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

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ABSTRACT

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

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 analysis of alternative policy situations. However, their internal validity is often questioned. In this paper, we suggest combining the two approaches by putting structure on a regression discontinuity (RD) design. The RD estimation exploits the fact that childless single individuals under 25 years of age are not eligible for social assistance in France. The behavioral model is identified by the discontinuity and by an additional exclusion restriction on the form of financial incentives to work. We investigate the performance of the behavioral model for predictions further away from the threshold, check external validity and use the model to predict important counterfactual policies, including the extension of social assistance to young people and the role of in-work benefit components.

NON-TECHNICAL SUMMARY

We study the effect of the pre-2009 French social assistance program, the RMI, on labor supply. We find that eligibility for this program, which began at age 25 for single people, led to a drop of between 5 and 9% in the employment rate of young highschool dropouts. The replacement, in 2009, of the RMI by the RSA, which introduced an in-work benefit component to the transfer, is found to restore financial incentives to work and alleviate the inactivity trap for highschool dropouts. With this new system, which combines transfers to both workless and working poor, extending the program to under-25 year olds does not seem to create any significant disincentive effects to work. This is an important policy finding in the current context of very high youth unemployment and poverty in Europe.

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[*] The usual disclaimer applies.

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 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.[1]

In this study, we combine the two approaches, focusing on the labor supply effect of tax-benefit policies. We first rely on an age condition leading to a discontinuity in eligibility for the main social assistance program in France. We focus on the welfare program in place before 2009, a transfer to the workless poor (the *Revenue Minimum d'Insertion*, RMI). We exploit the fact that childless single individuals under 25 years of age are not eligible for this transfer. Estimates of the negative employment effect of social assistance are identified at the threshold using an RD design. To extrapolate further away from the discontinuity and perform counterfactual simulation, we add structure to the model. The labor supply model 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. The age discontinuity affects these financial gains and is used to identify the model. An additional exclusion restriction allows us to make predictions of participation responses at ages further away from the threshold.

This framework provides an illustration of how valuable it is to combine ex post and ex ante methods. The discontinuity guarantees credible identification of the structural

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[1] For instance, the recent development of collective models has allowed the shifting of welfare analysis from the household to the individuals level (cf. Vermeulen, 2001).

model while the latter 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 EITC-type of 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 (almost 11% when the poverty line is half the median income).[2] 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 and on the efficiency of in-work transfers such as those in place in the UK and the US (see Immervoll et al., 2007). 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 welfare system. 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 in 2009. We also find that extending the new welfare program to those under 25 years of age should not reduce participation significantly. Hence, it may help to reduce poverty in this group without further weakening their attachment to the labor market.

The paper is structured as follows. Section 2 reviews the literature, section 3 presents the institutional background and the data while section 4 explains the empirical strategy. Section 5 reports and analyzes the results while section 6 concludes.

2   Literature

2.1   Structural Labor Supply Models and (Quasi-)Experiments

A very large number of policy studies have relied on cross-sectional or panel data and structural models 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 their predictions to policy changes is not guaranteed. Maybe the main

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[2]Basically one youth out of four is unemployed. France has the largest youth unemployment in Europe after the four Southern European countries. Youth unemployment and youth poverty are also suspected to have additional external effects like increasing crime (cf. Fougère et al., 2009).

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 can not 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 allows the incorporation of all nonlinearities and discontinuities introduced by tax-benefit rules to individual budget curves. Exploiting this variation, together with variation in demographic characteristics, is the basic identification in most models (e.g. Laroque and Salanié, 2002, for France, van Soest, 1995, for the Netherlands).[3] Identification may also 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 bring the identification of structural models closer to the quasi-experimental approach.

Relatively independently from this, 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 US Earned Income Tax Credit (EITC) reform on the labor supply of single mothers. They find compelling evidence that single mothers joined the labor market in response to this incentive. Francesconi and Van der Klaauw (2007) use changes in the generosity of the UK Working Family Tax Credit (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).[4] Interestingly, this policy feature concerns a group

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[3]Indeed, two persons with the same gross wage but different family composition may face different effective tax schedules. This type of identification is parametric since demographics themselves affect labor supply. It must rely on some implicit assumption of preference continuity across demographic groups (and tax-benefit functions must be assumed to be sufficiently nonlinear to provide credible identification). Interestingly, the discontinuity under investigation in this study plays a similar role. Yet we only require that people just under 25 are identical to people just above 25, other things being equal.

[4]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).

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 households or single individuals with children). It is, however, important to infer policy responses for this group. Indeed, 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 that may follow an extension of social transfers to the under 25's, as motivated in the introduction.

2.2 Comparison

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. A systematic comparison of the employment effect of tax-benefit policies, as measured by ex post evaluation techniques, with those predicted using structural models is essentially absent from the literature. A few studies have recently pursued this comparison, carrying out ex post evaluations using difference-in-difference methods (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. In particular, the external validity of ex post evaluation methods is hard to test if the parameters of interest are endogenous to policies. In this case, (well identified) structural models may be used for that purpose. This motivates the approach of our study.

2.3 Using (Quasi) Experiment to Identify Structural Models

This attempt is not new. 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, 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. Arguably, there is more room for such work where (quasi) experimental variation is used to improve the identification of structural models.

In the absence of pure experimental data, the question of which type of natural experiment is suitable to identify behavioral models arises. In this paper, 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 the age discontinuity in the RMI, 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. 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. These considerations are guiding our approach. We also 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 of extrapolation to other subpopulations than those used for causal inference.[5] We show that combining RD with a structural behavioral model under minimalist assumptions allows us to perform counterfactual simulations for answering important policy questions.

3   Institutional Background and Data

Institutional Background.   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

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[5]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.

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.

Data. 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, however, corresponds to a peak year, with around one million RMI recipients and following a gradual expansion of the scheme from 1990 to 1999 (see Bargain and Doorley, 2011). Note that more recent Census years could be used in principle. Census data collection became annual starting in 2004. However, only 1/5 of the population is interviewed each year, resulting in samples of 1/20 of the population each year. For this reason and because of limited access to these new waves, we find it more appropriate to carry out our main analysis on the 1999 Census. Census data for years 2004-2011 is nonetheless used hereafter to study the RSA reform and to check the external validity of our model.

The Census provides data on age (in days), employment, type of contract, work duration, marital status and household type. Data on income and receipt of RMI or other benefits is, unfortunately, not available. Wage estimations are therefore conducted using the French

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Labor Force Survey (LFS), a panel survey conducted on an annual basis for the period 1990-2002. For cross-sectional use, the annual LFS 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).

Selection.   The selection is applied to both Census and LFS 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 clear interpretations 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 and Disposable Income Simulations.   Both Census and LFS data have comparable definitions of the key variables and, in particular, education categories.[6] Table 1 provides descriptive statistics (for LFS, we consider the sole year 1999 or, alternatively, a pool of years 1997-2001). We show that the two selected samples are comparable in terms of demographic and education structures, which gives confidence in the wage imputation we conduct hereafter. 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, 2012). The LFS shows larger employment rates (as reflected in the average employment figures in Table 1), a discrepancy that becomes smaller for older age groups. Given the smaller sample size of the LFS, employment levels by age also show a slightly more erratic pattern

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[6]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".

in these surveys. The overall trends are, however, very similar. For both samples, we also calculate disposable income $C(E; A)$ for each individual in the data, as a function of gross earnings E and age A.[7] This function accounts for social contributions and taxes paid on labor income 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. Importantly, Table 1 shows that the levels of disposable income are consistent across the two data sources. Note that we also use our simulations to generate disposable income levels under different labor supply choices, for the purpose of estimating a structural labor supply model. That is, we simulate disposable income when an individual is not working, $C(0; A)$, or when she is working H hours per week, paid at the wage rate w, $C(wH; A)$. As described below, we also calculate disposable income under hypothetical, counterfactual scenarios where (i) RMI is replaced by the 2009 RSA system, (ii) RMI is withdrawn from the French social system; (iii) the age condition for eligibility is removed; (iv) RMI is replaced by the 2009 RSA and extended to all (age condition removed).

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

	All			Under 25			Over 25		
	Census	LFS (pool)	LFS	Census	LFS (pool)	LFS	Census	LFS (pool)	LFS
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 (a)*	**1,032**	**1,132**	**1,136**	**893**	**983**	**926**	**1,081**	**1,190**	**1,217**
Disposable income (b)*	**1,002**			**855**			**1,054**		
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 Labor Force Survey (LFS) and the 1999 LFS. Disposable income is calculated using labor income and the EUROMOD tax-benefit simulator on the data. In Census data, it is calculated on the basis of two alternative wage imputations: (a) using wage estimation on LFS and (b) using wage matching with the LFS. * All monetary variables are expressed in 1999 EUR/month. Employment income excludes zeros. Disposable income is found to be positive for all observations.

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[7]Capital income is ignored as very small amounts are reported in this age group, especially for the low-educated youths that we focus on.

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 program on labor market participation.

4.1 RD Design

We start from Rubin's framework, denoting Y_i the participation binary variable 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 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 $\overline{A}$ as captured by any discontinuity at this threshold:

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

This RD design can be expressed parametrically. In fact, this becomes necessary when the forcing variable is discrete, which is a more reasonable framework when age is expressed in *years* or *quarters*. This is a more appropriate setting since it is not clear when the potential labor supply response should occur (after turning 25).[8] Also, age cells would be very small and would display a very erratic pattern if age is expressed in days. A discrete dependent variable means that we cannot compare observations "close enough" on both sides of the cutoff point to be able to identify the effect. Hence, we rely on various parametric functions of the forcing variable A in order to balance the usual trade-off between precision and bias (Lee and Card, 2008). Consider the regression model with

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[8]Using panel administrative data and a very similar setting for Denmark, Jonassen (2013) shows that transitions in and out of social assistance driven by the age condition take place within 26 weeks.

Y_i^* denoting the propensity to be employed for individual i:

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

The model is easily estimated by logit or probit techniques, with employment $Y_i = 1$ for those with $Y_i^* > 0$ and 0 otherwise. Alternatively, a simple linear probability model can be used by replacing Y_i^* by Y_i in (1) (see Lemieux and Milligan, 2008). The effect of age A_i on the outcome variable is captured by a smooth function $\delta(A_i)$ and by $T_i = I(A_i \geq \underline{A})$. Under the identification assumption of $\delta(\cdot)$ being a continuous function, i.e. the parametric version of Condition 1, the treatment effect β is obtained by estimating the discontinuity in the empirical regression function at the point where the forcing variable switches from 0 to 1. Note that coefficients vary linearly with a set Z_i of individual characteristics other than age (gender, education) and, so, are written with the subscript i. In particular, we may have (observed) heterogeneity in the treatment effect, with $\beta_i = \beta_i^0 + \beta_i^1 Z_i$. In particular, because of their weaker attachment to the labor market, HS dropouts may behave differently from other education groups so that we must differentiate the employment effect for HS dropouts from those with a degree.[9]

At this stage, it becomes clear that the RD design allows only limited extrapolation. The employment elasticity of social assistance parameters can be calculated. For instance, denoting $\overline{Y}$ the mean employment rate and focusing on the maximum benefit level R, we can derive the employment elasticity of a change in social assistance $\frac{d\overline{Y}/\overline{Y}}{dR/R}$ (around $-.05$ in Bargain and Doorley, 2011, and $-.04$ in Lemieux and Milligan, 2008). 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. 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:

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

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[9]We refrain from using more detailed education categories for comparability with the next model, as explained further below.

with disposable income $C(w_iH; A_i)$ (equivalent to consumption in this static framework) and hours worked 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 it accounts for the full tax-benefit rules in function C, this structural model is broadly used for policy analysis (see Blundell and MaCurdy, 1999). As previously discussed, identification often relies on the nonlinearity of this function or time/spatial variation in the tax-benefit rules. In our setting, we originally use the age condition in social assistance eligibility, creating exogenous variation in financial incentives, as the key source of identification. Since this discontinuity changes only the financial conditions 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:

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

In this way, the "disutility" of work or other components like work costs are specified through choice-specific terms a_{ij} and g_{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 better fits the data. In addition, coefficients in (3) vary linearly with several taste-shifters (gender, education) and possibly random terms for unobserved heterogeneity. While it is obvious that β_i, in the reduced form equation (1), is identified at the age discontinuity and cannot, itself, vary with age, we must impose such a simple restriction in the structural equation (3):

Condition 2 (*exclusion restriction*) *Marginal utility of consumption b_{ij} does not vary with age.*

In a standard labor supply model, this means that one of the usual taste shifters, age, is left out of some of the coefficients. This exclusion restriction is debatable, yet it is obviously the price for identification based on the age discontinuity and it is totally consistent with the reduced form RD equation. Moreover, age affects the utility function in other, relevant ways: (i) a direct effect $\delta(A_i)$, entering the utility function in an (additive) separable way and reflecting how age changes work preferences, fixed costs of work or search costs (these three components are usually not identified from each other, see van Soest et al., 2002) and (ii) an indirect effect through financial incentives C, since age is a determinant of both wages w_i and the tax-benefit function $C(\cdot, A_i)$.

Participation Model. With this setting, we now focus on the participation margin.[10] The choice of working full-time ($j = 1$) rather than staying out of the labor market ($j = 0$) depends only on the difference $Y_i^* = U_{i1} - U_{i0}$ so that only the coefficients on consumption are identified while the other ones are normalized to zero for the non-working option ($a_{i0} = g_{i0} = 0$). The quadratic term in consumption, in equation (3), is not necessary as we model participation only. The final model is very close to the specification used in Laroque and Salanié (2002). Dropping subscript 1 from coefficients a_{i1} and g_{i1}, we can write the propensity to be employed as:

$$Y_i^* = a_i + g_i.\delta(A_i) + b_{1i}.C(w_i H_1; A_i) - b_{0i}.C(0; A_i) + \epsilon_i \tag{4}$$

with $\epsilon_i = \epsilon_{1i} - \epsilon_{0i}$. The model is now very similar to the RD model in equation (1), with the same constant and smooth function of age $\delta(A_i)$, plus a term capturing the discontinuity effect. The main difference, however, is the structure put on the latter. The treatment effect, i.e. the age condition of the RMI, affects individual participation decisions through their financial incentives to work, now measured by the distance between disposable income when employed, $C(\widetilde{w}_i H_1; A_i)$, and disposable income when out of work, $C(0; A_i)$.[11] By focusing on a specific group of the population, i.e. childless singles, we rule

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[10]While the main reason for this choice is a clean identification (i.e. the age discontinuity only affects this margin), other motivations are given in the literature. Laroque and Salanié (2002) estimate female participation on French data and justify this focus by the small variability in work hours in France. Also, participation is the main margin of adjustment in the short-run (in particular, labor market frictions ensure that people cannot adjust their work duration beyond the mere choice to participate or not, cf. Chetty et al., 2011). In our application, we set H_1 to 39 hours per week, the institutionally set full time option in France in 1999.

[11]In practice, as can be seen in equation (4), we do not force the model to depend on the exact difference between these two income levels. Instead, we let them freely affect the probability of employment. Indeed,

out most of the usual sources of identification stemming, as explained above, from the nonlinearity of tax-benefit systems combined with variation in demographic composition. The identification of the model relies on the same behavioral assumption as in the RD design: (statically) optimizing agents decide upon their labor supply based on financial incentives, and those aged 25 have lower incentives to work than similar persons aged 24. As in the reduced-form model, coefficients vary with gender and education. The latter is simply a dummy for HS dropouts: in addition to lower wage prospects, which should be reflected in wages w_i, people with only compulsory education may have lower attachment to the labor market than individuals with a degree (see Beffy et al., 2006; Gurgand and Margolis, 2008). In a supply-side model, this can be rationalized in the form of larger search costs, i.e. participation costs (see van Soest et al, 2002). Notice that we refrain from using more detailed education categories for identification purposes. Indeed, detailed education is the main information identifying wages and, hence, cannot also be used in preferences. This exclusion restriction is common in the literature (van Soest et al., 2002). We also add unobserved heterogeneity in coefficient b_{1i}, that is:

$$b_{1i} = b_{1i}^0 + b_{1i}^1 Z_i + b_{1i}^2 u_i$$

where u_i is a random, normally distributed term u_i (with zero mean and variance σ_u^2). This term corresponds to the unobserved preference for work, so that the total distribution of the model is a mixture of a normal and an EV-I distribution. In this case, the model can be estimated by simulated maximum likelihood, as explained below.

4.3   Wages, Estimation Method and Discussion

Wage Imputation: Estimation and Matching.   The 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 to predict wages 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

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individuals may value additional income when not working in a different way from in-work earnings, simply because of different marginal utilities of consumption at the two labor supply points (but also for other reasons like fixed costs of work or the stigma effect when living on welfare).

by omitted unobservable variables (being hard working) which are associated with preferences, as discussed above. 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, the so-called division bias. To avoid this 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 (LFS) 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 definition. As argued above and in Chemin and Wasmer (2012), the LFS 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 (B.1) 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 workers cannot receive wages below the minimum wage, in principle, we discard ν_i draws leading to wages below this wage floor for individuals in work in the Census. The normality assumption may be a poor approximation for the specific population studied (childless single aged 20-30). Hence, we also suggest an alternative imputation method based on a matching approach. That is, for each individual in the Census, we pick a wage value randomly in his/her age-gender-education group in the LFS. Over a large number of draws, this is equivalent to imputing the conditional mean wage of a given cell plus an error term drawn from the empirical distribution (rather than from a normal distribution). Once again, we discard draws that lead to imputed wages below the minimum wage for those who are observed working in the Census, while those who do not work can earn any wage in the random distribution.

Estimation of the Structural Model. Model (4) is estimated by simulated maximum likelihood. 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 $\widetilde{\nu}_i$ 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

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probability over a number of draws for these terms, recalculating disposable income each time.[12]

Non-employment and Demand-Side. Non-employment can be rationalized by (i) low financial gains to work (low ν_i or high $C(0; A_i)$); (ii) high preferences for leisure (low u_i); (iii) classic unemployment (productivity below the minimum wage); (iv) "other" non-employment corresponding to frictional or cyclical unemployment. Our modeling of (i) and (ii) is very similar to Laroque and Salanié (2002). For (iii), we cannot estimate the wage equation jointly with the employment model in order to account for the probability of being rationed in the individual likelihood. Hence, we simply assume that workers cannot be paid below the minimum wage, as explained above, while non-workers may have such low productivities. For (iv), we take $a_i + g_i.\delta(A_i)$ as a non-identified combination of supply-side factors (work disutility or work costs) and demand-side factors (job search costs), as previously explained.[13] In Bargain and Doorley (2011), we show that HS dropouts and those with a basic vocational training have similar financial gains to work but the latter show no drop in employment at 25. This indicates that those with basic qualifications have more attachment to the labor market, i.e lower job search costs.[14]

5 Results

5.1 Basic Estimation Results and Comparisons

We first present the estimations of the RMI effect on youth employment in France using RD versus predictions of the participation model. Next, we provide further checks of the internal and external validity of the model. Other results are reported in Appendix B. In particular, wage imputation using both estimation and matching appears to reproduce reasonable estimates of the wage distribution in the Census data (see Appendix B.1). We also discuss estimates of both RD and structural models in detail (Appendix B.2).

RD Estimates. We first present a graphical analysis of the RMI effect. In Figure 1, we plot raw employment rates by age, along with 95% confidence intervals using our

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

[13]See van Soest et al. (2002) for a similar interpretation of involuntary unemployment in a supply-side framework. For a more structural modeling of the "other" non-employment, based on similar exclusion restrictions, see Laroque and Salanié (2002).

[14]Note that under the assumption stated in condition 2, this (reduced-form) part of the model is not RMI-dependent, so that our policy simulations should not be biased. Arguably, however, general equilibrium effects may invalidate this statement.

selected sample from the 1999 Census. We distinguish between the full sample and the sub-group of HS dropouts. The graphical representation of the discontinuity suggests a very small drop in employment at age 25 when all groups are taken into account. For HS dropouts, however, there is a significant drop, of around 4 percentage points (ppt). RD estimations of equation (1) confirm a magnitude of β_i in a range between 3.6 and 5.8 ppt for HS dropouts over all specifications of the model (age in years or quarters, $\delta(\cdot)$ as quadratic, cubic, quartic or quadratic spline). 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 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. 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 2, there is no sign of a discontinuity at 25 for these two placebo groups.

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) as assignment to treatment, i.e., eligibility for the RMI, is inevitable (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), 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

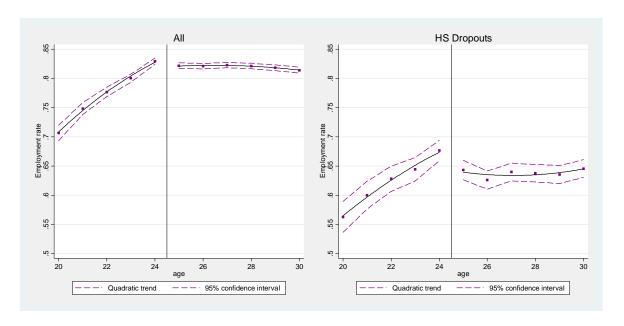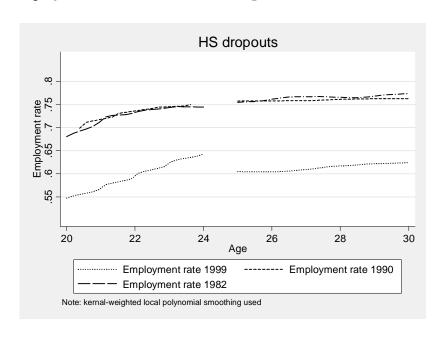Figure 1: Employment Rate of Childless Singles and Discontinuity (Census 1999)

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

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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. Finally, 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 2). The overall difference in employment rates between 1990 and 1999 is partly due to the steep increase in youth employment from the 1990's onwards and partly due to the fact that HS dropouts represent a smaller (and more negatively selected on the labor market) proportion of the overall population now than they did in the 1980's and 1990's.

Estimates of the RMI Employment Effect. Table 2 compares the predictions of the RMI employment effect from both RD design and the structural model, relying on a cubic function δ of age in both cases. The first columns report the actual employment rates at 24 and 25 years of age. The difference is -0.7 ppt in the broader group compared to -3.4 ppt among HS dropouts. In the RD framework, accounting for the age trends to extrapolate towards the threshold, we obtain treatment effects of -1.6 ppt and -3.9 ppt for these two groups respectively. Both effects are statistically significant and confirm a substantial negative effect of the RMI on singles. The effect is stronger for men, even if not significantly so. Turning to the behavioral model, we find slightly underpredicted rates of participation compared to actual ones. Nonetheless, the employment effect of the RMI predicted by the model is well in line with the RD results, i.e. -1.5 and -3.9 ppt for the whole selected sample and for HS dropouts respectively (see Appendix A on how to calculate the treatment effect in both cases). 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 slightly larger effects for men in this specification well. The effects are not significantly different from those of the RD approach.[15] Note that a good fit at the cutoff was expected, since the behavioral model simply translates the

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[15]Alternative specifications of the smooth function of age (quadratic, quartic) do not affect these conclusions qualitatively, and quantitative differences are relatively small (see Table 5).

discontinuity effect from a reduced-form to an income effect specification in the objective function. Performances of the model regarding predictions of employment rates further away from the cutoff or of policy reforms are less guaranteed – we check them in the next subsection. A final aspect we need to investigate here is whether the basic results are sensitive to the distribution of wages. We apply the alternative way to impute wages as discussed above, i.e. the matching procedure. For each observation in the Census, we simply draw a wage among the LFS observations within the same age-education-gender cell. Table 3 shows that with this imputation technique, the model predicts employment levels better than with the previous approach based on wage estimation. In contrast, it tends to underpredict slightly the employment effect of the RMI for the HS dropouts and to attenuate difference between gender. Nonetheless, estimates of the RMI effect are close enough to previous results and to RD estimates, i.e. a drop of 1.5 ppt overall and 3.4 among HS dropouts. We provide an interpretation of these results in the next sub-section.

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

	Actual Participation Rates			Predicted Part. Rates (Model)			RMI Effect (RD)		RMI Effect (Model)	
	Age 24	Age 25	Diff.	Age 24	Age 25	Diff.	Estim.	s.e.	Estim.	s.e.
All education groups										
All	82.9%	82.2%	-0.7	81.7%	81.2%	-0.6	**-1.6**	(0.4)	**-1.5**	(0.5)
Male	83.4%	83.3%	-0.1	82.8%	82.0%	-0.8	-0.7	(0.6)	**-1.7**	(0.5)
Female	82.4%	80.8%	-1.6	80.6%	80.2%	-0.4	**-2.5**	(0.7)	*-1.3*	(0.6)
HS Dropouts										
All	67.7%	64.3%	-3.4	65.8%	62.6%	-3.2	**-3.9**	(1.4)	*-3.9*	(1.5)
Male	70.5%	66.5%	-4.0	68.0%	64.5%	-3.5	*-4.2*	(1.8)	**-4.1**	(1.6)
Female	63.1%	60.8%	-2.3	62.3%	59.6%	-2.7	-3.4	(2.4)	*-3.6*	(1.8)

All figures are based on the 1999 Census data. Participation rates are predicted using the structural behavioral model (Model), with wage estimated from the Labor Force Survey. The employment effect of the RMI is estimated using the RD design (RD) or predicted using the behavioral model (Model). Both approaches rely here on a cubic age specification. Estimates significant at the 1% (5 or 10%) level in bold (italic). Standard errors are reported in brackets.

5.2 Checking for Internal and External Validity

We now check the internal and external validity of our model, combining identification from a natural experiment (the discontinuity) and standard behavioral assumptions (optimizing behavior based on financial incentives in a static framework). Internal validity refers here to the fit to the data while external validity refers to the ability of the model to correctly perform out-of-sample predictions or predictions of future policy scenarios.

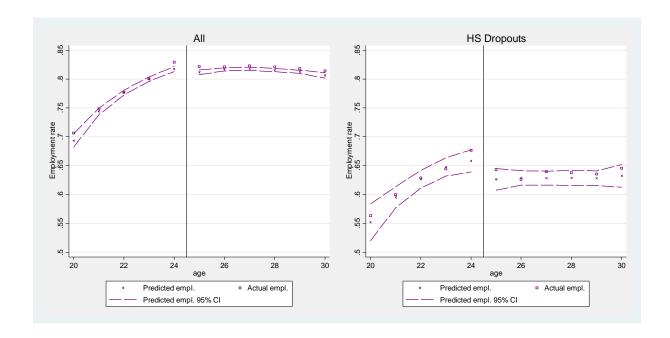Table 3: Employment Effects of the RMI: RD vs. Structural Model (Matched Wages)

	Actual Participation Rates			Predicted Part. Rates (Model)			RMI Effect (RD)		RMI Effect (Model)	
	Age 24	Age 25	Diff.	Age 24	Age 25	Diff.	Estim.	s.e.	Estim.	s.e.
All education groups										
All	82.9%	82.2%	-0.7	82.4%	81.9%	-0.5	**-1.6**	(0.4)	**-1.5**	(0.5)
Male	83.4%	83.3%	-0.1	83.3%	82.8%	-0.5	-0.7	(0.6)	**-1.5**	(0.5)
Female	82.4%	80.8%	-1.6	81.4%	80.8%	-0.6	**-2.5**	(0.7)	**-1.6**	(0.6)
HS Dropouts										
All	67.7%	64.3%	-3.4	66.5%	63.6%	-2.9	**-3.9**	(1.4)	*-3.4*	(1.5)
Male	70.5%	66.5%	-4.0	68.5%	65.9%	-2.6	*-4.2*	(1.8)	*-3.3*	(1.6)
Female	63.1%	60.8%	-2.3	63.2%	59.8%	-3.5	-3.4	(2.4)	*-3.6*	(1.8)

All figures are based on the 1999 Census data. Participation rates are predicted using the structural behavioral model (Model), with wage matching from the Labor Force Survey. The employment effect of the RMI is estimated using the RD design (RD) or predicted using the behavioral model (Model). Both approaches rely here on a cubic age specification. Estimates significant at the 1% (5 or 10%) level in bold (italic). Standard errors are reported in brackets.

Internal Validity: Employment Rates at Various Ages. A first check of the performance of the structural model is whether the model can predict employment rates well at all age levels. Figure 3 reports actual employment levels, predicted employment rates and confidence intervals at all ages (using specifications with a cubic function of age). We distinguish results for the whole selection and for HS dropouts respectively. The model actually shows a good fit for the entire selection of years around the discontinuity, which confirms the role of the discontinuity in the identification of the model. For almost all age groups, actual employment rates are in the predicted confidence intervals. Figure 4 shows the same type of result when matching is used for wage imputation. As commented above, employment rate predictions are even better in this case. The reason for this is that wage matching captures the actual wage distribution better, as shown in Appendix B.1. This approach does not impose normality on wage errors nor any specific functional form on the impact of education or age on wages. As a result, the predicted employment-age pattern in this case lines up very closely with the actual one, as can be seen in Figure 4. Yet, the wage estimation technique, although less flexible, is more suited to the RD approach in the sense that it imposes the same smooth function of age on wage predictions (the full model underlying Figure 3 thus depends on a cubic function of age in both preferences and wage determinants). Hence, it conforms more with the continuity assumption imposed through function δ and leads to a more continuous employment rate profile, as seen in Figure 3. As a result, while being less accurate in terms of employment level predictions, it is more accurate in its prediction of the employment drop at 25. The important conclusion is that, despite this trade-off, both wage imputation techniques (and the various specifications of δ) lead to similar conclusions. This is also true for the series of simulations to come. This

gives us confidence in the extrapolation we perform next using the structural model.
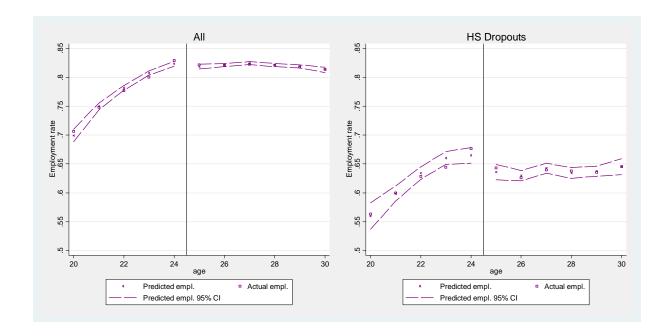
Figure 3: Employment Rate of Childless Singles: Fit of the Structural Model

Out-of-sample Prediction of the RMI Effect. We now rely on a cross-validation sample to provide a first check of the external validity of the structural model. We rely on two sub-samples for the same year of data (1999). We estimate our base model on a random half of the selected sample (estimation sample), and use estimates to predict employment rates and treatment effect on the other half (holdout sample).[16] Results are reported in Table 4. The first observation is that 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 "true" response as measured by the RD (-2.0 and -4.1 for the whole selection and

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[16]The advantage of such a strategy (see also Keane and Wolpin, 2007), compared to using another year of data, is that we do not need to control for time changes that may affect the sample and which could be different for the "treated" and the "control" groups (the main difficulty in difference-in-difference studies). However, a more advanced validation should rely on a "holdout sample" which would differ from the sample used in the estimation and whose policy regime is well outside the support of the data. For instance, we could simulate the withdrawal of the RMI and compare to the actual pre-RMI situation using 1982 Census data. This seems too old, however, to be used as a check for the simulation of a no-RMI situation in 1999. The next paragraph suggests another validation exercise of this type.

Figure 4: Employment Rate of Childless Singles: Fit of the Structural Model (Matched Wages)

for HS dropouts respectively). In line with the RD results, the model points to larger responses by single men compared to single women, both in the full sample and among HS dropouts. It predicts the RMI effect for men very precisely and seems to overpredict responses for women, yet both RD estimates and model predictions agree that the effect is not significant for the latter.

External Validity: Predicting the Effect of the 2009 Reform. We now attempt to compare model predictions of policy reforms with what effectively happened after these reforms. We first simulate the 2009 reform, which essentially reduced the withdrawal rate t from 100% to 38%, introducing an in-work-benefit component. This new minimum income is called the *Revenu de Solidarite Active* (RSA). The simulation of the RSA reform in Figure 5 shows a small positive effect on the over-25 employment rates for the whole selection. For the group of HS dropouts, it has a larger positive effects on employment rates, of about 3 ppt, which fades towards age 30. For the under-25, note that there is no effect since the age condition also applies to the new scheme. Focusing on HS dropouts, we report the employment effects of the RMI (using RD and model predictions) and of the RSA (using model predictions) in the left panel of Table 5. For the RMI, we observe slightly larger effects with the quadratic and the quartic functions of age, both in the RD

Table 4: Employment Effects of the RMI: using Cross-validation Samples

	Actual Participation Rates			Predicted Part. Rates (Model)			RMI Effect (RD)		RMI Effect (Model)	
	Age 24	Age 25	Diff.	Age 24	Age 25	Diff.	Estim.	s.e.	Estim.	s.e.
All education groups										
All	82.8%	82.3%	-0.5	81.8%	81.0%	-0.8	*-1.1*	(0.6)	**-2.0**	(0.7)
Male	83.6%	83.8%	0.2	82.6%	81.7%	-0.9	*-1.7*	(0.7)	**-2.1**	(0.8)
Female	80.9%	80.2%	-0.7	80.9%	80.2%	-0.7	-0.4	(0.7)	-1.8	(0.8)
HS Dropouts										
All	66.8%	64.1%	-2.7	65.7%	62.6%	-3.1	*-3.5*	(1.6)	*-4.1*	(2.1)
Male	70.9%	65.9%	-4.9	67.3%	64.0%	-3.3	*-4.1*	(1.7)	*-4.2*	(2.2)
Female	59.9%	61.2%	1.3	63.0%	60.3%	-2.7	-2.5	(1.8)	-3.9	(2.5)

All figures are based on a 50% sample of the 1999 Census data. Participation rates are predicted using the structural behavioral model (Model). The model is estimated on the other 50% sample (with wage estimated from the Labor Force Survey). The employment effect of the RMI is estimated using the RD design (RD) or predicted using the behavioral model (Model). Both approaches rely here on a cubic age specification. Estimates significant at the 1% (5 or 10%) level in bold (italic). Standard errors are reported in brackets.

and in the structural model. As previously discussed, however, estimates are in a range of reasonably similar magnitude, pointing to a disincentive effect of the RMI between -5.6 and -4.1 for men and -5.1 and -3.6 for women according to the behavioral model (comparing the two first columns, we also confirm that RD and model predictions are very close in all specifications). The RSA employment effect at age 25 (third column) 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 RMI and RSA effects points to a correction of the inactivity trap by around 3 ppt thanks to the RSA reform, with slightly larger effects for women (between 3.5 and 3.9) than for men (between 2.6 and 3). In the right panel of Table 5, we compare these results with RD estimates of the RMI effect before the actual reform took place (we pooled Census data for years 2004-2008 to obtain a sample size similar to the Census 1999, as explained in the data section above) and after it took place (we pool years 2010 and 2011, excluding 2009 since the reforms took place in the middle of that year). Results are very similar to the 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 reform. It is slightly smaller than in 1999, i.e. between -3.6 and -2.6 overall. Then, the two years under the RSA system show no disincentive effect at the cutoff. Finally, the differential effect between the two welfare regimes is positive and very close to our simulation, i.e. between 2.8 and 3.4 overall. RD estimates also confirm a slightly larger re-incentivization of the RSA for women compared to men. 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.[17] Notwithstanding, the proximity with our model prediction is reassuring – even if it is only suggestive evidence – about the external validity of the model and of the natural experiment underlying model identification.
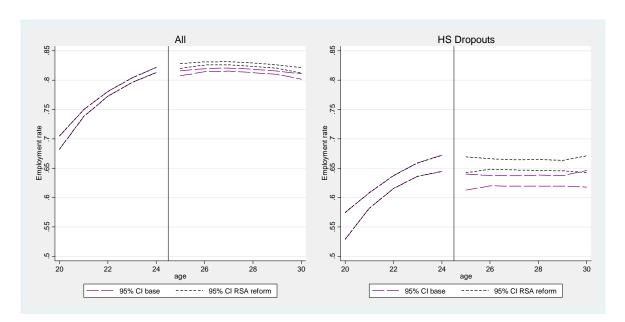
Figure 5: Counterfactual Employment Simulations: 2009 In-Work Benefit Reform (RSA)

5.3 Counterfactual Simulations

Finally, we use our behavioral model to predict important counterfactual policy scenarios. We provide results using the cubic specification of age and the wage estimation approach. Results with other specifications of δ are very similar (available from the authors) as are results based on wage matching (reported in Appendix C).

Policy Simulation: Abolishing the RMI. Our first simulation examines the effect of abolishing the RMI. As expected, Figure 6 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 5, we see that the RSA reform simulated earlier has almost the same relative effect on employment as removing the RMI, i.e. it brings the employment level of HS dropouts

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[17]As explained in the data section, Census data since 2004 is collected on an annual basis and for 1/5 of the population each year. Our RD estimates for the RSA thus rely on a sample for years 2010-2011 corresponding to 2/5 of the size of the sample used for our main analysis.

Table 5: External Validity of the Structural Model: Employment Effect of the RSA Reform

Age specification	Census 1999				Census 2004-2011		
	1999 RMI effect (RD)	RMI effect (model)	RSA effect (model)	Diff.	2004-08 RMI effect (RD)	2010-11 RSA effect (RD)	Diff.
	(1)	(2)	(3)	(3) - (2)	(4)	(5)	(5) - (4)
Quadratic							
All	-5.8	-5.4	-2.5	3.0	-3.6	-0.8	2.8
	(1.1)	(1.4)	(1.4)	(2.0)	(1.0)	(1.5)	(1.4)
Men	-5.8	-5.6	-3.0	2.6	-3.1	-0.6	2.6
	(1.9)	(1.4)	(1.4)	(2.0)	(1.3)	(2.2)	(2.0)
Women	-4.2	-5.1	-1.6	3.5	-5.0	-1.2	3.8
	(1.8)	(1.7)	(1.7)	(2.4)	(1.5)	(4.9)	(4.2)
Cubic							
All	-3.9	-3.9	-1.0	3.0	-2.6	0.8	3.4
	(1.4)	(1.5)	(1.5)	(2.2)	(1.6)	(2.3)	(2.1)
Men	-4.2	-4.1	-1.5	2.6	-2.2	0.8	3.0
	(1.8)	(1.6)	(1.6)	(2.2)	(1.9)	(3.1)	(2.8)
Women	-3.4	-3.6	-0.1	3.5	-3.5	1.3	4.8
	(2.4)	(1.8)	(1.8)	(2.5)	(1.9)	(6.1)	(5.2)
Quartic							
All	-4.5	-4.6	-1.3	3.3	-2.9	0.2	3.0
	(1.6)	(1.8)	(1.8)	(2.5)	(1.5)	(1.4)	(2.1)
Men	-6.2	-4.8	-1.9	3.0	-2.6	0.2	2.8
	(2.0)	(1.8)	(1.8)	(2.5)	(1.9)	(2.4)	(2.3)
Women	-2.2	-4.4	-0.4	3.9	-3.7	0.2	4.0
	(2.6)	(2.0)	(2.0)	(2.9)	(1.9)	(5.6)	(4.8)

Selection: childless single individuals aged 20-30, HS dropouts. Estimates represent the employment effect of RMI/RSA according to RD design (RD) or to predictions of the structural model (model). Simulation of a shift from RMI to RSA using the model estimated on 1999 Census data is compared to the actual policy chance (replacement of RMI by RSA in 2009). Both RD and structural model rely on alternative smooth functions of ages (quadratic, cubic or quartic). Standard errors in brackets.

aged 25-30 to around $65-67\%$. Even if more costly, the RSA scheme is certainly more politically acceptable and was the path taken by the French government in 2009.
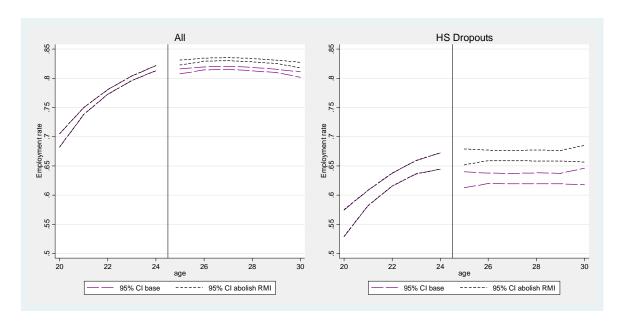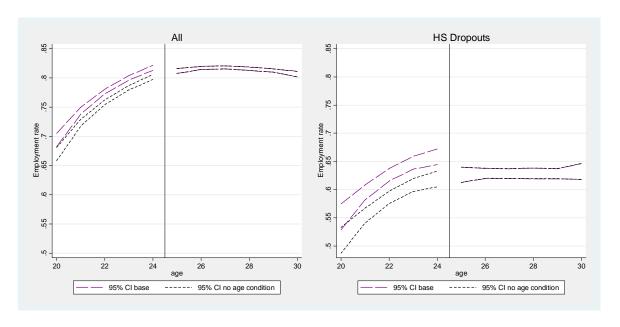
Figure 6: Counterfactual Employment Simulations: Abolishing the RMI

Policy Simulation: 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 becomes even 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 7 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 of extending unconditional welfare payments to this group.

Figure 7: Counterfactual Employment Simulations: Extending RMI to the Young

Policy Simulation: 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? This is a highly topical and relevant question in the current policy debate in France (see Bargain and Vicard, 2012).[18] We start with a baseline simulation of the RSA policy scenario and additionally 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 8, 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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[18]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 "RSA junior" so this should not affect our results on Census 2010-2011 in the previous sub-section.
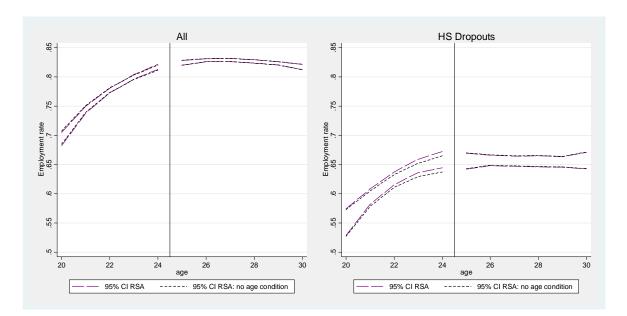
Figure 8: Counterfactual Employment Simulations: Extending RSA to the Young

6   Conclusions

We study the labor supply effect of the pre-2009 French social assistance program around age 25, i.e. the age limit under which young workers are not eligible. This discontinuity provides a neat identification of the policy effect around the cutoff. However, RD estimates do not allow extrapolation further away from it or the simulation of alternative systems. Hence, we estimate a more structural model identified on the same discontinuity and on an additional exclusion restriction which allows extrapolation. The model reproduces the participation drop at age 25 and also predicts employment levels at other age levels satisfactorily. It also allows the simulation of counterfactual policies. Compared to recent RD results for the 2009 reform, the model performs relatively well in showing that this reform and, notably, its in-work benefit component, restores financial incentives to work and alleviates the inactivity trap for HS dropouts. With this new system which combines transfers to both workless and working poor, the extension to the under-25 year olds does not seem to create any significant disincentive effects.

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. Note, however, that the more general labor supply model presented above could be identified and estimated using additional sources of exogenous variation, e.g., other discontinuities affecting the financial gains to work part-time versus full-time. We leave this for future research. Moreover, labor supply models rarely account for the interaction between labor supply adjustment and the demand-side of the economy. Future work should integrate the two approaches more systematically. Finally, the structural model should be tested more formally, notably the exclusion restriction that allows extrapolation further away from the age cutoff. For this, better data are required. For instance, consecutive years of Census data with changes in the nature of the discontinuity could be used to control for year (business cycle) effects and age effects while checking the prediction of the model regarding changes in the size of the social welfare discontinuity over time.

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A   Measuring the Treatment Effect

We can use the structural model to predict employment levels at 24 and 25, and check whether predictions reproduce the actual discontinuity in employment-age patterns. The age differential in employment levels is not exactly equal to the treatment effect, however. 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:

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

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

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

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 counterfactual situation C^0 (no RMI):

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

The policy effect at the cutoff is therefore:

$$\overline{Y}_{25} - \overline{Y}_{24} + g.[\delta(25) - \delta(24)] \tag{8}$$
$$+ b_0\{C(0;25) - C^0(0;24)\} - b_1\{C^0(\widetilde{w}_iH;25) - C(\widetilde{w}_iH;24)\}$$

In this formula, $C(0;25) - C^0(0;24)$ is zero 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.

B Wage and Labor Supply Estimations

B.1 Wage Imputation

Log hourly wage estimations using the LFS data are reported in Table B.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 LFS (Chemin and Wasmer, 2012, reproduce our RD results using 12 years of LFS 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 first row shows that actual and predicted log wage distributions for workers in the LFS are relatively comparable, with the exception of the few observations below the minimum, a situation that we rule out in our predictions. The middle-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 LFS (top right graph). This confirms that comparable distributions of socio-demographics in both surveys allows comparable predictions of the wage distribution. The second row also compares the log wage imputation in the Census based on linear estimation to the other method based on matching (see section

4.3). The latter actually compares much better to the actual (log) wage distribution, i.e. the top right graph, and captures, in particular, the density of observations just above the minimum wage. Finally, the last row of each Figure compares the distributions of predicted (log) wages for the whole Census selection, i.e. both for workers and non-workers, using both methods. Moving from wages to disposable incomes, we have seen in Table 1 that predicted disposable incomes – calculated using tax-benefit simulation and either actual gross incomes (in the LFS) or work duration and imputed wages (in the Census) – line up quite closely in the two datasets.

B.2   Labor Supply Estimations

Table B.2 shows the estimates of the RD model and of the participation model. The constant for the RD model is in line with the treatment effect for uneducated females as reported in Table 2 (-3.3). 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 four times smaller, which could reflect (i) the fact that financial incentives depend primarily on income prospects on the labor market, (2) the negative effects attached to welfare payments (e.g., stigma), (3) other reasons including the lack of variability in $C(0, A)$ for the identification of a differentiated effect. For educated females, the effect of welfare income is reduced by half in each model.

Table B.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 LFS 1997-2001. Standard errors in parenthesis

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

	RD		Behavioral Model	
	Coefficient	s.e.	Coefficient	s.e.
Preference for work				
Age	0.721	0.221	2.837	1.217
Age2	-0.027	0.009	-0.104	0.049
Age3	0.000	0.000	0.001	0.001
Age*educated	-0.366	0.244	-1.206	1.421
Age2*educated	0.015	0.010	0.051	0.057
Age3*educated	0.000	0.000	-0.001	0.001
Male	0.061	0.005	0.557	0.059
Male*educated	-0.031	0.005	0.043	0.029
Educated	3.228	1.994	10.869	11.635
Constant	-5.857	1.805	-27.041	9.953
Coefficients on Age >=25				
Educated	0.027	0.012		
Male	-0.009	0.004		
Constant	-0.033	0.012		
Coefficients on Income when H=0 (divided by 100)				
Educated			-0.025	0.022
Male			0.011	0.008
Constant			0.047	0.020
Coefficients on Income when H=39 hours/week (divided by 100)				
Educated			-0.070	0.007
Male			-0.039	0.005
Constant			0.214	0.007
Log Likelihood			-91701	
prob > chi2			0	
Observations	202093		202093	

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.
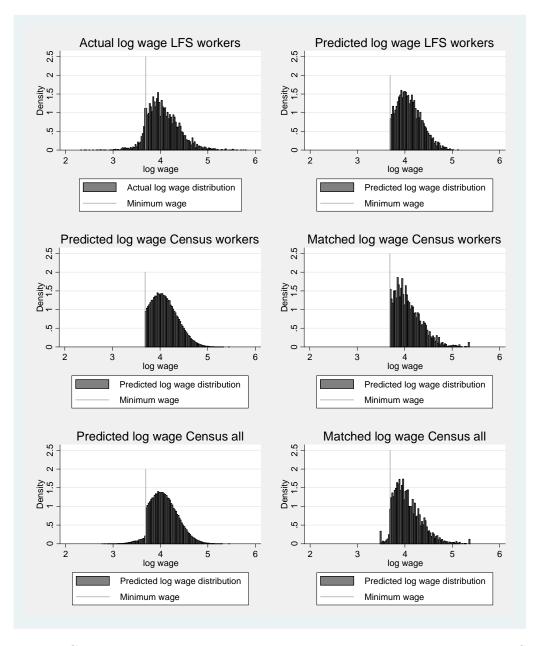
Figure B.1: Comparing Predicted and Matched Log Wage Distributions in LFS and Census Data (Men)
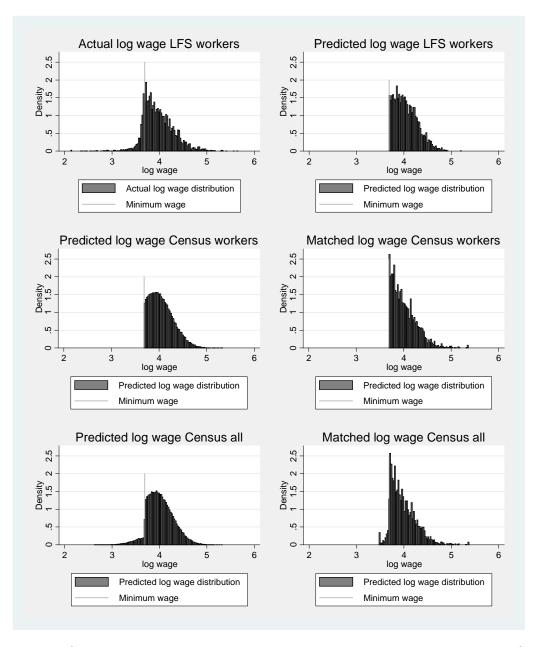
Figure B.2: Comparing Predicted and Matched Log Wage Distributions in LFS and Census Data (Women)
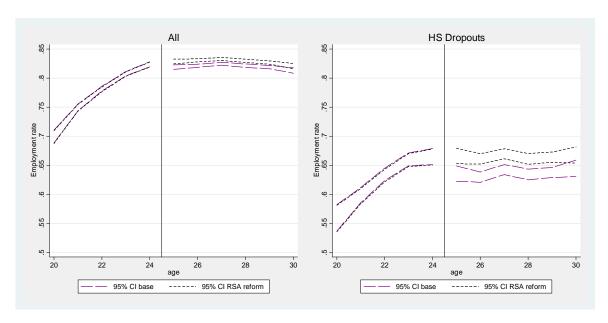
C   Results with Matching Wage Imputation

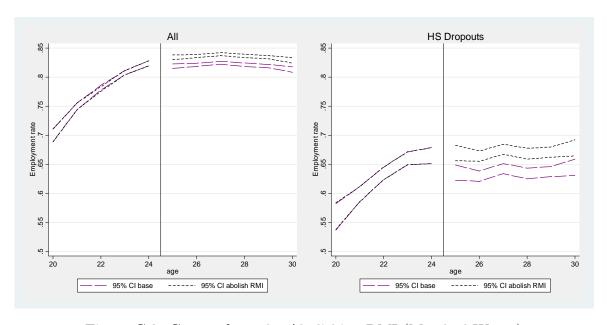Figure C.1: Counterfactuals: Replacing RMI with RSA (Matched Wages)

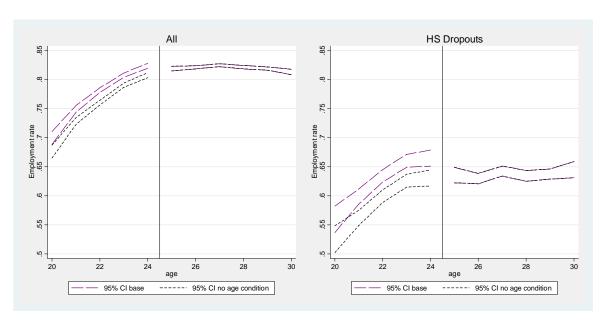Figure C.2: Counterfactuals: Abolishing RMI (Matched Wages)

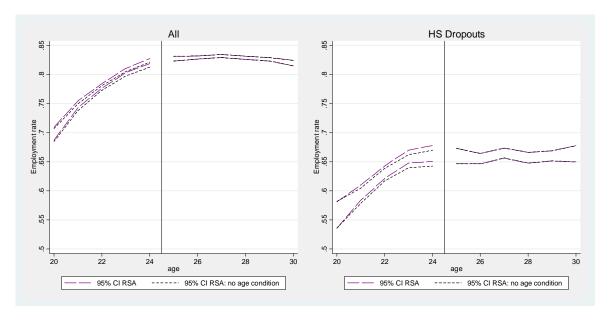Figure C.3: Counterfactuals: Extending RMI to the Young (Matched Wages)

Figure C.4: Counterfactuals: Extending RSA to the Young (Matched Wages)