).

Table 3: External Validity: Employment Effect of the RSA Reform (HS Dropouts)

	RD (1999)	Model A (1999)			Model B (1999)			Model C (1999)			RD (2004-11)		
	RMI effect	RMI effect	RSA effect	Diff.	RMI effect	RSA effect	Diff.	RMI effect	RSA effect	Diff.	RMI effect (2004-08)	RSA effect (2010-11)	Diff.
	(1)	(2)	(3)	(3) - (2)	(2)	(3)	(3) - (2)	(2)	(3)	(3) - (2)	(4)	(5)	(5) - (4)
Quadratic specification for α^1 and a^1													
All	-5.8	-5.4	-2.4	**3.0**	-5.4	-2.4	**3.0**	-5.5	-2.4	**3.1**	-3.6	-0.8	**2.8**
	(1.1)	(1.4)	(1.4)	(2.0)	(1.4)	(1.4)	(2.0)	(1.4)	(1.4)	(2.0)	(1.0)	(1.5)	(1.4)
Men	-5.8	-6.0	-3.4	**2.6**	-6.0	-3.3	**2.6**	-6.0	-3.3	**2.7**	-3.1	-0.6	**2.6**
	(1.9)	(1.5)	(1.5)	(2.2)	(1.6)	(1.5)	(2.2)	(1.6)	(1.5)	(2.2)	(1.3)	(2.2)	(2.0)
Women	-4.2	-4.5	-0.8	**3.7**	-4.6	-0.9	**3.7**	-4.6	-0.8	**3.7**	-5.0	-1.2	**3.8**
	(1.8)	(1.9)	(1.9)	(2.7)	(1.9)	(1.9)	(2.7)	(1.9)	(1.9)	(2.7)	(1.5)	(4.9)	(4.2)
Cubic specification for α^1 and a^1 (baseline)													
All	-3.9	-3.9	-0.9	**3.0**	-3.9	-0.9	**3.0**	-3.9	-0.9	**3.1**	-2.6	0.8	**3.4**
	-(6.7)	(1.5)	(1.5)	(2.2)	(1.5)	(1.5)	(2.2)	(1.5)	(1.5)	(2.2)	(1.6)	(2.3)	(2.1)
Men	-4.2	-4.5	-1.9	**2.6**	-4.5	-1.8	**2.6**	-4.5	-1.8	**2.7**	-2.2	0.8	**3.0**
	-(7.7)	(1.6)	(1.6)	(2.3)	(1.6)	(1.6)	(2.3)	(1.6)	(1.6)	(2.3)	(1.9)	(3.1)	(2.8)
Women	-3.4	-2.9	0.7	**3.6**	-3.0	0.7	**3.7**	-3.0	0.8	**3.7**	-3.5	1.3	**4.8**
	-(8.1)	(1.9)	(2.0)	(2.8)	(2.0)	(2.0)	(2.8)	(2.0)	(2.0)	(2.8)	(1.9)	(6.1)	(5.2)
Quartic specification for α^1 and a^1													
All	-4.5	-4.6	-1.3	**3.3**	-4.6	-1.2	**3.4**	-4.6	-1.2	**3.5**	-2.9	0.2	**3.0**
	(1.6)	(1.4)	(1.8)	(2.3)	(1.4)	(1.8)	(2.3)	(1.4)	(1.8)	(2.3)	(1.5)	(1.4)	(2.1)
Men	-6.2	-5.2	-2.3	**2.9**	-5.2	-2.2	**3.0**	-5.2	-2.1	**3.0**	-2.6	0.2	**2.8**
	(2.0)	(1.5)	(1.9)	(2.4)	(1.6)	(1.9)	(2.4)	(1.6)	(1.9)	(2.4)	(1.9)	(2.4)	(2.3)
Women	-2.2	-3.7	0.3	**4.0**	-3.8	0.3	**4.1**	-3.8	0.4	**4.1**	-3.7	0.2	**4.0**
	(2.6)	(1.9)	(2.2)	(2.9)	(1.9)	(2.2)	(2.9)	(1.9)	(2.2)	(2.9)	(1.9)	(5.6)	(4.8)

The employment effects of the RMI in 1999, the RMI in 2004-08 and the RSA in 2010-11 are estimated using the RD design on Census data from these different periods. Behavioral models (versions A-C) are estimated on Census 1999 and used to predict employment effects of both RMI and RSA. RD and structural models include an additive function of age (quadratic, cubic or quartic specification). In addition, models B and C include a linear and quadratic form of age, respectively, in the marginal utility of income. Selection: childless single individuals aged 20-30, HS dropouts. Differential effects ("Diff.") reflect the re-employment impact of the RSA compared to the RMI (the former incorporates an in-work benefit). Models predict this differential effect on the basis of estimates on Census 1999 while RD 2004-11 show the actual differential effect around the year (2009) when the RSA was actually implemented in replacement of the RMI. Standard errors in brackets.

Focusing on HS dropouts, the actual employment levels for 2004-2011 are shown in Figure A.5 using Census data for these more recent years. As discussed in the last paragraph of section A.2 in the Appendix, employment levels have declined since 1999 and particularly after the Great Depression. Importantly, we observe a drop at 25 in the years prior to the 2009 RSA reform – corresponding to the RMI disincentive effect – but no such drop in 2010-11. Hence, it seems that the reform has succeeded in closing the inactivity trap. Columns (4) and (5) of Table 3 report the RD estimates for 2004-08 and 2010-11 respectively. Column (4) confirms the presence of a disincentive effect of the RMI in 2004-08, comparable to that of 1999 (as seen in column (1)).[16] Column (5) also corroborates that there is no significant change in employment at 25 under the RSA scheme, which may reflect the re-incentivizing effect of the in-work component.

Admittedly, our supply-side model is not able to predict employment levels many years after 1999 – and certainly not the overall decline in employment following the Great Depression. Yet, what matters is that the new labor market conditions should be similar just above and below age 25, so that the way the reform differentially affects the two age groups should translate into a reasonable prediction of the new labor supply differential around the cutoff. To check this, we use the model to predict the impact of the RSA reform on employment using 1999 Census data. Figure 3 shows a small positive effect on the over-25 employment rates for the whole selection and a larger re-employment effect above 25 years old for HS dropouts (there is no effect for the under-25 because the age condition is maintained under the new scheme). Hence, our simulations clearly indicate that the inactivity trap is eliminated under the RSA policy.[17]

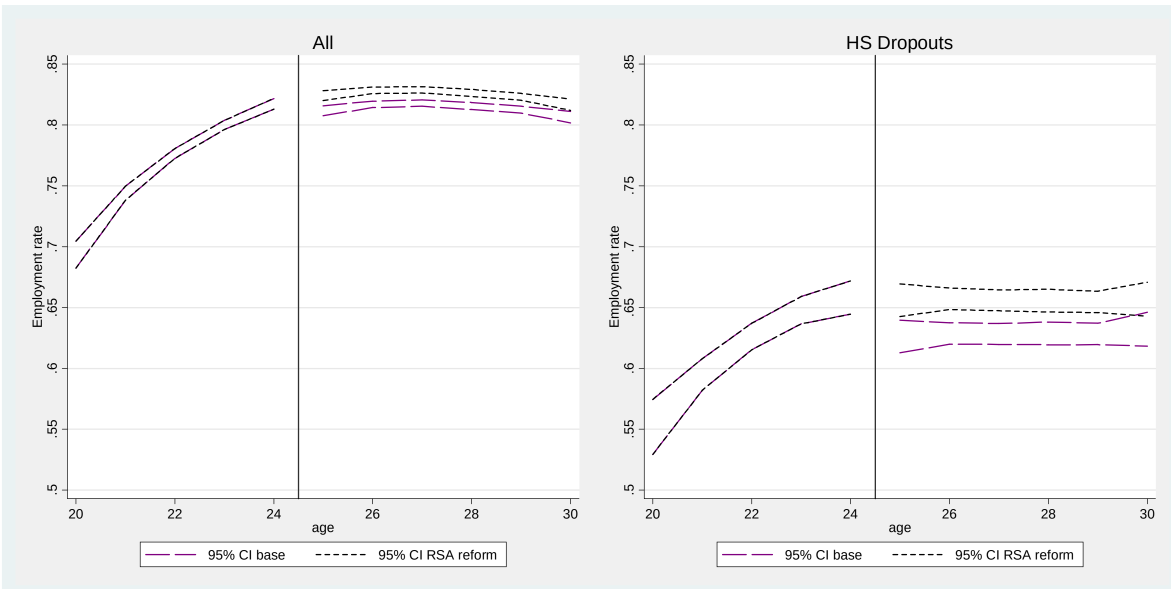
Focusing on HS dropouts, Table 3 reports the employment effects of the RMI (columns (2)) and of the RSA (columns (3)) using predictions from models A, B and C. These results can be compared to the actual effects of the reform, i.e. to RD estimates of columns (4) and (5) for Census years 2004-2008 (RMI) and 2010-2011 (RSA) respectively. First, our model predictions of the absolute effects around 25 are relatively close to these estimates, despite time changes in labor market conditions between 1999 and 2004-2011. We observe a very similar disincentive effect of the RMI before the 2009 reform for women while we under-predict the effect for men. We confirm that under the RSA scheme, there is no significant employment effect at age 25. Second, and most importantly, the difference (3)-(2) between the two welfare regimes points to a correction of the inactivity trap of $3 - 3.5$ ppt (over all specifications) associated with the RSA in-work component, which is very close to RD estimates (5)-(4) of around $2.8 - 3.4$ ppt. Model predictions indicate

[16] It is slightly smaller than in 1999, in a range of -3.6 to -2.6 over all age specifications of the model instead of -5.8 and -3.9. Another difference is that the effect is now smaller for men and slightly larger for women, yet not significantly so. See Bargain and Vicard (2014) for interpretations about what can explain time variation in the RMI effect.

[17] Unreported additional results show that due to an increase in wage rates with age, the disincentive effect of the RMI decreases with age and so does the re-incentivizing effect of the RSA. The change is insignificant at age 30.

slightly larger effects for women $(3.6 - 4.1)$ than for men $(2.6 - 3)$, which is confirmed by RD estimates. Results are particularly close when using the quadratic and quartic specification. Such similarity in the results is very reassuring regarding the external validity of the model and gives confidence in our ability to use the model to simulate hypothetical reforms, as suggested hereafter.

Figure 3: Counterfactual Employment Simulations: 2009 In-Work Benefit Reform (RSA)

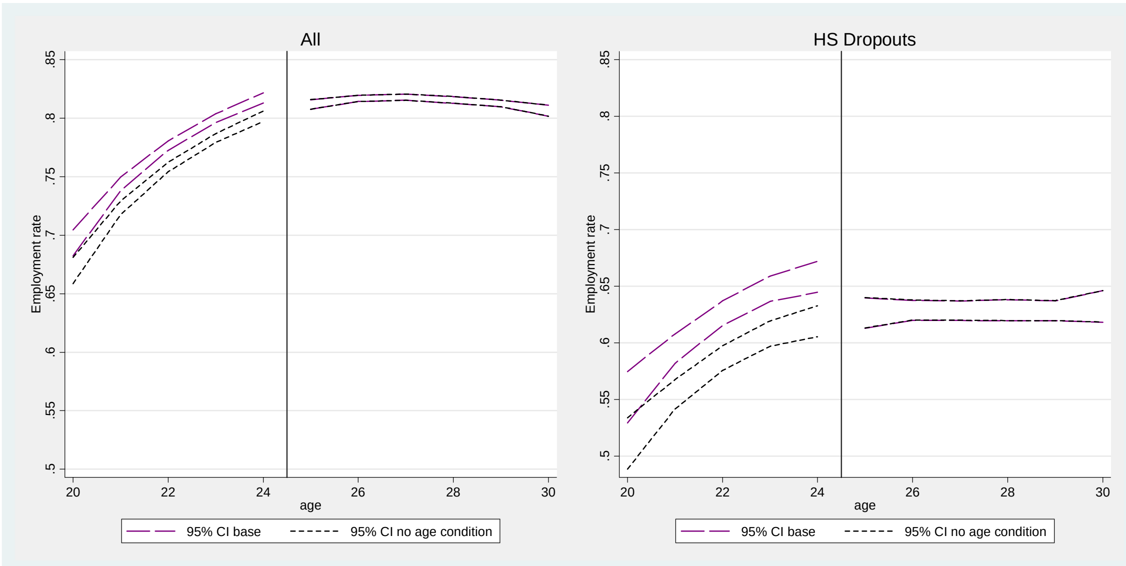


Note: Predicted employment rates from 1999 French Census data using structural model B, for RMI baseline and introduction of RSA (sample of 20-30 year old men and women who are available for work).

5.3 Counterfactual Policy Simulations

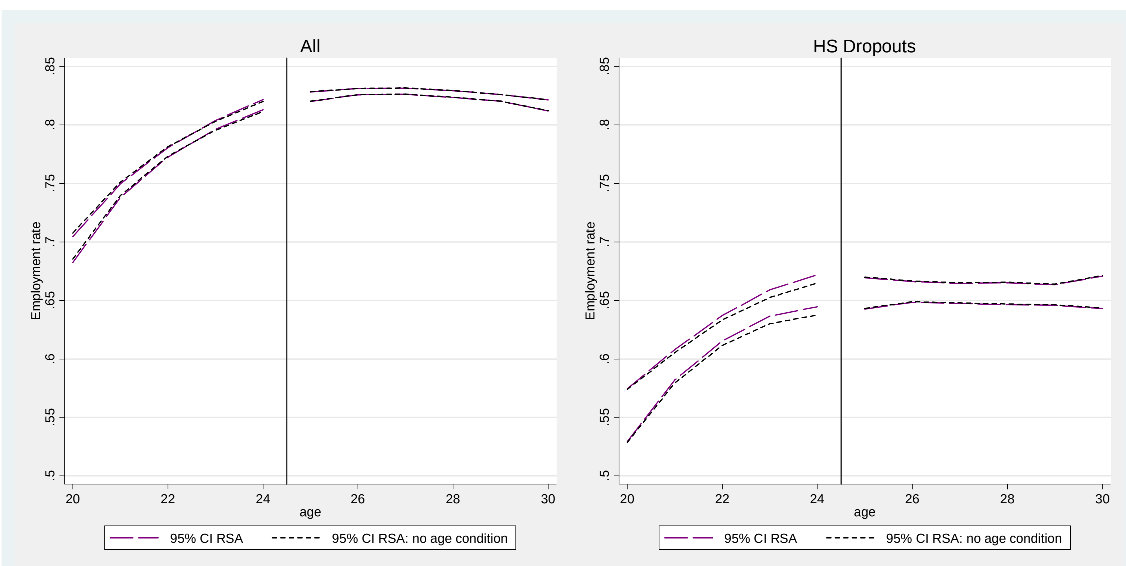
The behavioral model is finally used to predict important counterfactual policy scenarios. We rely on model B with a cubic specification of age for $a^1(\cdot)$ (results with other specifications, available from the authors, are very similar). We focus on an extension of welfare programs to the under-25s. Currently, their limited access to social benefits results in very large poverty rates, as discussed in the introduction. However, extending the RMI or RSA to those under 25 runs the risk of increasing welfare dependency by fostering it at a younger age and of further increasing inactivity among young workers. In fact, the in-work component of the RSA was granted to the under-25s in January 2016 while an extension of the out-of-work welfare payment was suggested by the French government in April 2016. The latter is perceived as a very controversial proposition due to fears of aggravating the rate of non-employment among the under-25s.

Figure 4: Counterfactual Employment Simulations: Extending RMI to the Young



Note: Predicted employment rates from 1999 French Census data using structural model B, for baseline and removing the RMI age condition (sample of 20-30 year old men and women available for work).

Figure 5: Counterfactual Employment Simulations: Extending RSA to the Young

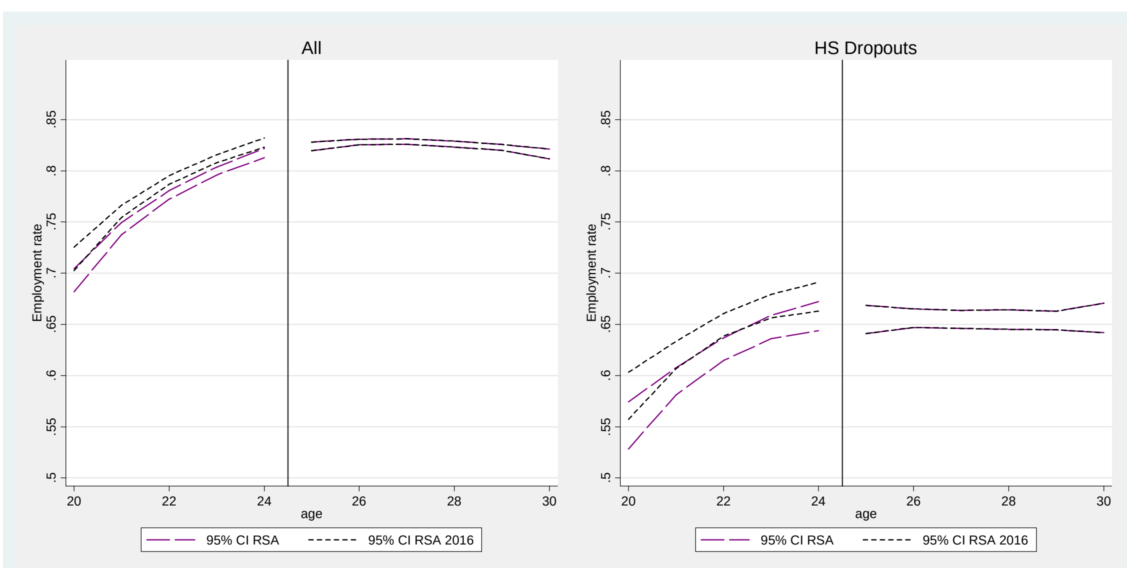


Note: Predicted employment rates from 1999 French Census using structural model B, for RSA scenario and removing the RSA age condition

In this context, we suggest a series of simulations that show the merit of different policy schemes. First, Figure 4 represents the result of a simulation taking the 1999 RMI scenario as baseline and abolishing the age condition. While this hypothetical reform has little effect on the whole sample, the HS dropouts show a negative employment response at every age point below 25 that is similar to the one observed at the cutoff. Introducing the RMI – equivalent to the out-of-work component of the RSA ("RSA socle") – for those under 25 induces a drop in participation of 5 ppt in this group. That is, young workers with low wage prospects may be tempted to claim the RMI and live on welfare, which casts doubts on the desirability of extending unconditional welfare payments to this group.

Next, we start with a baseline simulation of the RSA policy scenario, i.e. the real world situation after 2009, and simulate a removal of the age condition. Extending the RSA to the young combines two opposite forces – the disincentive effects of the out-of-work welfare payment versus the incentivization due to the in-work component. The results in Figure 5 show that extending the RSA to the under-25s would not have a significant employment effect for the whole selected group. We observe a small decrease in employment rates for the more vulnerable HS dropouts, yet it is not significant. Hence, our simulation gives support to the extension of welfare programs in France provided that in-work components are in place to "make work pay".

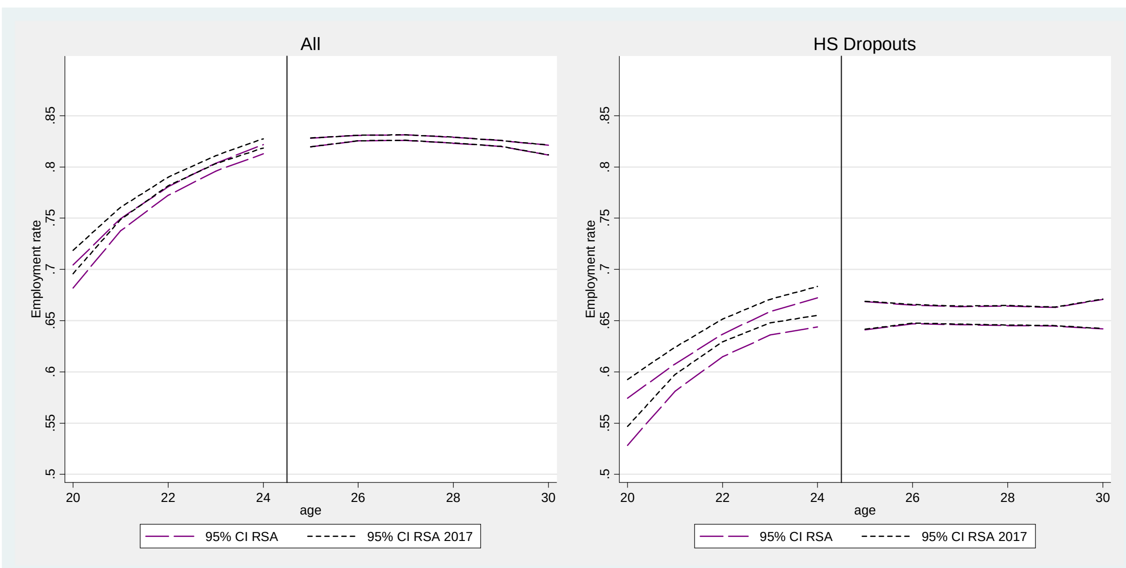
Figure 6: Counterfactual Employment Simulations: Extending In-Work Component of RSA to the Young



Note: Predicted employment rates from 1999 French Census using structural model B, for RSA scenario and removing the RSA age condition for the in-work benefit component

In Figure 6, our baseline scenario correponds to the real situation between 2009 and 2016 (RSA for the over-25, nothing under 25). The simulated reform is based on the actual policy change of January 2016, whereby the age condition for the in-work benefit part of the RSA ("RSA activité") is lifted, i.e. those under 25 who work but earn below the RSA threshold are now eligible for the RSA with a taper rate of 38%. However, those under 25 years of age with no labor income are still not eligible for the RSA. This "RSA 2016" reform increases the employment probability of those under 25 years of age. By increasing the gains to work for the under-25 population, this reform pushes the employment levels of the under-25s above that of the older workers.

Figure 7: Counterfactual Employment Simulations: Partial Extension to the Young (Full In-Work and Partial Out-of-Work Components)



Note: Predicted employment rates from 1999 French Census using structural model B, for RSA scenario and granting the in-work component plus a small out-of work payment component to the under-25's

Finally, we simulate a situation where the age condition on the in-work benefit component is removed (as per January 2016) but where an out-of-work payment is also granted to the under-25s. This welfare payment for the under-25s without resources is however lower than the national one, i.e. it is set to half of that for those over 25. This ensures a degree of safety from poverty for the young while still preserving some of their gains to work and putting less pressure on public finance. It also corresponds to the hypothetical reform currently under political discussion for implementation in 2017.[18] As seen in Figure 7, the

[18]In Denmark, the 25 year-old age cutoff also corresponds to different levels of benefits at different

"RSA 2017" also increases the probability of employment of those under 25 (compared to the RSA 2009) but by less than "RSA 2016" due to the fact that a mini-RSA is available for those under 25 who do not work. Turning 25 in this situation does not provide the same financial incentive to drop out of the labor force as the RMI and (potentially) the "RSA 2016" did.

6 Conclusions

Using 1999 Census data for France, we have modelled labor supply using the age condition on social assistance for identification. The model translates RD estimates directly in terms of financial gains to work, allowing us to predict any reform. The model not only reproduces the (RD estimated) participation drop at the age cutoff very well but also shows good external validity. Precisely, the predicted effects of a reform extending social assistance to the working poor are very similar to the actual effects (assessed by RD using data around the reform year 2009). This is important because many studies in the literature fit the data with a structural model and then claim that this can be used for other policy simulations. We not only make this claim but have shown that the model does successfully reproduce the effects of the reform. This informal check, despite being only suggestive evidence, is an important finding and encourages more work where quasi-experimental variation is used to improve the identification of structural models. We also use the model to simulate key reforms and show that new welfare programs, combining out-of-work and in-work components, could be granted to under-25 year olds without creating new disincentive effects in this vulnerable population.

Possible improvements would first require the extension of the approach beyond the participation margin. This could be done using the general discrete choice approach presented in the paper if additional exogenous variation were available, e.g., other discontinuities affecting the financial gains to work part-time versus full-time. Moreover, we had to rely on parametric assumptions to extrapolate further away from the age cutoff for simulations. It would also be good to check external validity along these lines (for instance through a change in the age condition). More generally, very recent studies have attempted to identify causal effects away from the RD discontinuity by conditioning on covariates besides the running variable (Angrist and Rokkanen, 2015) or through derivatives and local neighborhood assumptions (Dong and Lewbel, 2015). In future work, structural assumptions may actually help to delineate such assumptions required for extrapolation to other subpopulations than those used for causal inference around the discontinuity.

ages.

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A Online 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:

$$\log w_i = \theta(A_i) + \zeta.EDUC_i + \kappa.Z_i + \rho\lambda_i + \nu_i \tag{5}$$

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.[19] 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.

[19] 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.

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 below) 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.

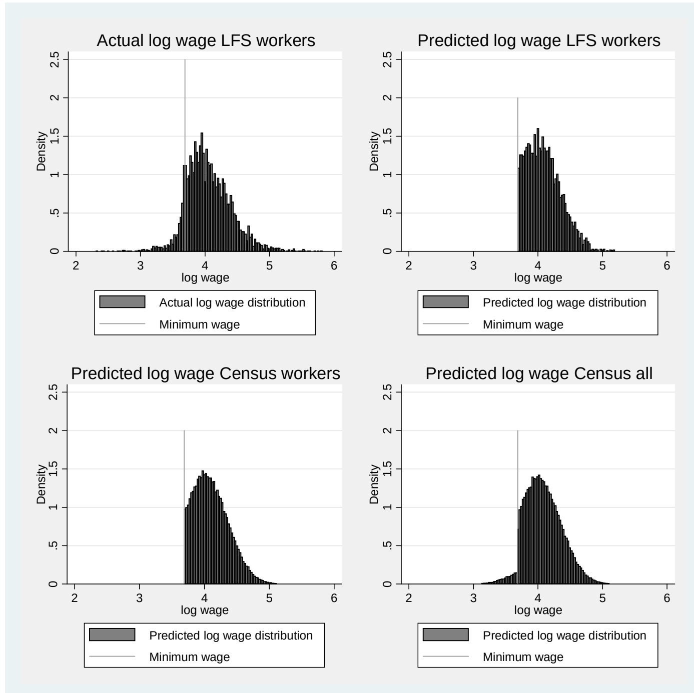

Figure A.1: Comparing Actual and Predicted Log Wage Distributions in FLFS and Census Data (Men)

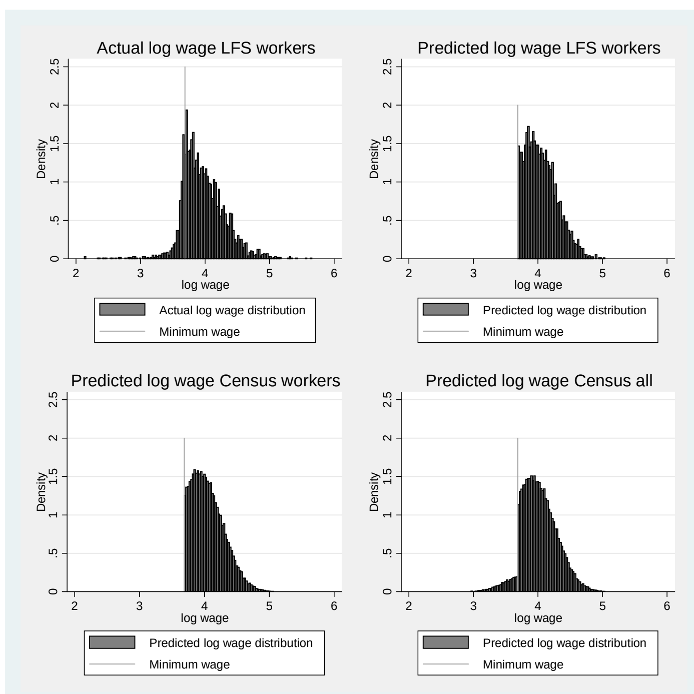

Figure A.2: Comparing Actual and Predicted Log Wage Distributions in FLFS and Census Data (Women)

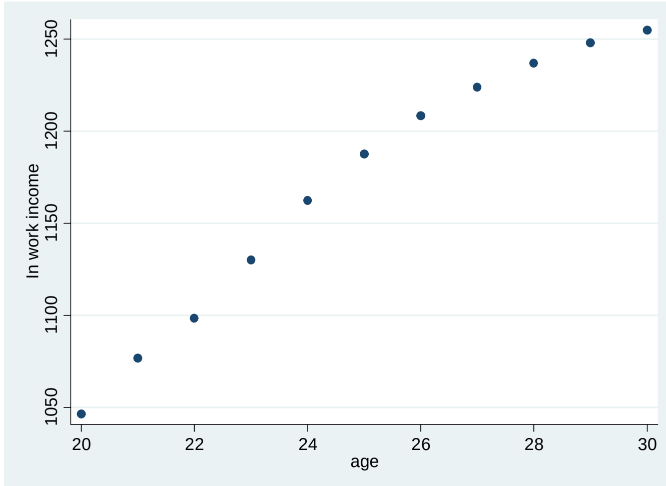

Figure A.3: Mean Predicted In-work Income by Age (Census data)

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^0 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 b_0) / 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^o_1) / 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.

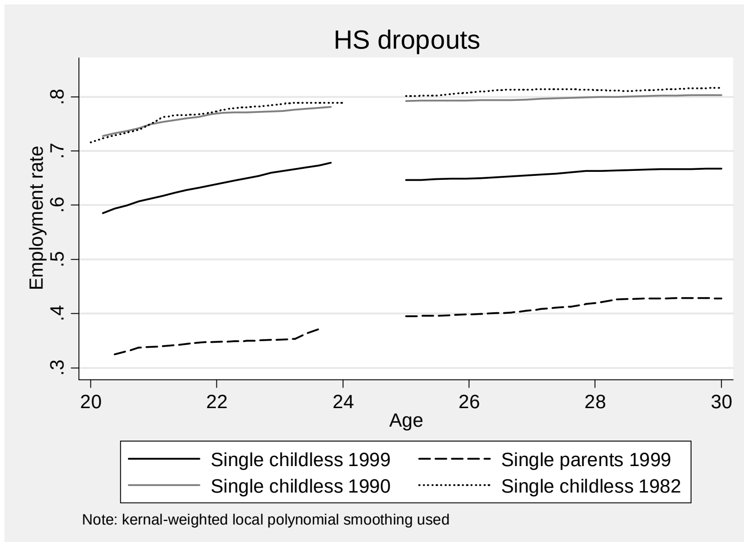

Figure A.4: Employment Rates of Childless Singles (Census, Age in Days: Nonparametric Fit)

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.

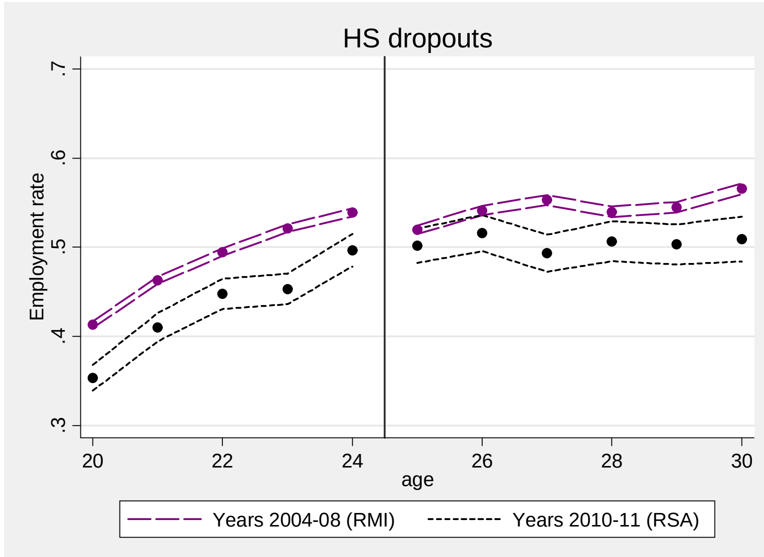

Figure A.5: Employment Rates of Childless Singles (Census, Age in Years)

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:

$$\beta = \overline{Y}_{25} - \overline{Y}_{24} - [\alpha^1(25) - \alpha^1(24)] \tag{6}$$

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

$$\overline{Y}_{25} - \overline{Y}_{24} - [a^1(25) - a^1(24)]. \tag{7}$$

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

$$\{b_1 C(wH; 25) - b_0 C(0; 25)\} - \{b_1 C(wH; 24) - b_0 C(0; 24)\}\,, \tag{8}$$

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 (7) 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:

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

Using this expression and the equality between (7) and (8), the policy effect becomes:

$$\begin{aligned}&\overline{Y}_{25} - \overline{Y}_{24} - [a^1(25) - a^1(24)]\\ &+\{b_0(25)C(0;24) - b_0(24)C(0;24)\}\\ &-\{b_1(25)C(\widetilde{w}_i(25)H;24) - b_1(24)C(\widetilde{w}_iH(24);24)\}.\end{aligned} \tag{9}$$

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:

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

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.

A.4 Additional References in the Appendix

[1] Arulampalam, W., A. Booth and M. 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.

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

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