Putting Structure on the RD Design:
Social Transfers and Youth Inactivity in France

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Abstract

Natural experiments provide explicit and robust identifying assumptions for the estimation of treatment effects. Yet their use for policy design is often limited by the difficulty in extrapolating on the basis of reduced-form estimates of policy effects. On the contrary, structural models allow us to conduct ex ante policy analysis but their internal validity is often questioned. In this paper, we suggest combining the two approaches by putting structure on a regression discontinuity (RD) design. We start with a RD estimation, exploiting the fact that childless single individuals under 25 years of age are not eligible for social assistance in France. A behavioral model is then identified using the same age discontinuity. While this model replicates well the employment effect obtained by RD, it can also be used to predict actual policy reforms and, hence, to check external validity. Showing good performances in this regard, it is finally used to simulate important counterfactual policies, namely the extension of social assistance to young people and the employment effects of a large in-work benefit reform.

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

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". For causal inference of actual policy effects, it is hard to dispute that the experimental and quasi-experimental approaches are preferable. 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. In fact, most studies using structural models are identified on the basis of strong or unclear assumptions. As a result, their internal validity is often questioned. By contrast, ex post evaluation methods provide credible identifying assumptions. 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 a RD design. We argue that the RD estimate is not a sufficient statistic to perform out-of-sample predictions. Counterfactual simulations and extrapolations further away from the discontinuity require the addition of structure to the model. A labor supply model simply makes the underlying interpretation of the RD design explicit, i.e. optimizing agents in a static framework make participation decisions based on financial incentives to work. Exogenous variation of these financial gains at the age discontinuity identifies the model. Variation in expected wage rates at different ages creates further changes in gains to work which explains how different age groups may react differently to policy reforms. The only parametric restriction required for making predictions at ages further away from the threshold is that age affects behavioral parameters continuously.

This framework illustrates the valuable combination of ex post and ex ante methods. 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 political debate: Does an extension of welfare programs to under-25 year-olds generate greater unemployment and, possibly, long-term poverty among the youngest workers? 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)? The first question is of particular importance in the present context of very high youth unemployment. The $16 - 24$ year olds have been hit particularly hard by the crisis and face the highest rate of unemployment in France. The youth also have limited access to welfare programs, which results in a poverty rate twice as large as that of the 25-30 years-old.[1] Studying age conditions for social benefits is not only relevant for France, as such discontinuities exist in several EU countries (e.g. Spain, Luxembourg, Denmark) and in Canada (see Lemieux and Milligan, 2008). The second question 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). We simulate several counterfactual policies to answer these questions, notably the extension of social assistance to the under-25 year-olds and the introduction of the 2009 RSA reform. We find that the 2009 system restores work incentives among the over-25 year olds, which is confirmed by an ex post analysis of what actually happened after 2009. In this way, we provide an original check of the model external validity. We also find that extending the new welfare program to those under 25 years of age should not reduce participation significantly. Hence, it seems possible to reduce poverty in this 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 data while section 4 presents the empirical strategy. Section 5 reports and analyzes the results while section 6 concludes.

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[1]With a youth unemployment rate of 24.7% in 2012, France is above the EU-15 and EU-28 averages (22.3% and 23%). Unemployment of the under-25 year olds has increased steadily in recent years in France, from 22.9% in 2011 to 25.5% in 2013. Youth unemployment and youth poverty are also suspected to have additional external effects like increasing crime (Fougère et al., 2009).

2 Literature and Contribution

2.1 Structural Labor Supply Models

A very large number of policy studies have relied on structural models estimated on cross-sectional or panel data to analyze existing fiscal and social policies, to compare them to optimal designs or to help policy making of future redistributive systems (see for instance the discussion in Blundell and MaCurdy, 1999). As argued in the introduction, the internal validity of these models is not guaranteed. Maybe the main identification issue concerns the fact that omitted variables (e.g., being a "hard working" person) could positively affect gross wage rates and consumption-leisure preferences simultaneously. If variation in gross wages in the population is endogenous to preferences, it cannot be directly used to infer potential responses to financial incentives (for instance a tax reform). In traditional labor supply models, identification is provided by exclusion restrictions and hinges on the validity of instruments (e.g., Hausman, 1981, for the US or Bourguignon and Magnac, 2001, for France).

More recently, the use of discrete choice models has allowed the incorporation of the complete effect of tax-benefit policies on household budget constraints. In this way, identification can be obtained from exogenous variation in tax-benefit rules across regions (e.g., across US states in Hoynes, 1996) or over time (e.g., Blundell et al., 1998). Time or spatial variation in tax-benefit rules brings the identification of structural models closer to the quasi-experimental approach. Most often, however, only cross-sectional variation is available. In this case, discrete choice models are identified by all the nonlinearities 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). For instance, two identical persons (same gross wage, age, gender, etc.) will face different *effective* tax schedules if one has two children and the other has three, simply because child benefits, child tax allowances or other child-related policies vary with gross income. This type of identification is parametric since demographics themselves affect labor supply. More fundamentally, it must also rely on some implicit exclusion restrictions (in our example above, we may assume that 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 this study, the age discontinuity plays a similar role. However, while labor supply models estimated on the full population muddle multiple sources of identification, that are usually not made explicit, we focus on a very homogeneous group, i.e. childless singles aged around 25. In this way, we reduce demographic variation to only one dimension (age), which provides us with a clean setting for identification. First, the exclusion restriction

is more reasonable in this case. Contrary to the example with children above, there is no obvious reason for preferences to vary discontinuously with age. Second, age is a dimension over which individuals have no control, in contrast to fertility or marital status. Finally, focusing on only one source of heterogeneity makes the underlying identifying assumption explicit.[2] As shall be seen, the RD design only requires that people just under 25 are identical to people just above 25, other things being equal. The structural model requires a little more.

2.2 (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. Using a RD design, 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 older. They find that more generous transfers reduce employment.

We exploit a similar discontinuity here, drawing on the RD design detailed 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).[3] 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. Youth unemployment is a recurrent problem in many OECD countries and in France in particular. It is therefore crucial to evaluate the potential increase in inactivity

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[2]For all these reasons, we refrain from estimating a standard labor supply model on a broader population. This would defeat the purpose of our "clean" exercise. In particular, we would not know what role is played by the age discontinuity among the multiple sources of identification provided by policy nonlinearities and discontinuities.

[3]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).

that may follow an extension of social transfers to the under 25's, as motivated in the introduction.

2.3 Comparing and Combining Approaches

Comparing methods is a first important step. Lalonde's (1986) landmark paper studied the ability of a number of econometric methods, including Heckman's selection model, to replicate the results from an experimental evaluation of a labor market program, on the basis of non-experimental data. In the same vein, comparisons of randomized or natural experiments with the predictions of structural models would be useful. Yet there is no systematic attempt to do so in the labor supply literature. A few studies have recently compared the employment effect of tax-benefit policies predicted using structural models with the actual effect as measured by ex post evaluation techniques, including difference-in-difference (e.g., Blundell, 2006, Cai et al., 2007, Thoresen et al., 2012), regression discontinuity (Hansen and Liu, 2011) or randomised experiments (Todd and Wolpin, 2006). While most of these studies point to the satisfying performance of structural models, others do not (especially Choi, 2011 and Keane and Wolpin, 2007). Most of these studies tend to put structural model predictions beside an ex post evaluation of the same policy effect, and conclude from the comparison on the quality or flaws of the structural approach. This is an important and useful exercise. Yet such comparaisons run the risk of treating one or other of the approaches in a biased way.

More fundamentally, ex post and ex ante evaluation approaches are complementary, as discussed in the introduction. Treatment effect estimates inferred from natural experiments are often reliable as they derive from clear and robust identification strategies. However, while they can inform about the labor supply effect of the policy regime under study, they are of limited use for predicting future or alternative policy scenarios. Indeed, their reduced-form nature makes that these estimates are often endogenous to the policy environment and cannot be used to simulate policy reforms. Even when they escape from this type of Lucas critique, these estimates are usually far from being a sufficient statistic that can predict all types of counterfactual reforms, as explained in section 4. Thus we suggest "adding some structure", i.e., designing a structural model that is identified using the same natural experiment (a policy discontinuity in our approach).[4] It is then used

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[4]A few studies have explored the benefits of randomization or quasi-experiments for identification, estimation and assessment of structural models. Imbens (2010) cites an early example, Hausman and Wise (1979), who estimate a model for attrition with data from a randomized income maintenance experiment. Recent examples include Card and Hyslop (2005), who estimate a structural model of 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; Imbens,

to simulate an actual policy reform that extends redistribution towards the working poor in France, the RSA. Comparing the predicted employment effet of this reform against the actual effect allows us checking the external validity of the model. 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 simulation. In what follows, we do not only make this claim but show that the model does successfully reproduce the effects of the RSA reform.

In the absence of purely experimental data, the question of which type of natural experiment is suitable for our purpose arises. We suggest using RD as one of the simplest and cleanest forms of natural experiments. Using RD designs is, unsurprisingly, popular in the labor supply literature as this strategy provides assignment to treatment that is 'as good as random' in the neighborhood of the discontinuity (Lee and Lemieux, 2010). Additionally, studying specific policy discontinuities, such as an age discontinuity in social assistance rules, provides a more clear-cut assessment than natural experiments based on policy changes over time, which must control for simultaneous changes in the economic environment.[5] These considerations are guiding our approach. Yet we must acknowledge that, even though RD designs may have the highest degree of internal validity among quasi-experiments, they also show strong limitations regarding the possibility to extrapolate to other subpopulations than those used for causal inference.[6] The behavioral model allows extrapolations, notably those further away from the cutoff, but at the price of an additional identifying assumption (i.e. the global continuity of behavioral parameters with the forcing variable, as explained in section 4.2).

Rubin and Sacerdote (2001) who estimate labor supply models, exploiting random variation in unearned income using data from lottery winners and Duflo, Hanna, and Ryan (2007) who look at the effect of monitoring and financial incentives on teacher's absences. There is certainly more room for such work where (quasi) experimental variation is used to improve the identification of structural models.

[5]Lemieux and Milligan (2008) actually find that commonly used difference-in-differences estimators may perform poorly with inappropriately chosen control groups, notably, groups not placed in the same labor market as the treated. RD analyses provide an advantageous alternative when available, although they must verify if other policies could generate similar discontinuities (which we check in section 3.1). Here, a related difficulty with double differences is the question of how the control group should be incorporated in the structural model estimation, since this group would also require exogenous variation for identification of its behavioral parameters.

[6]One recent attempt to do so identifies causal effects away from the RD discontinuity by conditioning on covariates besides the running variable, in an effort to eliminate the relationship between the running and outcome variable (Angrist and Rokkanen, 2013) The authors, however, admit that it is not always possible to find such controls.

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. We describe here the situation relevant 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 an in-work transfers 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 education. 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. In practice, entitlement to the RMI does not include any obligation to actively seek work or to train, and it 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 cut-off and for someone out of work, we have $C(0; 24) = S(0)$ and $C(0; 25) = S(0) + R$. With 1999 figures, $C(0; 25)$ is around EUR 540 per month and 162% more than $C(0; 24)$. 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.

Graphical Illustration. Figure 1 aims to clarify the impact of these redistributive schemes on living standards and to compare them together and with an international reference point. Precisely, 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 eligible to 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). Figure 1 also reports budget constraints under the three redistributive regimes (these counterfactual simulations are obtained using the tax-benefit

Figure 1: Schedules of Alternative Redistributive Schemes and Budget Constraints
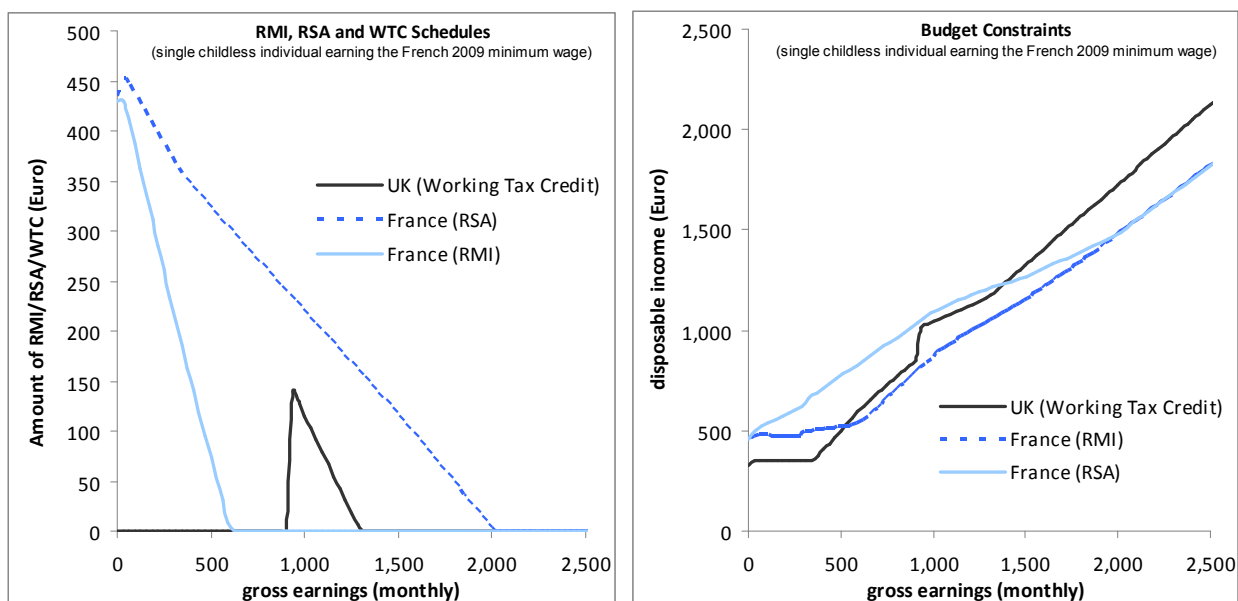
microsimulation EUROMOD, which reproduces the tax-benefit rules for several European countries including France and the UK).

The first graph of Figure 1 shows that the RSA schedule is particularly generous for a minimum wage 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 in our example. The second graph of Figure 1 shows that 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 1000-1500 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 as seen in the first graph and because of generous tax free allowances in the UK, which allow very low income people pay no tax).

Confounding Institutional Factors at Age 25. A last important aspect of the institutional background is the possible confounding factors regarding the age discontinuity.

Along all institutional features that could also be responsible for a discontinuity in employment patterns at age 25, we first investigated other tax-benefit policies. The only relevant benefit policy in terms of age condition appeared to be the RMI itself, i.e., parents receiving the RMI obtain an increment for children aged 21-24. Yet, 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 do 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 detail).

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 still 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). As explained below, external validity is checked using more recent Census data for years 2004-11.[7]

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

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[7]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 have used to conduct RD analyses for external validity checks (see section 5.2).

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 and extra time payment and in-kind advantages), education and demographics. Hence, it is possible to calculate hourly wages and estimate wage equations on key variables like age and detailed education categories, as explained below (see also Chemin and Wasmer, 2012).

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 education, in the army or living on a (disability) pension. Our analysis focuses on *singles without children who live alone.* First, childless single individuals 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.

Descriptive Statistics. 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 comparable definitions of the key variables used to estimate wage rates and, in particular, education categories.[8] Table A.2 in Appendix A.2 provides descriptive statistics of both datasets (for FLFS, we consider the year 1999 or, alternatively, a pool of years 1997-2001). We show there that the two selected samples are comparable in terms of demographic and education structures, which gives us confidence in the wage imputation. Table A.2 also shows that average simulated disposable incomes line up quite closely in the two datasets.

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[8]Both datasets provide detailed information on qualifications: junior school diploma (*Diplôme National du Brevet*, BEPC, or lower secondary level diploma), junior vocational qualification certificates (*Certificat d'Aptitude Professionnelle*, CAP, and *Brevet d'Etudes Professionnelles*, BEP), high school diploma (*Baccalauréat*, or upper secondary level diploma), first college degree or advanced vocational degree, higher degrees from universities or business/engineer "Grandes Ecoles".

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 Vicard, 2014). The FLFS shows larger employment rates (as reflected in the average employment figures in Table A.2), 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.

4   Empirical Approach

Before turning to the structural model, we 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.

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. Age is available in days so that 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:

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

It is more appropriate to express age in *years*, *quarters* or *months*. With age expressed in days, age cells would be small and would display a very erratic age-employment pattern. A discrete forcing variable means that we cannot compare observations "close enough" on

both sides of the cutoff point to be able to identify the effect. As explained in Lemieux and Milligan (2008), parametric assumptions are required in this case. Hence, we specify the RD model as:

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

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 $\delta(A_i)$ and by $T_i = I(A_i \geq \underline{A})$. The parametric version of Condition 1 requires that $\delta(A_i)$ 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 $\delta(A)$ in order to balance the usual trade-off between precision and bias (see Lee and Lemieux, 2010). Note that coefficients α_i and β_i bear a subscript i as they vary linearly with a set Z_i of individual characteristics. In particular, we shall introduce heterogeneity in the treatment effect with $\beta_i = \beta^0 + \beta^1 Z_i$. As explained, there is little demographic variation left except gender. The other variation concerns education: we are especially interested in estimating specific participation effects of the RMI for HS dropouts, so that all coefficients will vary with a dummy equal to 1 for individuals in this group.

At this stage, it becomes clear that the RD design allows only limited extrapolation. Given the employment effect of the switch in maximum benefit level R due to the age condition (see amounts in section 3.1), it is possible to calculate the employment elasticity with respect to R. Denoting $\overline{Y}$ the mean employment rate, this elasticity is written $\frac{d\overline{Y}/\overline{Y}}{dR/R}$ and estimated around $-.05$ (similar elasticities are found in Bargain and Doorley, 2011, and Lemieux and Milligan, 2008). It can be used to predict the employment effect of uprating policies, for instance when social assistance is uprated more rapidly than price inflation. Yet it is difficult to say much more. For instance, we cannot extrapolate further away from the discontinuity to answer our initial question regarding the employment effect of extending social assistance to those under 25. Also, we cannot predict the effect of a change in another social assistance parameter, the withdrawal rate t. Hence, we cannot predict the effect of a reform that increases work incentives by reducing this implicit taxation rate. These two examples, among many, are motivated by the policy questions asked in the introduction. They also point to the fact that the RD estimate is not a sufficient statistic for predicting all types of policy reform. At a minimal cost, putting structure on the RD design shall allow us to do so.

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 (for instance, van Soest, 1995). In their discrete version, these models are based on the assumption of agents choosing the weekly worked hours option $j = 1, ..., J$ in a discrete 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:

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

with disposable income $C(w_iH_j; A_i)$ (equivalent to consumption in this static framework) and worked hours H_j. Disposable income is reduced by a level F_i for positive hours choices. This term may capture fixed costs of working as well as the cost of job search on the labor market, so that it must vary with individual characteristics including age. 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 observational errors, optimization errors or transitory situations.

Because function $C(\cdot; A_i)$ accounts for the full tax-benefit rules, this structural model is widely used for policy analysis (see Blundell and MaCurdy, 1999, for a survey). As previously discussed, identification often relies on the nonlinearities/discontinuities or time/spatial variation in the tax-benefit rules. In our setting, we use the age condition in social assistance eligibility, creating exogenous variation in financial incentives at 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. As discussed in the concluding section, the more general model presented in equation (2) could be identified using our approach but would require more variation (for instance other discontinuities affecting financial gains between full and part time work).

Specifications and Exclusion Restriction. We complete the specification in the general case. Translog or quadratic utility functions in hours H_j and consumption C are typically used for function U_i (see Blundell and MaCurdy, 1999). Bargain (2006) and van Soest et al. (2002) show, however, 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. Instead, we opt for a flexible specification where preference parameters vary with the choice j:

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

13

In this way, the "disutility" of work or other components like work costs are specified through choice-specific terms a_{ij} and, hence, are not forced to vary linearly or quadratically with H_j as in standard functional forms. The same is true for interaction between hours and consumption, with coefficients b_{ij} and c_{ij}. Bargain (2006) shows that this specification nests the standard quadratic utility function used in many applications and fits the data better (we check hereafter that it does not overfit it). In addition, coefficients in (3) vary linearly with several taste-shifters Z_i and, possibly, random terms for unobserved heterogeneity. Taste shifters include gender and education (a "HS dropout" dummy) as in model (2). Preference parameters can also vary with age and, in particular, the first term can vary with the same smooth function as in the RD model, i.e. $a_{ij} = a_{ij}^0 + a_{ij}^1 \delta(A_i)$.[9] We must impose the following continuity condition:

Condition 2 *(global continuity) Behavioral parameters* a_{ij}, b_{ij}, c_{ij} *vary continuously with age.*

Three remarks are in order. First, while the parametric version of Condition 1 required $\delta(A_i)$ to be a smooth function of age *around the discontinuity*, we specify behavioral parameters in (3) as *globally* continuous in age in order to use the model for extrapolation further away from the threshold. Second, this exclusion restriction is standard in the literature and there is no harm in assuming away the possibility of discontinuous preferences with respect to age (see our discussion at the end of section 2.1). Third, in the budget constraint, wages w_i are also a smooth function of age, so that the only source of age discontinuity in the model is the exogenous change in financial incentives.

Participation Model. With this setting, we now focus on the participation margin. The choice of working full-time ($j = 1$) 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 quadratic term in consumption in equation (3) is not necessary as we model participation only. The propensity to be employed is written as:

$$Y_i^* = a_i + b_{1i}C(w_iH_1; A_i) - b_{0i}C(0; A_i) + \epsilon_i \tag{4}$$

---

[9]This term entering utility in an (additive) separable way may capture work preferences, fixed costs of work and search costs, all possibly varying with age. The latter interpretation, seach costs, rationalizes demand-side constraints in such a pure supply-side setting (cf. van Soest et al., 2002). Coefficients a_{ij}^0 and a_{ij}^1 both have subscript i as they vary with Z_i, as do coefficients in model (2). In particular, they vary with a "HS dropout" dummy. Indeed, uneducated workers do not only have lower wage prospects but also a weaker attachment to the labor market and, hence, larger search costs (see Beffy et al., 2006, and Gurgand and Margolis, 2008).

with $\epsilon_i = \epsilon_{1i} - \epsilon_{0i}$. The model is now very similar to the RD model in equation (1). The first term

$$a_i = a_i^0 + a_i^1 \delta(A_i)$$

is specified with the same smooth function of age. The rest captures the discontinuity effect (age condition) not through a single coefficient β but with a bit more structure, i.e. through financial gains to work expressed as the distance between disposable income when employed, $C(w_i H_1; A_i)$, and disposable income when out of work, $C(0; A_i)$.[10]

Note that while b_{1i} may vary with age in a continuous way, b_{0i} cannot vary with age as it is identical for all individuals on the same side of the age threshold. We specify three models. In the first, b_{1i} does not vary with age (model A). In the second, it varies linearly (model B) while, in the third, it varies quadratically with age (model C). All coefficients $a_i^0, a_i^1, b_{0i}, b_{1i}$ also vary with Z_i (gender and a "HS dropout" dummy). Additionally we make b_{1i} 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).

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). Second, disposable income is calculated for each observation and at each discrete labor supply choice (see Appendix A.2). 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.3). 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 number of draws for these terms, recalculating disposable income each time.[11] Finally, the model can be used to simulate counterfactual policy

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[10]In practice, in (4), we do not impose these two income levels to have the same marginal effect (i.e. $b_{1i} \neq b_{0i}$). Indeed, individuals may value marginal out-of-work income differently from marginal in-work earnings (b_{0i} may capture, for instance, the stigma effect when living on welfare).

[11]A computationally convenient approach consists of using sequences of Halton draws, as suggested by Train (2003). This allows us to reduce the number of draws to a tractable level ($r = 10$).

scenarios, i.e. alternative functions $C(\cdot; A)$ and hence new levels of disposable income, used to predict the new optimal choice of each individual. Hypothetical, counterfactual scenarios include abolishing the RMI (which we denote by function $C^0(\cdot; A)$ in Appendix C), the replacement of the RMI by the 2009 RSA system and the removal of the age condition.

5   Results

In this section, we focus on the main results (detailed results concerning wage and labor supply estimates can be found in Appendix A, as noted above). 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 reports actual employment levels at all ages as well as predicted employment rates and their confidence intervals obtained with our structural model (as specified in equation (4) and using a cubic function of age for $\delta(\cdot)$). 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. Figure 2 also shows a very small drop in actual employment rates at age 25 for all education groups but a more significant drop, around 3.4 percentage points, for HS dropouts. Hence, for the group combining both low wage prospects and little labor market attachment, there is a noticeable disincentive effect of the RMI.[12] We now turn to a precise assessment of the RMI employment effect.

RMI Employment Effect.   As a first visual check, we see in Figure 2 that, 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. The more formal check consists of comparing the RMI employment effects measured by RD with those predicted

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[12]Similar responses are found to the age discontinuity in social assistance in Canada (Lemieux and Milligan, 2008) and Denmark (Jonassen, 2013).

by the structural model. We focus on the specification with a cubic form of $\delta(\cdot)$ for both the RD model (1) and the strutural model (4). The difference in actual employment rates at 24 and 25 years of age, $\overline{Y}_{25} - \overline{Y}_{24}$, is -0.7 percentage points (ppt) in the broader group compared to -3.4 ppt among HS dropouts (not reported). When additionally accounting for a cubic age trend to extrapolate towards the threshold, we obtain RD treatment effects β_i of -1.6 ppt and -3.9 ppt for the broader group and for HS dropouts respectively, as indicated in the first column of Table 1. Both effects are statistically significant. Hence, we confirm the substantial negative effect of the RMI on singles in the case of HS dropouts.[13]

Figure 2: Employment Rate of Childless Singles: Fit of the Structural Model

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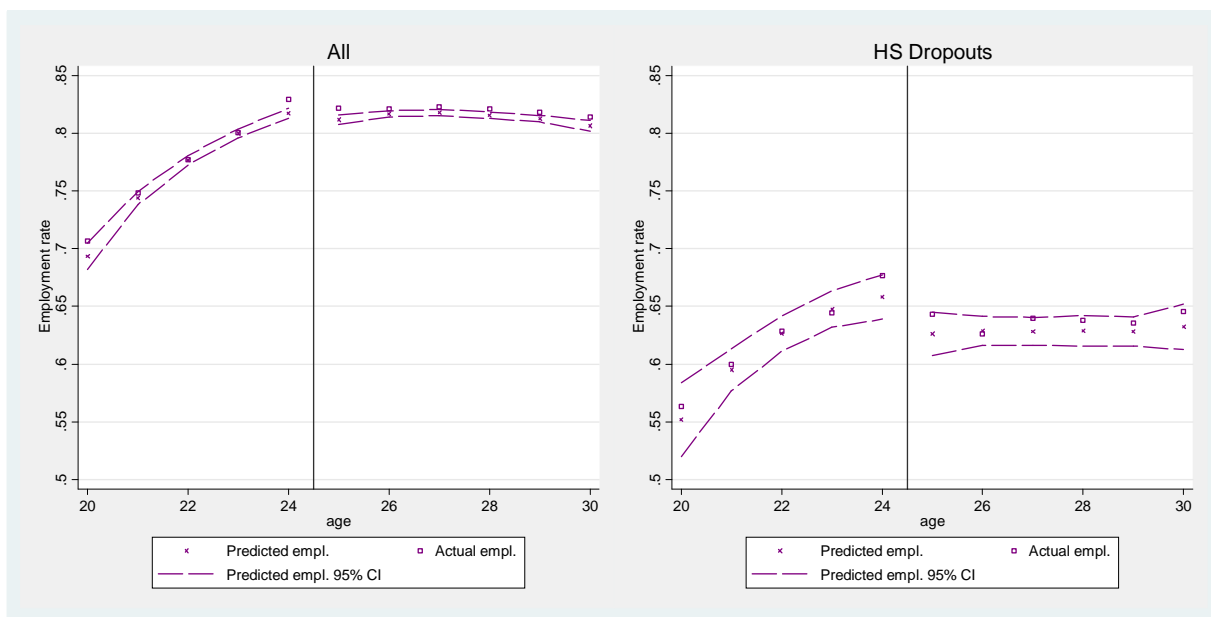
Note: Actual employment rate from 1999 French Census compared to predicted employment rate using structural model A (sample of 20-30 year old men and women who are available for work).

The next columns of Table 1 compare these estimates with the prediction of the structural model. The treatment effect in this case accounts for the drop in employment plus the trends on both sides of the cutoff in absence of policy effect, as defined in Appendix C. We suggest several specifications of the structural model. In model A, as described above, age

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[13]The RD graphical analysis as well as sensitivity checks of the RD estimates are provided in Appendices B.1 and B.2. Estimations of model (1) for different specifications of the RD model (age in years or quarters, $\delta(\cdot)$ as quadratic, cubic or quartic) indicate a magnitude of β_i in a range between -5.8 and -3.6 ppt for HS dropouts.

Table 1: Employment Effects of the RMI: RD vs. Structural Model

RMI Effect	Regression Discontinuity (RD)	Behavioral model					Out-of-sample predictions	
		A	A2	A3	B	C	RD	Model A
All education groups								
All	-1.6 ***	-1.5 ***	-1.6 ***	-1.6 ***	-1.5 ***	-1.5 ***	-1.1 *	-1.9 ***
	(0.4)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.6)	(0.7)
Male	-0.7	-1.7 ***	-1.8 ***	-1.8 ***	-1.6 ***	-1.5 ***	-1.7 **	-2.0 ***
	(0.6)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.7)	(0.8)
Female	-2.5 ***	-1.3 **	-1.4 **	-1.4 **	-1.5 **	-1.5 **	-0.4	-1.8 **
	(0.7)	(0.6)	(0.6)	(0.6)	(0.6)	(0.6)	(0.7)	(0.8)
HS Dropouts								
All	-3.9 ***	-3.9 **	-3.6 ***	-3.6 **	-3.9 **	-3.9 **	-3.5 **	-4.1 *
	(1.4)	(1.5)	(1.4)	(1.5)	(1.5)	(1.5)	(1.6)	(2.1)
Male	-4.2 **	-4.5 ***	-4.2 **	-4.2 ***	-4.5 ***	-4.5 ***	-4.1 **	-4.9 **
	(1.8)	(1.6)	(1.7)	(1.6)	(1.6)	(1.6)	(1.7)	(2.2)
Female	-3.4	-2.9	-2.5	-2.5	-3.0	-3.0	-2.5	-2.8
	(2.4)	(1.9)	(2.3)	(1.9)	(2.0)	(2.0)	(1.8)	(2.5)

*The employment effect of the RMI is estimated using the RD design or predicted using behavioral models (versions A-C). Both approaches rely here on a cubic age specification for the additive term. 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 figures are based on the 1999 Census data (for behavioral model, wages are imputed using estimations on the French Labor Force Survey). Out-of-sample predictions are performed on 50% of the sample using the other 50% for estimating the model. Estimates significant at the 1%,5% or 10% levels are indicated using ***, ** and * respectively. Standard errors are reported in brackets.*

is excluded from the marginal utility of consumption (age affects preferences continuously only through function $\delta(\cdot)$ in the additive term a_i, as in the RD design). Other variants of this specification, models A2 and A3, use information on age in quarters and months respectively, rather than age in years.[14] In models B and C, the individual's valuation of the monetary gains from work varies linearly and quadratically with age, respectively. 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. We observe slightly more homogenous results across gender groups for the whole sample compared to RD estimates. For HS dropouts, however, the model predicts the larger effects for men well.[15]

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[14]The forcing variable (age) can be treated more as a continuous variable in this case, so that extrapolations around the discontinuity are less dependent on the parametric form used (see sensitivity analyses of the RD estimates in Bargain and Doorley, 2011, and the discussion Lee and Card, 2008). It also generates more noise given smaller age cells. This is not a problem for the fit of the structural model and we notice very little variation when using models A2 and A3.

[15]Alternative specifications for $\delta(\cdot)$ (quadratic, quartic) do not affect our conclusions qualitatively, even if small quantitative differences are observed. For HS dropouts, this can be seen in Table 2 in the next sub-section. Compared to results with the cubic form, we observe larger effects for men, and slightly larger (smaller) effects with the quadratic (quartic) form for women. Importantly, comparing columns

Finally, we 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 are reported in the last two columns of Table 1. The treatment effect on the holdout sample, measured by RD, is 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.

Overall the internal validity of the behavioral model is very satisfying, which was expected since this model is identified using the same discontinuity as the RD model. That is, the reduced-form estimate of the discontinuity effect is simply transposed to a more structural setting, i.e. a change in the level of financial gains to work, so the model shoud replicate well the discontinuous effect.

## 5.2   External Validity: Predicting the Effect of the 2009 Reform

We now address the external validity of the behavioral model. Extrapolations using this model 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).

Our behavioral model is used 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. For HS dropouts, it has a larger positive effect on employment rates above 25 years old, of about 3 ppt. 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. There is no effect for those under-25 because the age condition is maintained under the new scheme.

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(1) and (2) confirms that RD estimates and model predictions are very similar in all specifications.

Figure 3: Counterfactual Employment Simulations: 2009 In-Work Benefit Reform (RSA)
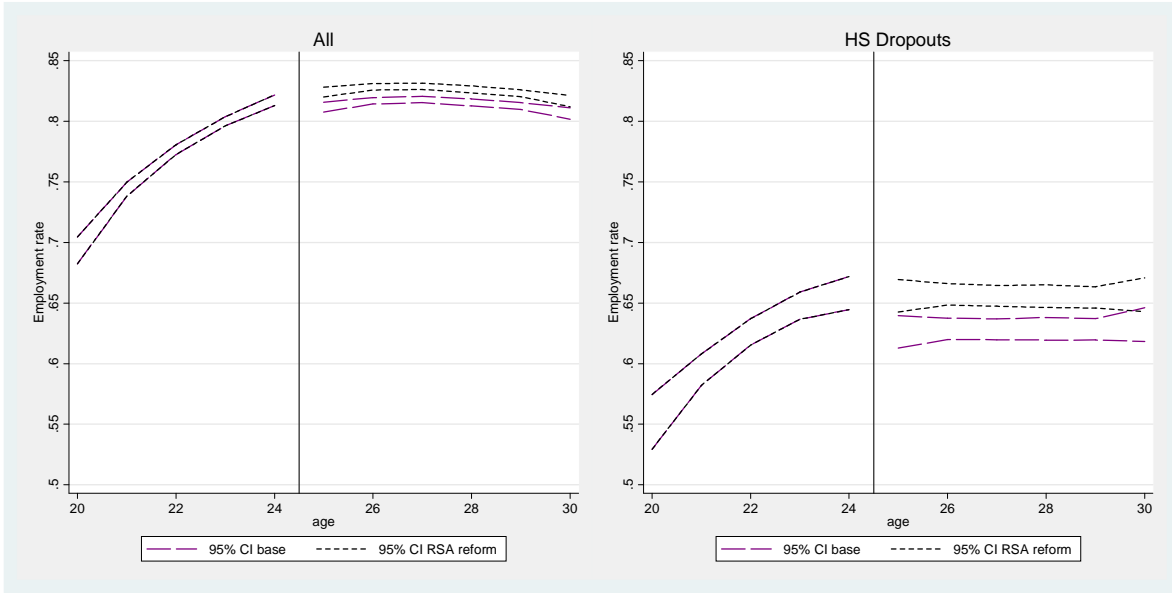
Note: Predicted employment rates from 1999 French Census data using structural model A, for baseline and introduction of RSA (sample of 20-30 year old men and women who are available for work).

Focusing on HS dropouts, we report the employment effects of the RMI and of the RSA in the left panel of Table 2, using predictions from models A, B and C. Compared to the RMI, the RSA employment effect at age 25 is much smaller and not significantly different from zero in most cases, confirming the re-incentivizing effect of the in-work component. The difference between the RMI and the RSA effects points to a correction of the inactivity trap of around 3 ppt thanks to the RSA reform, with slightly larger effects for women (between 3.6 and 4.1 over all specifications) than for men (between 2.6 and 3).

These results can be compared to the actual effects of the reform. In the last three columns of Table 2, we report RD estimates of the RMI effect before the reform took place (i.e. using Census data for years 2004-2008) and just after (years 2010-2011). Results turn out to be very similar to our model prediction. First, despite time changes in labor market conditions between 1999 and 2004-2008, we observe a similar disincentive effect of the RMI before the 2009 reform. It is slightly smaller than in 1999, i.e. between -3.6 and -2.6 over all age specifications of the model in column (4). Second, the two years under the RSA system show no disincentive effect at the cutoff (column (5)). Finally, the differential effect between the two welfare regimes, (5)-(4), is positive and very close to our model simulations, i.e. between 2.8 and 3.4 over all age specifications. RD estimates also confirm a slightly larger re-incentivization of the RSA for women compared to men, as predicted

Table 2: 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)
Age specification: quadratic													
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)
Age specification: Cubic													
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**
	(1.4)	(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**
	(1.8)	(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**
	(2.4)	(1.9)	(2.0)	(2.8)	(2.0)	(2.0)	(2.8)	(2.0)	(2.0)	(2.8)	(1.9)	(6.1)	(5.2)
Age specification: Quartic													
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 and continuous 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.

by the behavioral model. The effect is, unfortunately, not statistically significant in most specifications because of the smaller sample used for the RSA regime, which results in a lack of power. Nevertheless, such similarity in the results, and in differential results between men/women or across specifications, are very reassuring regarding the external validity of the model.

5.3   Counterfactual Policy Simulations

Finally, the behavioral model is used to predict important counterfactual policy scenarios. Results provided in this section use the cubic specification of age (results with other specifications of $\delta(\cdot)$, available from the authors, are very similar).

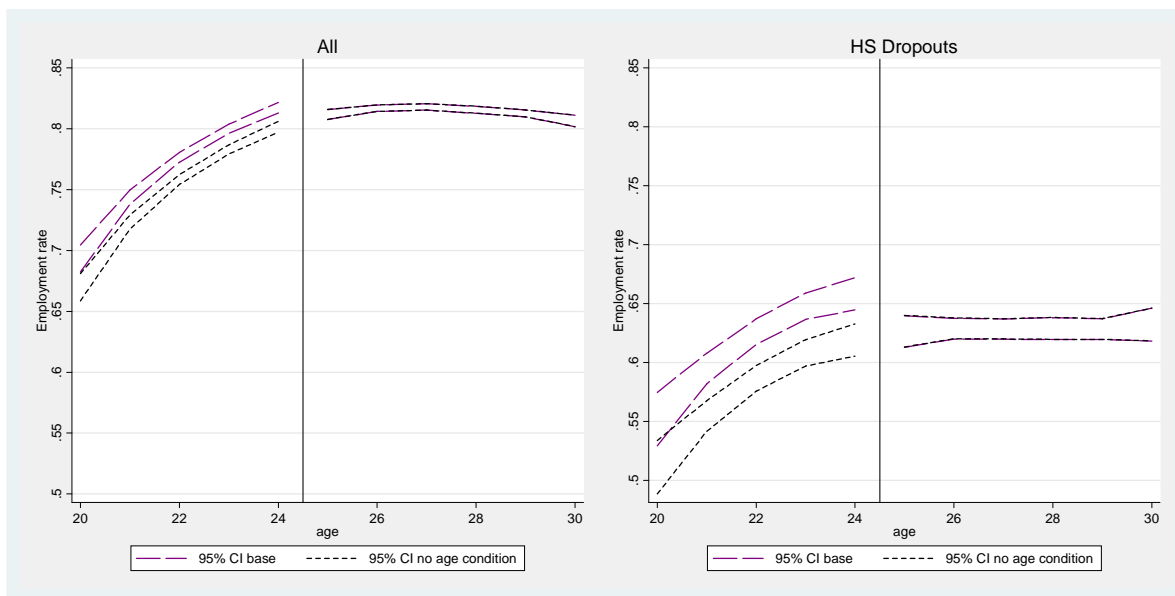Figure 4: Counterfactual Employment Simulations: Abolishing the RMI
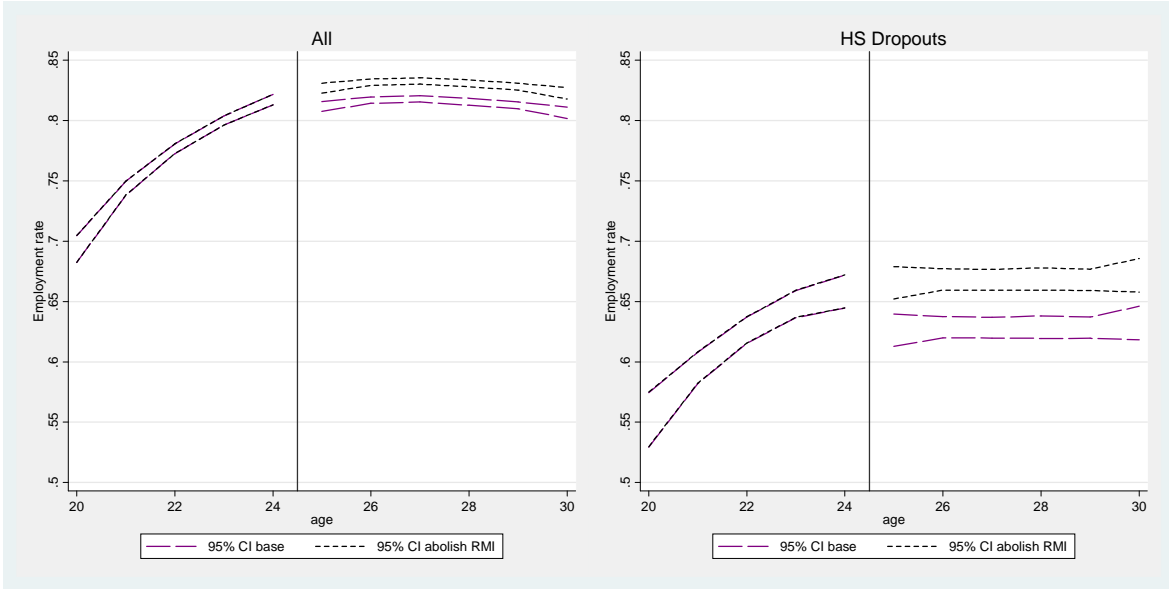
Note: Predicted employment rates from 1999 French Census data using structural model A, for baseline and abolishing the RMI (sample of 20-30 year old men and women available for work).

Abolishing the RMI.   Our first simulation examines the effect of abolishing the RMI. As expected, Figure 4 shows that removing the RMI would increase participation just over the 25-year-old threshold. This scenario is certainly not a political option but an interesting benchmark for comparison. In particular, comparing with Figure 3, we see that the RSA reform simulated earlier has almost the same relative effect on employment as that of removing the RMI, i.e. it brings the employment level of HS dropouts aged 25-30 to around $65 - 67\%$. Although more costly, the RSA scheme is certainly more

politically acceptable, generates important redistribution towards the poor and was the path actually taken by the French government in 2009.

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

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

Extending the RMI to the Youth.   Youth unemployment is a severe issue in France like in several EU countries. It has received renewed attention recently as it has become more accentuated in a recessionary context. As the young are more at risk of unemployment and less likely to have made enough contributions to claim unemployment benefit, the RMI can be an important source of income for them. Currently, their limited access to welfare programs results in very large poverty rates, as discussed in the introduction. This raises the question of extending the RMI to those under 25 years of age. Of course, this strategy runs the risk of increasing welfare dependency by fostering it at a younger age and of further increasing unemployment among young workers if inactivity traps exist. Figure 5 simulates the 1999 RMI scenario, abolishing the age condition. While this hypothetical reform has little effect on the whole sample, the HS dropouts show a negative employment response, similar to the one observed at the cutoff. Introducing the RMI for those under 25 induces a drop in participation of 5 ppt in this group. Symmetrically to the effect of abolishing the RMI, this shows that young workers with low wage prospects may be tempted to claim the RMI and live on welfare, which casts doubts on the desirability

23

of extending unconditional welfare payments to this group.

Figure 6: Counterfactual Employment Simulations: Extending RSA to the Young
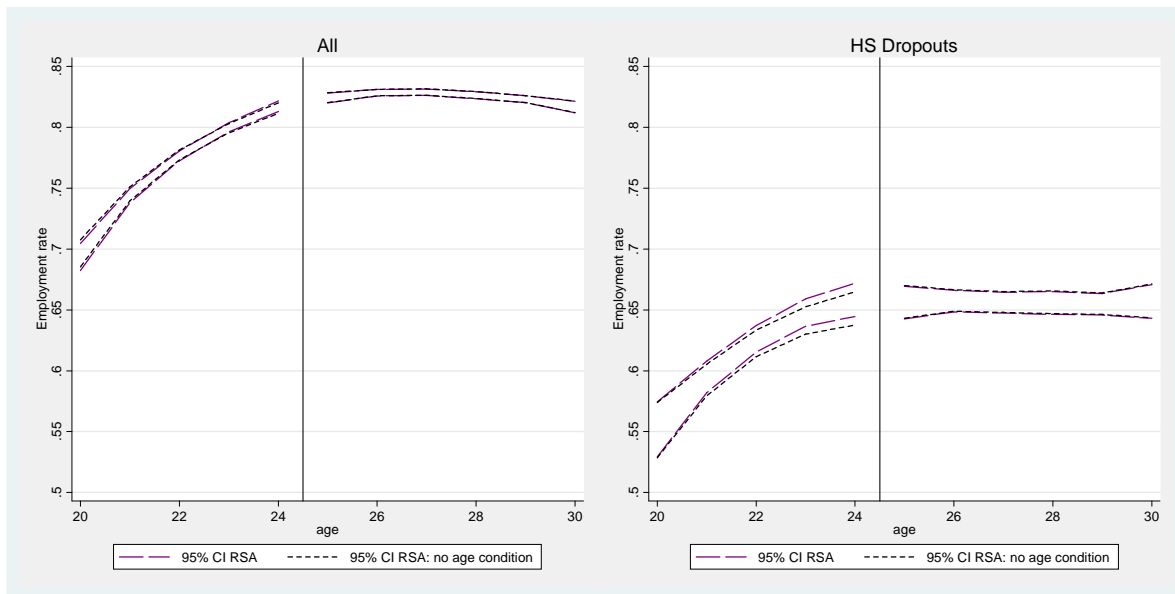
Note: Predicted employment rates from 1999 French Census using structural model A, for RSA scenario and removing the RSA age condition (sample of 20-30 year old men and women available for work).

Extending the RSA to the Youth. This calls for a last simulation: What would be the effect of extending the RSA scheme to the under-25 year-olds? This is a highly topical and relevant question in the current policy debate in France (see Bargain and Vicard, 2014).[16] We start with a baseline simulation of the RSA policy scenario and simulate a removal of the age condition. Extending the RSA to the young combines two opposite forces. On the one hand, we have seen that extending out-of-work welfare programs to the young creates disincentive effects for the under-25's, especially for the HS dropouts. On the other hand, the young can also benefit from in-work incentives with the RSA. The overall effect is undetermined. The results, in Figure 6, show that extending the RSA to the young 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".

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[16]An extension to 18-25 year olds was actually implemented in September 2011, although with very strict eligibility rules. A very small number of young workers have actually taken up this "junior RSA" so this should not affect our results on Census 2010-2011 in the previous sub-section.

6   Conclusions

We have studied the labor supply effect of the pre-2009 French social assistance program (RMI) around age 25, i.e. the age limit under which young workers are not eligible. This discontinuity provides a neat identification of the employment effect around the cutoff, which is estimated by RD on 1999 Census data. However, RD estimates do not allow predicting the effect of all types of counterfactual policy, notably the effect of decreasing the taper rate of the benefit, as implemented in the 2009 RSA reform. By doing so, this reform has introduced a generous in-work transfer component to the French redistributive system, and its re-incentivization effect needs to be assessed. To do so, we estimate a structural model identified on the same age discontinuity as in the RD design, i.e. financial gains to work change at age 25 due to the policy age condition.

Estimations of labor supply models on cross-sectional data rely on demographic heterogeneity combined with multiple nonlinearities or discontinuities in tax-benefit rules. These multiple sources of identification are rarely made explicit and are not necessarily robust (for instance, tax allowances for a third child cannot easily be used as exogeneous variation since having a third child also relates to specific preferences regarding fertility and labor supply). In constrat, we focus on a homogeneous group of single childless individuals around age 25 (the RMI age condition applies only to childless households). This provides us with a very clean setting since we can abstract from much of the demographic variation that can affect preferences and focus on the role of the age discontinuity in the identification of the model. Moreover, age is a source of variation upon which individuals have no control upon and which allows plausible exclusion restrictions (i.e. that preferences vary continuously with age).

As expected, our behavioral model fits the data well (it reproduces the participation drop at age 25 and predicts employment levels at other age levels satisfactorily). More impressively, the model shows good external validity. We check this by simulating the 2009 RSA reform and comparing its predicted employment effect with the actual effect assessed using data around the year 2009 (i.e., obtained as the differenced RD estimates before and after the actual reform). The predictions of our model closely mirror the actual effects of the reform under various model specifications. This informal check, despite being only suggestive evidence, is an important finding. Indeed, while labor supply models are widely used for policy simulation, their external validity is rarely tested in the literature. Moreover, going more structural helps answering questions a more reduced-form approach like the RD design could not answer, especially when we gain confidence in the model's out-of-sample predictions. Thus the model is used to simulate important reforms and shows that (i) extending redistribution toward the working poor thanks to

the RSA restores financial incentives to work among high school dropouts, (ii) this new redistributive policy could be extended to the under-25 year olds without creating new disincentive effects in this population.

We suggest a list of possible improvements. *First*, we have focused on a structural participation model. The extensive margin is, arguably, the primary dimension that merits investigation in the context of youth unemployment. This is surely the margin with the greatest degree of potential response in the short run, simply because people can always opt out of the labor market (in contrast, finding a different hour contract may be difficult and subject to constraints, cf. Chetty et al, 2011). In this respect it is, therefore, the best ground for reconciling structural models and natural experiments as we do here. Yet, the general discrete choice labor supply model presented in section 4.2 could be identified and estimated if additional sources of exogenous variation were found, e.g., other discontinuities affecting the financial gains to work part-time versus full-time. *Second*, external validity was checked using a change in the disincentive effect at 25 following changes in policy parameters. Ideally, we would also like to check the performance of the model at other age points further away from the discontinuity. This could be done, for instance, if the government changed the age condition from 25 to 22. *Third*, Census panel data (as available in Denmark, cf. Jonassen, 2013) could be used to check how far from the discontinuity we obtain good predictive power solely on the basis of the 25 year-old discontinuity. Using consecutive years of panel data could also be used to check employment rates for groups "crossing" the cutoff and to control for cohort effects.

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A   Wage Estimation, Simulated Income and Labor Supply Estimation

A.1   Wage Estimations

A central component of financial gains to work in equation (4) is the wage rate. When estimating structural models, it is standard to proceed in two stages, first with the estimation of a wage equation and wage prediction for non-workers, then with the estimation of the labor supply model. 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 $\theta(A_i)$, the set of detailed education categories $EDUC_i$ and additional controls Z_i (gender). The traditional labor supply literature has pointed to two issues relating to wage endogeneity. First, hourly wages may be partly determined by omitted unobservable variables (being hard working) which are associated with preferences, as discussed in section 2.1. 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 the age function $\theta(A_i)$, controls Z_i and disposable income at zero hours $C(0; A_i)$ as an instrument, relying again on the discontinuity at age 25 for identification. Second, calculated as earnings divided by worked hours, hourly wages may be contaminated by the same measurement error as those contained in worked hours. To avoid this so-called division bias, we predict wages for all observations, workers and non-workers, as suggested by Eklof and Sacklén (2000). Predicting for all makes it less of a concern to use one dataset for estimation (FLFS) and another for predictions (Census), as long as (i) the second data source provides accurate information on wages, (ii) both datasets contain the same variables, with identical definitions. As argued above and in 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 variables, and in particular the education categories in vector $EDUC_i$, are available in both datasets according the exact same definition. Thus we use estimates of equation (5) to predict wages for all individuals in the Census, drawing wage residuals ν_i in a normal distribution with zero mean and using their estimated empirical variance. 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.

Log hourly wage estimations using the FLFS data are reported in Table A.1 together with the reduced-form participation equation for the Heckman correction. A significant

gender gap can be observed, in line with the existence of a "sticky floor" effect in France (Arulampalam et al, 2007) as well as a regular wage progression with the level of education. The Inverse Mills ratio is not significant. In the participation equation, disposable income when out of work is negative, as expected. It is not statistically significant, probably due to the lack of power in the FLFS (Chemin and Wasmer, 2012, reproduce our RD results using 12 years of FLFS while we use here only the 3 years surrounding 1999).

We check the robustness of our wage imputation in Figures B.1 (men) and B.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 A.2 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×imputed wages in the Census), line up quite closely in the two datasets.

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

Variables	Log wage		Employment	
Age	-0.048	(0.023)	0.079	(0.099)
Age square / 100	0.001	(0.000)	-0.002	(0.002)
Female	-0.112	(0.007)	0.042	(0.027)
Junior vocational qualification	0.054	(0.011)		
Highschool diploma	0.168	(0.016)		
Vocational highschool dipl.	0.131	(0.013)		
Graduate qualification	0.352	(0.011)		
Disposable income 0 hours/100			-0.006	(0.017)
Inverse Mills ratio	-0.003	(0.101)		
Constant	4.177	(0.301)	-0.338	(1.263)
Observations	7,101		9,986	

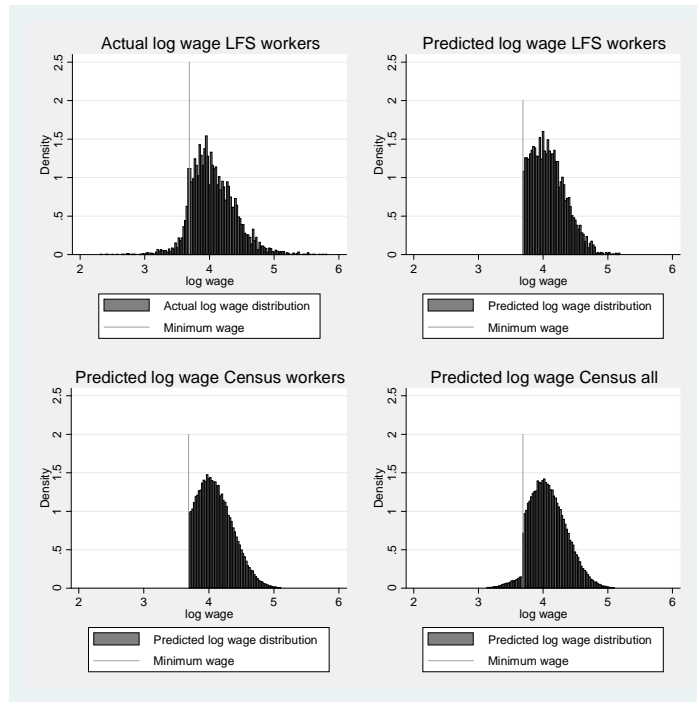*Selection: pooled French Labor Force Survey 1997-2001. Standard errors in parenthesis*

31

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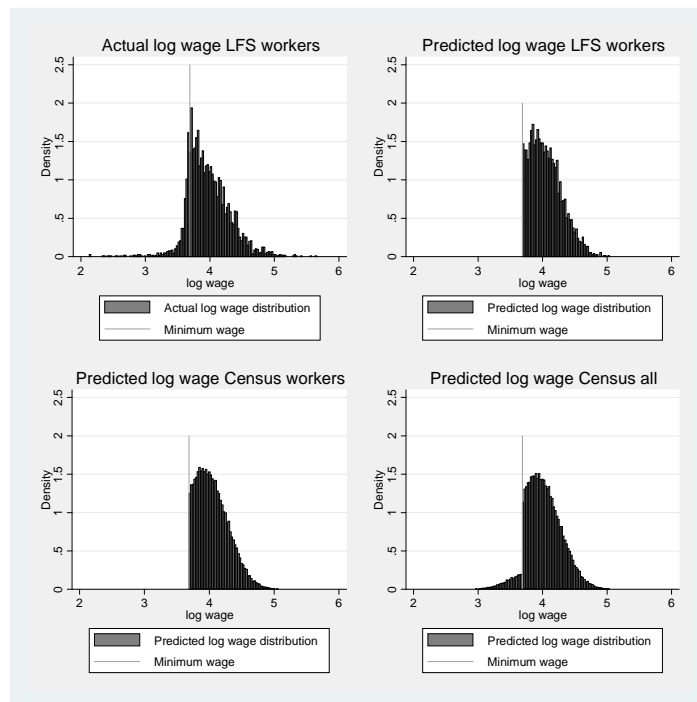

Figure B.2: Comparing Actual and Predicted Log Wage Distributions in FLFS and Census Data (Women)

A.2 Descriptive Statistics and Simulation of Disposable Incomes

Table A.2 reports descriptive statistics for the Census data and the FLFS (we present two alternative FLFS sample selections: a pool of years 1997-2001 to increase sample size and the 1999 wave). Results show that the distribution of demographic characteristics are very similar in the two datasets. 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). 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×predicted wages). Function $C(E; A)$ accounts for social contributions and taxes paid on labor income E as well as benefits received, which we approximate by very detailed numerical simulation of the French tax-benefit rules. For our selection of childless single individuals, simulated transfers essentially consist of the RMI (a function of age A) and housing benefits. Table A.2 shows that the levels of disposable income are consistent across the two data sources. As explained in the text, tax-benefit simulations are also used to calculate, for each individual, disposable incomes in and out of work, for the purpose of estimating the structural participation model. That is, disposable income $C(wH; A)$ is simulated at different worked hours H (zero and full-time) using imputed wages.

A.3 Labor Supply Estimations

Table A.3 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 uneducated (HS dropout) females with model A, which 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: Summary statistics for single childless 20-30 year olds in the Census and LFS

	All			Under 25			Over 25		
	Census	FLFS (pool)	FLFS	Census	FLFS (pool)	FLFS	Census	FLFS (pool)	FLFS
Proportion of men	0.56	0.58	0.57	0.51	0.54	0.51	0.58	0.60	0.60
Age	26	26	27	23	23	23	27.5	27	27
Education:									
Junior vocational qualification	0.26	0.25	0.25	0.29	0.25	0.26	0.24	0.25	0.24
Highschool	0.06	0.07	0.07	0.07	0.08	0.08	0.05	0.07	0.06
Vocational highschool	0.13	0.13	0.13	0.17	0.16	0.17	0.12	0.11	0.11
Graduate qualification	0.39	0.37	0.38	0.28	0.32	0.29	0.43	0.39	0.41
Dropouts	0.16	0.18	0.17	0.19	0.19	0.19	0.15	0.18	0.17
Work hours	30	32	26	29	31	31	31	33	32
Employment rate	0.81	0.83	0.81	0.79	0.83	0.81	0.82	0.83	0.81
Employment income*	1,534	1,440	1,429	1,392	1,267	1,228	1,583	1,510	1,510
Disposable income*	**1,032**	**1,132**	**1,136**	**893**	**983**	**926**	**1,081**	**1,190**	**1,217**
Sample size	202,093	9,986	2,040	53,048	2,833	570	149,045	7,153	1,470

Note: selection of childless single individuals between 20-30 years old. Data sources are the 1999 French Census, the pooled 1997-2001 French Labor Force Survey (FLFS) and the 1999 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 LFS data. All monetary variables are expressed in 1999 EUR/month. Employment income excludes zeros. Disposable income is found to be positive for all observations.

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

B   RD Analysis of the RMI Employment Effect

B.1   RD Estimates

We suggest here a detailed RD analysis. We start with a graphical investigation of the 1999 RMI employment effect. In Figure B.1, we plot actual employment rates by age along with the 95% confidence intervals using our selected sample from the 1999 Census. Note that these intervals, which indicate sampling errors, are different from the confidence intervals in Figure 2, which reflect prediction errors of model (4). As indicated in the main text, this graphical representation suggests a very small drop in employment at age 25 for the full sample but a larger drop for HS dropouts.

Turning to the RD model (1), in Table 2 we find estimates of β_i in a range between 3.9 and 5.8 percentage points for HS dropouts over all specifications of the model (age in years or quarters, $\delta(\cdot)$ as quadratic, cubic, quartic or quadratic spline). They are statistically significantly different from zero in all cases. The effect expressed in percentage points can be divided by the employment rate at age 24 for the HS dropout (67.7%) to give the proportion of people concerned by the disincentive effect at the discontinuity, i.e., between $5.3 - 8.6\%$ in this group. This order of magnitude is similar to estimates in Bargain and

Table A.3: 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.
Preference for work												
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	-0.026	0.009	-0.099	0.049	-0.100	0.049	-0.100	0.049	-0.007	0.003	-0.001	0.000
Age3	0.000	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000
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
Age*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*educated	0.014	0.010	0.047	0.057	0.050	0.057	0.051	0.058	0.004	0.003	0.000	0.000
Age3*educated	0.000	0.000	-0.001	0.001	-0.001	0.001	-0.001	0.001	0.000	0.000	0.000	0.000
Male*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
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
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
Coefficients on Age >=25												
Male	-0.020	0.010										
Male*educated	0.013	0.010										
Educated	0.019	0.014										
Constant	-0.027	0.013										
Coefficients on Income when H=0 (divided by 100)												
Male			0.025	0.016	0.024	0.016	0.024	0.016	0.026	0.016	0.026	0.016
Male*educated			-0.020	0.019	-0.026	0.019	-0.027	0.019	-0.020	0.019	-0.020	0.019
Educated			-0.014	0.025	-0.011	0.025	-0.011	0.025	-0.004	0.024	-0.004	0.024
Constant			0.038	0.022	0.039	0.022	0.039	0.022	0.033	0.021	0.033	0.021
Coefficients on Income when H=39 hours/week (divided by 100)												
Male			-0.052	0.013	-0.051	0.013	-0.051	0.013	-0.052	0.013	-0.052	0.013
Age					-0.002	0.002	0.021	0.037				
Age2							0.000	0.001				
Male*educated			0.012	0.014	0.015	0.014	0.015	0.014	0.012	0.014	0.012	0.014
Age*educated					-0.009	0.002	-0.048	0.041				
Age2*educated							0.001	0.001				
Educated			-0.067	0.012	0.166	0.063	0.661	0.528	-0.067	0.012	-0.067	0.012
Constant			0.217	0.011	0.270	0.057	-0.027	0.474	0.217	0.011	0.217	0.011
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 model is 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).

Doorley (2011) who focus on men only.

An important aspect is whether results are 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 have also checked 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).

Finally, we compare these results 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 B.2, there is no sign of a discontinuity at 25 for these two placebo groups.
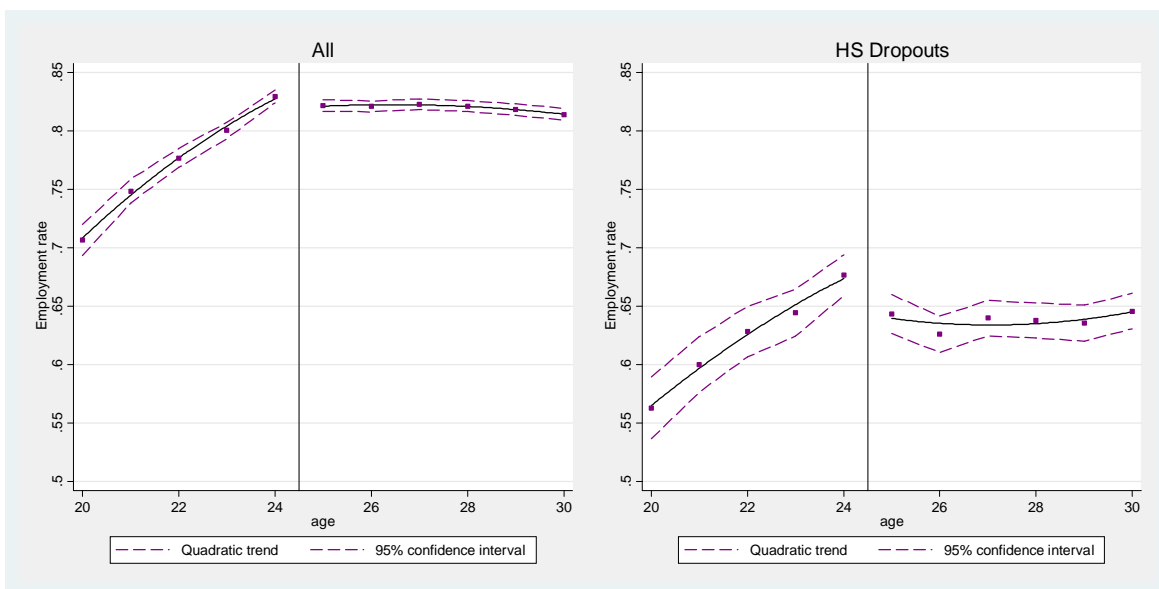

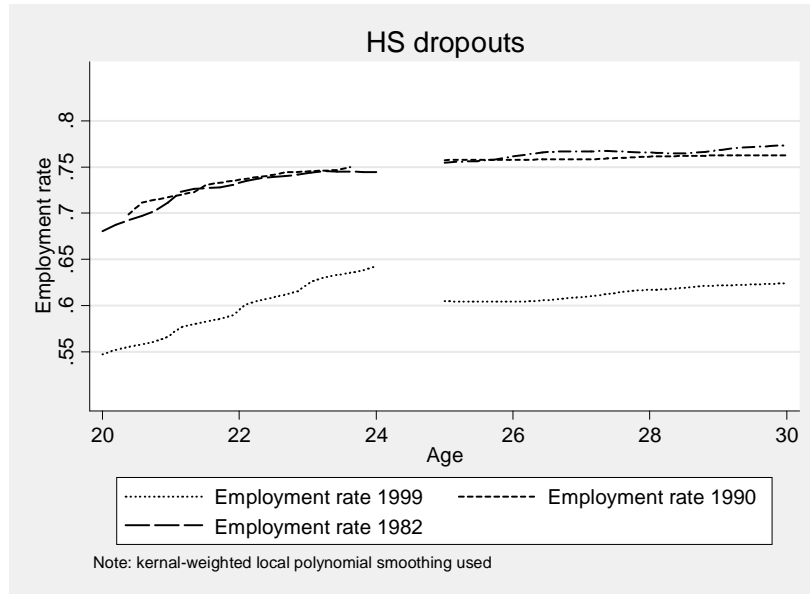Figure B.1: Employment Rate of Childless Singles and Discontinuity (Census 1999)

Figure B.2: Employment Rates of Childless Singles between 1982 and 1999 (Census)

B.2 Dynamics

The RD design in the case of an age-based discontinuity is a special case of the standard RD design (Lee and Lemieux, 2010). Assignment to treatment, i.e. eligibility for the RMI, is inevitable, as all subjects will eventually age into the program. Two issues arise in this case. Firstly, the discontinuity should be interpreted as the combined effect of all factors that switch on at the threshold. An extensive examination of any other potential influences on employment at age 25 is undertaken by Bargain and Doorley (2011) and summarized in section 3.1, confirming that there is no other factor at work at this age threshold, except the RMI. Secondly, because treatment is inevitable with the passage of time, individuals may fully anticipate the change in regime and adjust their labor market behavior before the threshold. In this case, optimizing behavior, in anticipation of eventual eligibility for the RMI, would accentuate observed effects.

We believe that this is implausible for a number of reasons (see further discussion in Bargain and Doorley, 2011). *First*, it seems unlikely that the group which displays the largest response to the RMI, HS dropouts, would be fully aware of the benefit rules and, thus, work more until they turn 25 in order to be able to drop out of the labor market at age 25. *Second*, for a 20-25 year old, eligibility for the RMI will certainly happen at age 25 but may also happen if the individual has a child in the meantime or cohabits with somebody who is eligible. We, however, observe no accelerated fertility or cohabitation rates before age 25, indicating limited anticipation effects in this respect. *Third*, we do find evidence that the share of HS dropouts on short-term contracts decreases discontinuously

after age 25, indicating that, rather than working more or harder, highschool dropouts are lingering in precarious activities until they become eligible for the RMI, at which point the cost of finding another short-term contract may seem large when a minimum income is guaranteed anyway. *Fourth*, a graphical inspection of the employment trends of 20-25 year olds in 1982 (before the introduction of the RMI), in 1990 and in 1999 shows little evidence of a time change in employment trends before the discontinuity (see Figure B.2).

Finally, after 25 years old, we notice a similarly flat employment pattern in both periods (1999 versus early years), which shows that the RMI disincentive effect is responsible for a change in employment levels at 25 but not for the shape of the employment curve after 25. As discussed in Bargain and Doorley (2011) and Lemieux and Milligan (2008), the flattening over 25-30 is due to a negative selection of childless single individuals on the labor market. These conclusions are reinforced by similar findings in Denmark. Using Census panel data, Jonassen (2012) confirms that the employment drop at the age cutoff corresponds to transitions out of work, which occur within 6 months after the 25th birthday.

C   Measuring Treatment Effect with the Behavioral Model

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:

$$\beta = \overline{Y}_{25} - \overline{Y}_{24} + \gamma.[\delta(25) - \delta(24)] \tag{6}$$

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

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

When assuming $b_1 = b_0 = b > 0$, this also corresponds to

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

i.e. a change in the financial gains to work between 25 and 24 years of age. This definition fails to account for the differentiated effect of age on wages at age 24 and 25, however.

Therefore, the correct measure of the policy effect at the cutoff requires the evaluation of the employment gap at age 25, accounting for the no-RMI counterfactual situation C^0:

$$[\{b_1 C(\widetilde{w}_i(25)H; 25) - b_0 C(0; 25)\} - \{b_1 C^0(\widetilde{w}_i(25)H; 25) - b_0 C^0(0; 25)\}].$$

The policy effect at the cutoff is therefore:

$$\overline{Y}_{25} - \overline{Y}_{24} + a^1.[\delta(25) - \delta(24)] \tag{8}$$
$$-b_1\{C^0(\widetilde{w}_i H; 25) - C(\widetilde{w}_i H; 24)\}$$

in the basic model where b parameters do not vary with age (model A) and recognizing that $C(0; 25) - C^0(0; 24) = 0$ by definition. Hence, the only difference with (7) is a correction for the difference in wage levels between age 25 and 24 in the last term. In the more general case, the effect is:

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