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 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 and use it to predict important counterfactual policies, including the extension of social assistance to young people and the role of in-work benefit components.

Key Words: discrete-choice, labor supply, regression discontinuity
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1 Introduction

The recent debates in the economic literature tend to compare the different approaches existing for policy evaluation (Angrist and Pischke, 2010, Deaton, 2009, Heckman and Urzua, 2010). It seems more reasonable to try to combine them optimally (Blundell, 2012). In particular, the economic literature should attempt to reconcile the approach based on randomized or natural experiments (ex post policy evaluation) with that 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 (Angrist and Pischke, 2009). In contrast, ex post evaluation methods provide credible identifying assumptions. However, 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. That is, ex post evaluation cannot be used systematically to make predictions about future or alternative policies (Rosenzweig and Wolpin 2000, Heckman and Vytlacil 2005).[1] This explains why structural models are still broadly used by policy analysts – they allow us to perform ex ante policy analyses and, hence, are extremely useful for policy advice and policy design. By modeling the complete objective functions (e.g. utility functions), the ex ante approach can also be used to perform welfare analysis.[2] Against this background, it is clear that cross-fertilization between ex post and ex ante methods is needed. Experiments or quasi-experiments could be used to improve the identification of structural models. In turn, in ex ante evaluations, channels of causal effects and mechanisms are derived from theory and the estimated parameters have a clear economic meaning. Hence, (well identified) structural models can be seen as a convenient framework to interpret and decompose policy effects as well as to check the external validity of the identifying assumptions provided by quasi-experiments.

[1]A recent literature in public economics combines the advantages of reduced-form strategies – transparent and credible identification – with the ability of structural models to make predictions about counterfactual outcomes and welfare. This recent work has developed "sufficient statistics", i.e. formulas for the welfare consequences of various policies that are functions of high-level elasticities rather than deep primitives (see the overview by Chetty, 2008).

[2]For instance the recent development of collective models and their ability to shift welfare analysis from the household to the individuals level (cf. Vermeulen, 2001).

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 structural 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 structural and reduced-form evaluation methods. While the RD design guarantees the internal validity of this natural experiment ("as good as random" around the discontinuity), it does not allow extrapolation further away from the cutoff or the simulation of alternative welfare systems. Combined with the structural model, however, it 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).[3] Studying the discontinuity on age eligibility for welfare transfers is not only relevant for France, as they 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.

[3]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).

We find that the 2009 system restores 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 limited literature comparing or integrating structural and quasi-experimental approaches. Section 3 presents the institutional background and the data while section 4 explains the empirical strategy in detail. 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 Blundell and MaCurdy, 1999). As argued in the introduction, the internal validity of their predictions to policy changes is not, however, guaranteed. Maybe the main identification issue concerns the endogeneity of wages and preferences. That is, omitted variables (being a "hard working" person) could positively affect gross wage rates and consumption-leisure preferences simultaneously. In the older generation of labor supply models (Hausman, 1981), identification is provided by exclusion restrictions and hinges on the validity of instruments. More recently, discrete choice models have been used. Contrary to the Hausman approach, which could only handle piece-wise linear budget constraints (or convexified budget sets, e.g. Bourguignon and Magnac, 1991, for France), they allow us to account for the effect of the complete tax-benefit system on individual budget constraints (e.g. Laroque and Salanié, 2002, and Gurgand and Margolis, 2008, for France). Identification relies, in this case, on the nonlinearities and discontinuities in tax-benefit rules, together with variation in demographic characteristics (van Soest, 1995), i.e., two persons with the same gross wage but different family composition may face different effective tax schedules.[4] Identification may also be obtained from exogenous variation in tax-benefit rules

[4] This type of identification is parametric since demographics themselves affect labor supply. It must rely on some implicit assumption of preference stability 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 the effect we identify is local, i.e. around age 25, and we require only that people just under 25 are, other things being equal, identical to people just above 25.

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. Yet the usual concerns about the validity of the control group arise (are the states with higher taxation similar enough to those with low taxation?). When policy reforms are used, identification must rely on many years of policy changes while, at the same time, the assumption of constant preferences of agents over the middle or long run can be difficult to uphold.[5]

Relatively independently from this, there is a strong history of using natural experiments to quantify labor supply. Notably, natural experiments that exploit important US/UK tax-benefit reforms have been extensively used to identify labor supply responses. For example, Eissa and Liebman (1996) use a difference-and-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 an RD design and a difference-in-difference approach, 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 30 years of age 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. Interestingly, this policy feature addresses the question of 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 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. In the same line of research, Wasmer and Chemin (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).

[5] Similar identification strategies relying on tax reforms over time are used in the more reduced-form approach, consisting of the estimation of the elasticity of taxable income (see Saez et al., 2012, for an overview). Yet, in this case, estimated parameters are treatment effects on the treated so that, in principle, they cannot be used to extrapolate policy advice at the population level.

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. He concluded that they could not do so systematically. A more systematic comparison of the employment effect of tax-benefit policies, as measured by ex post evaluation techniques, with those predicted using structural models is not present in the literature. A few studies have nonetheless recently pursued this comparison, carrying out ex post evaluations using either natural experiments (Blundell, 2006, Pronzato, 2012, Lise et al., 2005, Cai et al., 2007, Hansen and Liu, 2011, Geyer et al., 2012, Thoresen et al., 2012) 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 rely on different identification assumptions which may be competing – with clear advantages in favor of (quasi) experiments as previously discussed – but also complementary. Acknowledging this fact, our approach follows a different path. Rather than a mere comparison, we suggest using natural experiments to directly identify structural models.

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 assesment 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; Duflo, Hanna, and Ryan (2007) who look at the effect of monitoring and financial incentives on teacher's absences, and Athey, Levin and Seira (2004) who use randomized assignment of auction formats to estimate structural models of bidding behavior. There is more room for such work where (quasi) experimental variation is used to improve the identification of the structural models.[6]

In the absence of experimental data, the question of which type of natural experiment would be suitable to identify behavioral models arises. In this paper, we suggest using RD as one of the simplest and "cleanest" form 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 (Hahn et al., 2001). 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 have a limited degree of external validity. Without strong assumptions justifying extrapolation to other subpopulations (e.g., homogeneity of the treatment effect), RD designs do not allow the researcher to estimate the overall or global average effect of the treatment. We show that combining RD with a structural behavioral model under minimalist assumptions allows us to make important extrapolations for answering 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 reform that we describe and simulate below. 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 (defined at 40% of the median of

[6] The field of labor supply is an interesting domain to integrate both approaches. Indeed, the practical difficulty in identifying, and precisely estimating the full array of structural parameters appeared primarily in this field. According to Heckman and Urzua (2010), Hausman (1981) is one of the papers that "fueled the flight of many empirical economists away from structural models".

equivalized incomes). In practice, entitlement to the RMI does not include any obligation to actively seek work and 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 $C(E, A) = S(E) + max(0, R - t.E).1(A_i \geq 25)$ with A denoting age in years. 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 months and 162% more than $C(0, 24)$. For RMI recipients who have just taken up a job, it is possible to cumulate earnings and some RMI for a short period. After this period, 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. 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 *Labor Force Survey* (LFS), a panel survey conducted on an annual basis for the periods 1982-1989 and 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, net income, 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.

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, nonetheless, 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. Both Census and LFS data have comparable definitions of education categories, which is crucial for wage imputations.[7] Table 1 provides descriptive statistics. 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 – see also Bargain and Vicard (2012) – precisely compares the employment-age patterns within the two data sources, using the ILO definition in both cases, for people aged 20-30. 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 in these surveys. The overall trends are, however, very similar which is important in this study.

Simulation. 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.[8] This function accounts for social contributions and taxes paid on labor income as well as benefits received, which we approximate by numerical simulation of the French tax-benefit rules. Simulated transfers consist of the RMI and housing benefits, the two main transfers for which our selection of childless single individuals without disability are eligible. Function C depends on age, denoted A, since benefits, like the RMI, are conditional on age. Importantly, Table 1 shows that the levels of disposable income are consistent across the two data sources. Disposable income can also be simulated for alternative labor supply choices, as used hereafter. That is, we can 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, $C(wH;A)$.[9]

[7] 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".

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

[9] As explained later, we shall focus on the participation margin and, hence, set H to 39 hours per week, the institutionally set full time option in France in 1999.

Finally, we can also calculate disposable income under hypothetical, counterfactual scenarios where (i) RMI is completely withdrawn from the French social system, C^0; (ii) the age condition for eligibility is removed, C^{11}; or (iii) RMI is replaced by the 2009 RSA system which includes an in-work benefit but maintains the age condition, C^{02}; (iv) RMI is replaced by the 2009 RSA but extended to all, C^{22}, as described in section 5.3.

Table 1: Summary statistics for single childless 20-35 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
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 single individuals between 20-35 years old without children. Data sources are the 1999 French Census, the pooled 1997-2001 Labor Force Survey (LFS) and the 1999 LFS. Disposable income calculated using employment income and the EUROMOD tax-benefit simulator on the data. All monetary variables in EUR/month. Employment income excludes zeros, disposable income >0 for all. Statistics from the Census are also very comparable to a third data source, the Household Budget Survey (for all, employment rate of 0.80, mean disposable income of 851)

4 Empirical Approach

The problem of identification in labor supply models relates to the fact that observed choices are influenced both by consumption-leisure preferences and by financial incentives (wages and tax-benefit policies). Preferences are unobserved, wages are unobserved for non-workers or introduce endogeneity problems for workers (the so-called division bias, that we address below). Hence, exogenous variation in tax-benefit rules is required for identification. Discrete choice models fully account for the impact of tax-benefit policies on the budget constraint, so that it is possible to obtain identification from tax-benefit variation across space or over time. More common but less reliable identification relying on tax-benefit nonlinearities, combined with different socio-demographic groups, cannot be used in our context given the homogeneity of the group studied (childless single individuals aged 20-30). The age discontinuity in social assistance eligibility is, therefore, used as the key source of identification. 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).[10] Also, 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 Y_i^* denoting the propensity to be employed for individual i:

$$Y_i^* = \alpha + \beta.T_i + \gamma.\delta(A_i) + \varepsilon_i. \tag{1}$$

[10] 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.

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, the parametric version of condition 1, the treatment effect η is obtained by estimating the discontinuity in the empirical regression function at the point where the forcing variable switches from 0 to 1.

Coefficients may also be linear functions of a set Z_i of individual characteristics other than age (gender, education), and be written with the subscript i. Because of their weaker attachment to the labor market, HS dropouts may also behave differently from other education groups so that we must differentiate the employment effect for HS dropouts from those with a degree. We refrain from using more detailed education categories for comparability with the next model, as explained further below. The model becomes:

$$Y_i^* = \alpha_i + \beta_i.I(A_i \geq \underline{A}) + \gamma_i.\delta(A_i) + \varepsilon_i \tag{2}$$

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 employment rate and focusing on the 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 tell anything about the effect of a change in the mean wage at 24. We cannot extrapolate further away from the discontinuity, for instance 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

The interpretation of a potential disincentive effect of social assistance in the above RD design coincides with the assumption made in static structural labor models (for instance, van Soest, 1995). That is, 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). Adopting a flexible specification, where preference parameters vary with the alternative j, we can write utility at choice j as:

$$U_{ij} = a_{ij} + b_{ij}.C(w_i H_j; A_i) + g_{ij}.\delta(A_i) + \epsilon_{ij} \tag{3}$$

with disposable income $C(w_i H; A_i)$ (equivalent to consumption in this static framework) and hours worked H_j. The deterministic utility levels are completed by i.i.d. error terms ϵ_{ij}, assumed to follow an extreme value type I (EV-I) distribution and to represent possible observational errors, optimization errors or transitory situations. Coefficients are choice-specific but also vary linearly with several taste-shifters (gender, education) and some coefficients may vary with a normally-distributed random term for unobserved heterogeneity. Notice that this specification of the model is fairly general, notably with the "disutility" of work (or leisure utility) specified through choice-specific terms a_{ij} and $g_{ij}.\delta(A_i)$, i.e. not forced to vary linearly or quadratically with H_j as in standard functional forms (for instance in Blundell et al., 2000).[11] While it is obvious that β_i is identified at the age discontinuity and cannot, itself, vary with age in the reduced form equation (2), we must impose such a simple restriction in the structural equation:

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

That is, the direct effect of age on utility, $\delta(A_i)$, enters the utility function in an (additive) separable way. In a standard labor supply model, this means that one of the usual taste shifters, age, is left out of some 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) through preference specification with the smooth function $\delta(A_i)$, which may reflect how age changes work preferences, fixed costs of work or search costs (these three components are usually not identified from each others, see van Soest et al., 2002) and (ii) through consumption, since age is a determinant of wages w_i and of the tax-benefit function $C(\cdot, A_i)$.

Since the identification stemming from the discontinuity changes only the financial conditions between working and not working, we focus on the participation margin. This is also the choice of Laroque and Salanié (2002) who estimate labor supply on French data and justify this focus by the small variability in work hours in France. More generally, this is also the main margin of adjustment in the short-run (Heckman, 1993).[12] 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 ($a_{i0} = g_{i0} = 0$).[13]

[11] This specification nests the standard quadratic utility function in hours and consumption if we add a consumption squared term. Yet this term is not necessary in our application since we model participation only.

[12] In the short-run, labor market frictions ensure that people cannot adjust their work duration beyond the mere choice to participate or not, cf. Chetty et al. (2009).

[13] Since utility is a cardinal concept, we could also normalize b_{i0} to 1.

Dropping subscript 1 from coefficients a_{i1} and g_{i1}, we thus write the propensity to be employed as:

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

with $\epsilon_i = \epsilon_{1i} - \epsilon_{0i}$. The model is now very similar to the RD model in equation (2), as it contains the same constant and the same smooth function of age $\delta(A_i)$ plus a term capturing the discontinuity. The main difference, however, is the structure put on the latter. The treatment effect, i.e. the age eligibility to RMI, affects individual decisions through financial gains to work, as measured by the distance between disposable income when employed, $C(\widetilde{w}_i H; A_i)$ and disposable income when out of work, $C(0; A_i)$.[14] By focusing on a specific group of the population, i.e. childless singles, we rule 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. The discontinuity is not in a reduced form but accounted for by different levels of income when unemployed, i.e. $C(0; 25) >> C(0; 24)$.

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, 2008). 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., 2008). 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

[14] 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, 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).

model can be estimated by simulated maximum likelihood, integrating conditional choice probability over the distribution of random terms u_i (as explained below, residuals of the wage equation may also be integrated). We use sequences of Halton draws as suggested by Train (2003), which allows us to reduce the number of draws to a tractable level ($r = 10$).

4.3 Wages, Estimation Method and Discussion

Wage Imputation: Estimations. 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. 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) that are associated with preferences, as discussed above. Here, we obtain identification from exogenous variation in social assistance rules. Nonetheless, wage estimation may control for selection bias, as discussed below. 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. This also reduces the concern of using a different datasets for the wage estimation, as long as the second data source provides accurate information on wages. Like Wasmer and Chenin (2011), we rely on the LFS, which contains detailed and robust information on earnings (base salary plus all bonuses and extra time payment and in-kind advantages). While smaller than the Census, the LFS is large enough for credible imputation of wages according to a set of standard determinants. This is done by estimating the wage equation:

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

on the LFS, assuming a normally distributed wage residual ν_i. The same explanatory variables are also available in the Census so that wages can be predicted. They include a smooth function of age $\theta(A_i)$, the set of detailed education categories $EDUC_i$ (whose definition is common to both datasets) and additional controls Z_i (gender). We correct for selection into employment using a Heckman selection model. The inverse Mills ratio λ_i is estimated on the basis of a reduced form employment probability, including 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. Wages are predicted for observations in the Census using estimates of the wage equation and drawing wage residuals in a normal distribution with zero mean and using the empirical variance of ν_i

Wage Imputation: Matching. The normality assumption may be a poor approximation for the specific population under study (childless single aged 20-30). Hence, we also suggest an alternative imputation method based on a matching approach. That is, for each observation in the Census, we pick a wage value randomly in her age-gender-education group in the LFS. Over a large number of draws, this is equivalent to imputing the mean wage in a given cell – similar to the linear estimation procedure – but accounting for the empirical wage distribution rather than imposing normality on ν_i.

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 corresponds to the multinomial logit model, which boils down to a simple logit in this case. However, 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 in the likelihood. In practice, this is done by averaging the conditional probability over a number of draws for u_i and $\widetilde{\nu}_i$, recalculating disposable income each time. We use sequences of Halton draws as suggested by Train (2003), which allows us to reduce the number of draws to a tractable level ($r = 10$). This baseline participation model with integration of wage draws is denoted (P) in the result section.

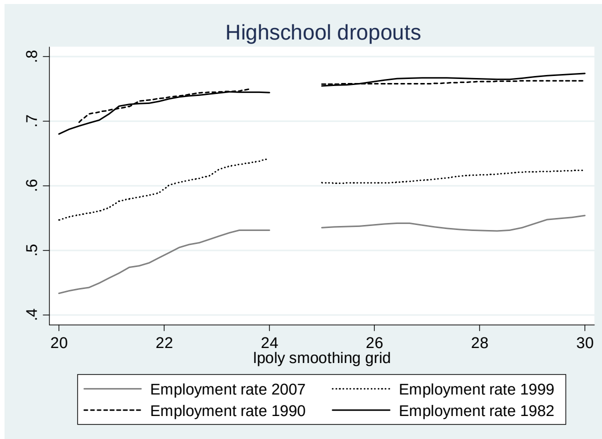
Non-employment and Demand-Side. Non-employment can be rationalized by (i) high preferences for leisure (low u_i) or low financial gains to work (low ν_i); (ii) classic unemployment when productivity is below the minimum wage; (iii) "other" non-employment corresponding to frictional (the person is between two jobs) or cyclical (Keynesian) unemployment. The approach of Laroque and Salanié (2002) to model the supply-side (i) is very similar to ours; yet our treatment of (ii) and (iii) is a bit different. For (iii), they explicitly model a probability for the "other" non-employment, identified using diploma and age as explanatory variables. We make a different parametric modeling choice here, interpreting $a_i + g_i.\delta(A_i)$ as a non-identified combination of supply-side factors (work disutility, work costs, stigma of receiving welfare). The information content is however the same (it depends on gender, age and a HS dropout dummy). The interpretation of involuntary unemployment in a supply-side model as job search costs is also suggested by van Soest et al. (2008), among others. In Bargain and Doorley (2011), we show that the 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, indicating that those with even basic qualifications have more attachment to the labor market, i.e lower job search costs.

Dynamics The RD design in the case of an age-based discontinuity is a special case of the standard RD design (Lee & Lemieux, 2010) as 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 & 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 labour market behavior before the threshold.[15] 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. Firstly, it seems unlikely that the group which displays the largest response to the RMI, highschool 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 labour market at age 25. Secondly, 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 (Bargain & Doorley, 2011) indicating limited anticipation effects in this respect. Thirdly, we do find evidence that the share of highschool dropouts on short-term contracts decreases discontinuously after age 25 (Bargain & Doorley, 2011) 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), 1990 (one year after its introduction) in 1999 and in 2007 shows little evidence of a change in employment trends before the discontinuity (see Figure 1) The overall difference in employment rates from year to year 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 labour market) proportion of the overall population now than they did in the 1980's and 1990's/

[15] To estimate intertemporal labor supply elasticities, Mulligan (1999) exploits the anticipated change in net-of-benefit wage corresponding to the end of eligibility for the AFDC benefit when the youngest child reaches the age of 18. A similar exercise, and the estimation of Frisch elasticities, could be suggested in our case if Census data provided information on wages.

Figure 1: Employment Rates of Childless Singles betwen 1982 and 2007 (kernel-weighted local polynomial smoothing)



5 Results

5.1 Wage Imputation

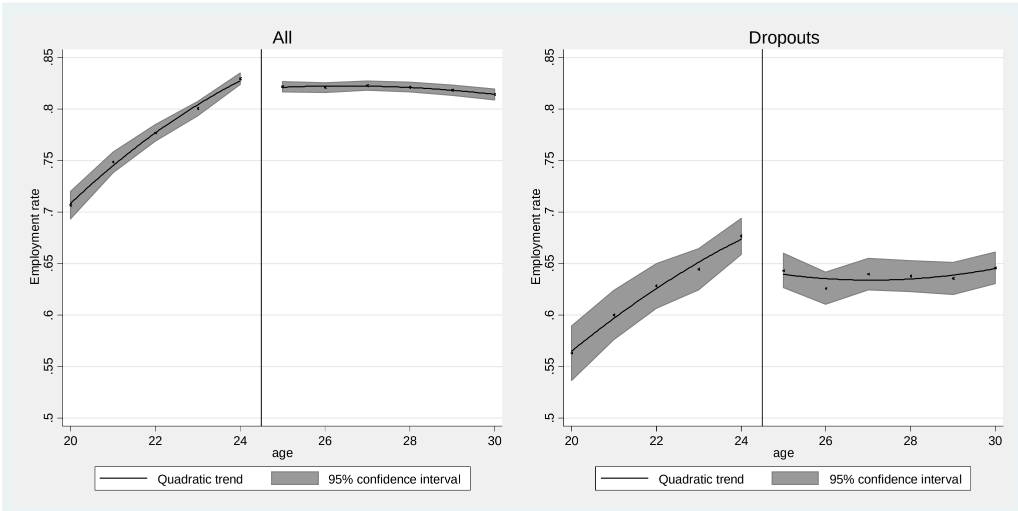
Log hourly wage estimations using the LFS data are reported in Table B.1 in the Appendix. A significant gender gap can be observed, in line with the existence of a "sticky floor" effect in France (Arulampalam et al, 2007) as well as a regular wage progression with the level of education. The Inverse Mills ratio is not significant. Disposable income at 0 hours of work is also insignificant in the first stage of the model (the participation decision) due to the fact that we use the LFS data to model wages. In this survey, the discontinuity does not appear to affect employment, which is certainly due to the erratic employment-age pattern discussed in Section 3. We check the robustness of the estimates in two steps. First, Figure A.1 in the Appendix shows the actual distribution of wages in the LFS, as well as the predicted distributions for workers only and for all (workers and non-workers). The vertical line shows the level of the minimum wage and, unsurprisingly, there is a large spike in log wages directly above the minimum wage level. Next, Figures A.2-A.4 compare the predicted distribution of wages for workers only and for all potential workers in the LFS and the Census (for all, men and women separately). As we constrain workers to earn at least the minimum wage, it is only in the distribution of wages "for all" that we see observations with less than the minimum wage. Reassuringly, the predicted wage distributions in the LFS and the Census resemble each other quite closely. Moving from wages to disposable incomes, we have seen in Table 1 that disposable incomes – calculated using tax-benefit simulation, actual incomes (in the LFS) and work duration plus predicted wages (in the Census) – line up quite closely in the two datasets.

5.2 Estimations and Comparisons

Model Estimates We first present a graphical representation of the RMI effect. In Figure 2, we plot raw employment rates by age, along with 95% confidence intervals using our selected sample from the 1999 Census. We distinguish between the full sample and the sub-group of HS dropouts. The graphical representation of this discontinuity suggests that employment drops sharply in the latter group at age 25, by around 4 percentage points (ppt). In Bargain and Doorley (2011), we suggest several robustness checks for this result. In particular, we check that no other policy or institutional features could be the cause for a discontinuous drop in employment at that particular age. We also compare this result to the changes in employment at age 25 for a number of control groups not affected by the discontinuity (uneducated workers prior to the introduction of RMI, uneducated workers with children and, hence, not affected by the age condition, etc.), for whom we find no significant employment change. In contrast, the employment effect of the total sample is relatively modest.
For $\delta(\cdot)$, we present results with a cubic form, which is flexible enough for our purpose. We have used a variety of polynomial forms, including standard linear, quadratic, cubic, linear and quadratic splines (separate regressions on both sides of the discontinuity). In Bargain and Doorley (2011), we show that results do not change much with the specification. Equally important 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 vary in a linear spline model for an increasingly small window around age 25. We find very stable estimates, which is additionally confirmed by non-parametric estimations with varying bandwiths (see Bargain and Doorley, 2011)

Table B.2 in the Appendix shows the estimates of the RD model and of the participation model P. Coefficients for models P0, P25 and P50, which account for different degrees of correlation between wages and unobserved preference heterogeneity, are also presented (as well as model D, which will be discussed in the robustness section). 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 models P - P50, 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,

Figure 2: Employment Rate of Childless Singles and Discontinuity



(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. The second observation is that the effect of income at zero hours is relatively constant across models. This explains the results we find in the next section, that model predictions do not vary much despite very different assumptions on the degree of endogeneity. Finally, the effect of income at full-time work declines with the level of correlation between wages and preferences for work. This points to the fact that in the case of extreme endogeneity between wage and preferences, participation becomes much less responsive to financial incentives due to in-work income (wage prospects but also taxes, tax credits, etc.). Models ignoring this heterogeneity must considerably overstate the effect of policies that affect in-work income (for instance EITC-type of reforms). This is crucial, given the current trend in in-work transfers and, notably, the 2009 reform in France which has extended the RMI to the working poor (see Bargain and Vicard, 2012). It means that using participation models, even identified on exogenous variations like policy discontinuities, would lead to hazardous predictions of the effect of policies affecting in-work income rather than out-of-work income.

RD and Structural Model: Comparisons. The first columns of Table 2 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 toward 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 uneducated singles. The effect is largely similar for women and men within specifications. To estimate the percentage decrease in employment, we divide the treatment effect by the employment rate at age 24 and find that employment decreased by 6% among highschool dropouts at age 25.

Turning to the baseline participation model (model P), we find slightly more homogenous results across gender groups, in contrast to the RD estimates.[16] The overall effect, however, is in line with the RD results: -1.5 and -3.9 ppt for the whole selected sample and for HS dropouts respectively. These effects are not significantly different from those of the RD approach. The treatment effects for models accounting for unobserved heterogeneity, as discussed in section 4 (models P0, P25 and P50) are very similar to those obtained using model P (results available from authors upon request). An alternative specification of the smooth function of age (quadratic) does not affect these conclusions qualitatively, and quantitative differences are relatively small (see Table 4).

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

	Actual Participation Rates			Predicted Participation Rates (Model P)			Treatment Effect			Treatment Effect		
	Age 24	Age 25	Diff.	Age 24	Age 25	Diff.	**RD**	s.e.		**Model P**	s.e.	
All education groups												
All	82.9%	82.2%	-0.7%	81.8%	81.2%	-0.6%	**-1.6%**	0.004	***	**-1.5%**	0.002	***
Male	83.4%	83.3%	-0.1%	82.8%	82.0%	-0.8%	**-0.7%**	0.006		**-1.7%**	0.003	***
Female	82.4%	80.8%	-1.6%	80.6%	80.2%	-0.4%	**-2.5%**	0.007	***	**-1.3%**	0.003	***
HS Dropouts												
All	67.7%	64.3%	-3.4%	65.9%	62.6%	-3.2%	**-3.9%**	0.014	***	**-3.9%**	0.002	***
Male	70.5%	66.5%	-4.0%	68.0%	64.5%	-3.5%	**-4.2%**	0.018	**	**-4.1%**	0.002	***
Female	63.1%	60.8%	-2.3%	62.3%	59.6%	-2.8%	**-3.4%**	0.024		**-3.6%**	0.003	***

Model P is a participation model with a cubic age specification estimated by simulated ML with conditional probabilities averaged over

Out-of-sample Prediction Ideally we would like to check the external validity of the models and, more precisely, the identifying role of the discontinuity in a year when the RMI was not in place. The RMI was introduced in 1989, ten years before the year of the data we use. Unfortunately, the closest pre-reform year of census data is 1982, which is

[16] How the policy effect at threshold is measured is explained in Appendix A

too old to be used for this purpose. Therefore, we rely on a cross-validation sample to provide a first check of the external validity of the structural model. The advantage of such a strategy, 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). Here we rely on two sub-samples for the same year of data (1999). We estimate our base model P on the first subsample (estimation sample), i.e. a random half of the selected sample, and use estimates to predict employment rates at all ages, as well as the treatment effect, on the other half (the holdout sample).

Table 3: Employment Effects of the RMI: using Cross-validation Samples

	Actual Participation Rates			Treatment Effect		Predicted Participation Rates			Treatment Effects	
	Age 24	Age 25	Diff.	RD	s.e.	Age 24	Age 25	Diff.	Model P	s.e.
All education groups										
All	82.8%	82.3%	-0.5%	**-1.1%**	0.006 *	81.8%	81.0%	-0.8%	**-2.0%**	0.003 ***
Male	83.6%	83.8%	0.2%	**-1.7%**	0.007 **	82.6%	81.7%	-0.9%	**-2.1%**	0.005 ***
Female	80.9%	80.2%	-0.7%	**-0.4%**	0.007	80.9%	80.2%	-0.7%	**-1.8%**	0.005 ***
HS Dropouts										
All	66.8%	64.1%	-2.7%	**-3.5%**	0.016 **	65.7%	62.6%	-3.1%	**-4.1%**	0.003 ***
Male	70.9%	65.9%	-4.9%	**-4.1%**	0.017 **	67.3%	64.0%	-3.3%	**-4.2%**	0.003 ***
Female	59.9%	61.2%	1.3%	**-2.5%**	0.018	63.0%	60.3%	-2.7%	**-3.9%**	0.004 ***

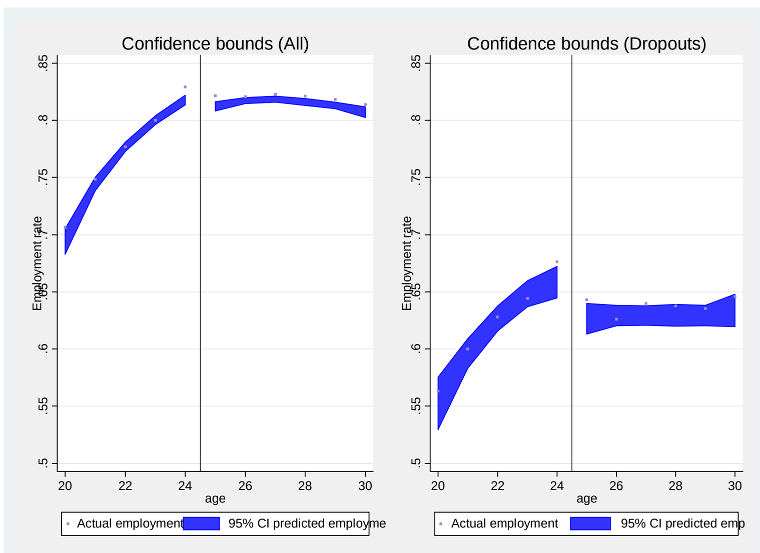
Continuous function of age: cubic. Participation model estimated by simulated ML with conditional probabilities averaged over ten wage draws (Model P) using estimation sample (1/2 half of Census selection); all figures above are actual and predicted on holdout sample (the other

Results are reported in Table 3. 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 at -2.0 and -4.1 for the whole selection and 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. 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.

First Check: Predicting Employment Rates 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 at all ages as well as predicted employment rates in the baseline situation (using model P, with a cubic function of age), 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. This gives us confidence in the extrapolation we perform next, based on this structural model

As discussed in Section 4.3, we also experiment with an alternative wage imputation which simply matches individuals in the census with a wage from their age-gender-education category. The employment rates predicted using this alternative wage imputation are shown in Figure 8 in Appendix B and correspond closely, both to the actual employment rates and to the predicted employment rates from figure 3.

Figure 3: Employment Rate of Single Childless Individuals



Second Check: Predicting the Effect of the 2009 reform As a second check, we compare the results of (i) our RD and structural model estimates of the discontinuity in 1999 and (ii) our structural model predictions of the discontinuity in 2009 with previous results from Bargain and Vicard (2012). The results from Bargain and Vicard (2012) come from a basic Difference-in-Difference between $Y(24)-Y(25)$ in 2009-10 and $Y(24)-Y(25)$ in 2005-08. As such, this is a relatively informal comparison since the results do not take account of the crisis or potential changes in nature of the census data. Nevertheless, the two sets of results are comparable. Our preferred specification (cubic age polynomial) indicates a $4.7ppt$ decrease in the disincentive effect of the RMI for HS dropouts upon the introduction of the in-work-benefit RSA component. Comparing the disincentive effect found using RD in 2004-08 to 2010-11, Bargain and Vicard (2012) find a 4.5*ppt* decrease in this disincentive effect.

Table 4: Comparing the Employment Effects of the RMI and RSA using RD and Counterfactual Simulation

Age specification	1999 RMI effect (RD)	1999 RMI effect (simulation)	1999 RSA effect (simulation)	**Difference**		2004-08 RMI effect (RD)	2010-2011 RSA effect (RD)	**Difference**
	(1)	(2)	(3)	**(3) - (2)**		(4)	(5)	**(5) - (4)**
Quadratic	-0.058 ***	-0.055 ***	-0.008 ***	**0.047** ***	0.000	-0.034 **	-0.014	**0.020**
	(0.011)	(0.002)	(0.002)	**(0.003)**	(0.000)	(0.015)	(0.016)	**(0.021)**
Cubic	-0.039 ***	-0.039 ***	0.007 ***	**0.047** ***	0.000	-0.035 ***	0.009	**0.045** **
	(0.014)	(0.002)	(0.002)	**(0.003)**	(0.000)	(0.009)	(0.018)	**(0.020)**

*Selection: childless single individuals aged 20-30, HS dropouts. Note: significance level at 1%, 5%, 10% indicated by ***, ** and **

5.3 Counterfactual Simulations

Abolishing RMI Our first simulation examines the effect of abolishing the RMI (as defined at the end of section 3). Interestingly, Figure 4 shows that abolishing the RMI would increase participation just over the 25-year-old threshold but the response fades away with higher age levels. This is consistent with the fact that wage prospects increase with age so that inactivity traps are less pronounced at older age groups.

Extending the RMI 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 (twice as large as that of the 25-30 years-old, i.e., almost 11% when the poverty line is half the median income). 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.

Figures 5 and 6 simulate (i) abolishing the age condition, which corresponds to a reform extending the RMI to those aged 20-25 ($C(0,A)$ replaced by $C^1(0,A)$) and (ii) simulating the 2009 reform of the RMI which essentially reduced the withdrawal rate from 100% to 32%, introducing an in-work-benefit component. This new minimum income is called the

Figure 4: Counterfactual employment Rate of Single Childless Individuals: Abolish RMI

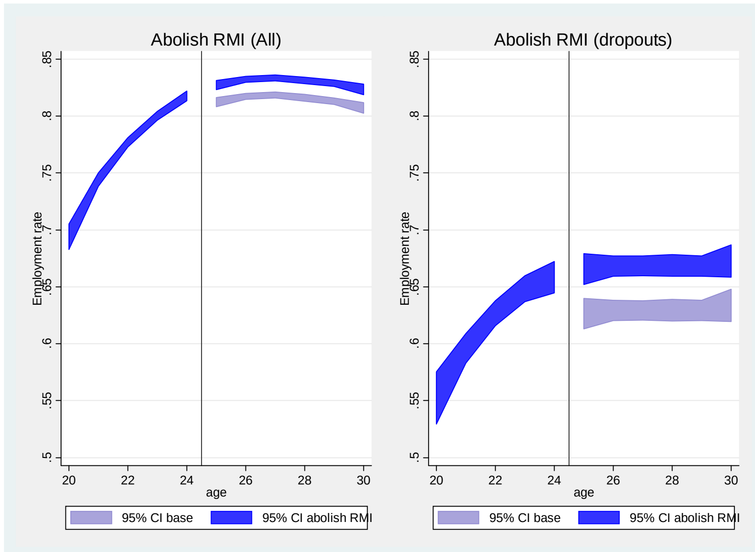
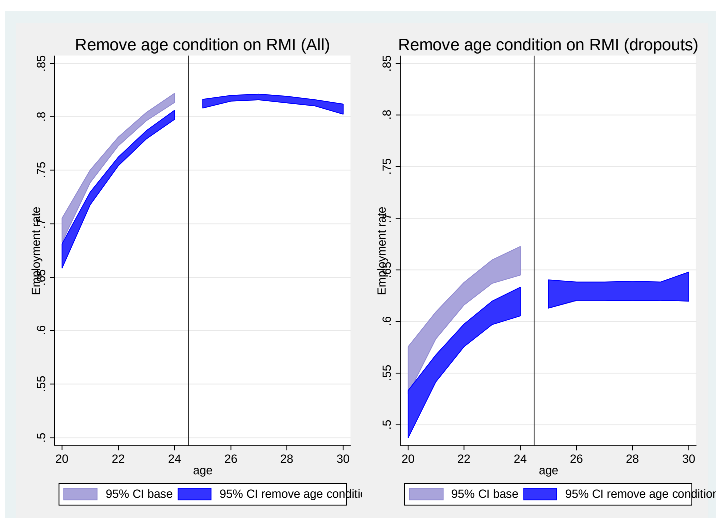
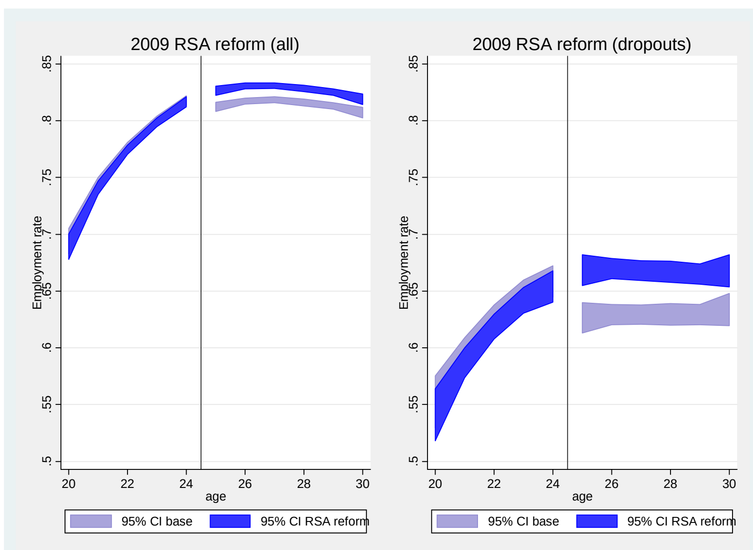


Figure 5: Counterfactual employment Rate of Single Childless Individuals: Extend RMI



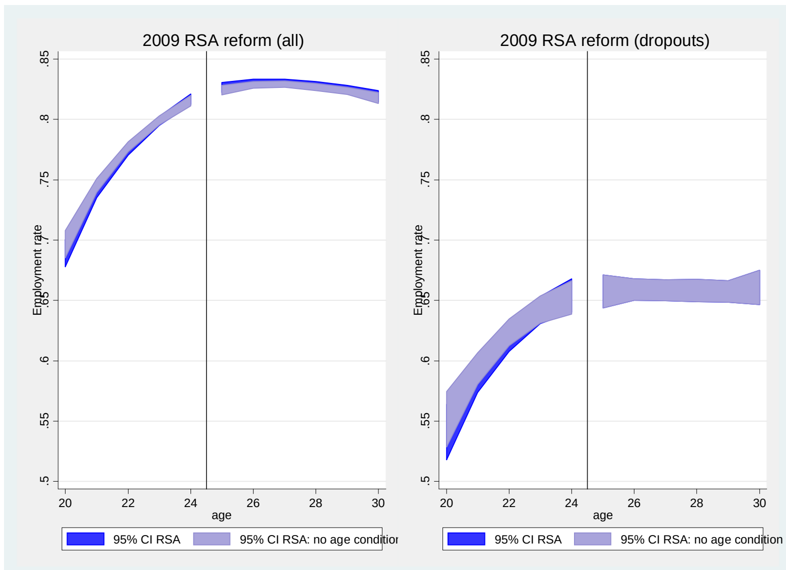
Revenu de Solidarite Active (RSA). While these hypothetical reforms have little effect on the whole sample, the HS dropouts show a response to both reforms. Introducing the RMI for those under 25 induces a drop in participation of 5 percentage points for those under 25 years of age. Symmetrically to 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 the RMI to this group. The simulation of the RSA reform has 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 effect on employment rates of about $3ppt$, which fades towards age 30. The change in the disincentive effect, due to the RSA, is almost as large as the effect of abolishing the RMI altogether, as seen in Figure 4

Figure 6: Counterfactual employment Rate of Single Childless Individuals: Replace RMI with RSA



Extending the RSA to the Youth One further simulation, which is relevant given the current policy debate in France (see Bargain and Vicard, 2012 and Cahuc et al., 2008), examines the effect of extending the RSA to the under-25's. The results, in figure 6, show that extending the RSA to the under-25's would not have a significant employment effect, either for the whole population or for the more vulnerable high-school dropouts. This is because, although potential out-of-work income doubles for the under-25 population, potential in-work income also increases for some low-earners, due to the withdrawal rate of 38%. Responses to in-work income are stronger than responses to out-of-work income, euro for euro (see table B.2 in the Appendix), making the overall employment effect ambiguous. This is in stark contrast to the extension of the RMI to the under-25 population, depicted in Figure 5. As the RMI lacks any in-work incentives, the employment effect is negative and becomes large for the population of HS dropouts.

Figure 7: Counterfactual employment Rate of Single Childless Individuals: Extend TSA



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 extrapolations. The model reproduces the participation drop at age 25 and also predicts employment levels at other age levels satisfactorily. It allows the simulation of counterfactual policies and, notably, the extension of the scheme to the young, pointing to significant disincentive effects at all ages between 20 and 25. 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, restore financial incentives to work and alleviate 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, 2009). In this respect it is, therefore, the best ground for reconciling structural models and natural experiments as we do here. Note, however, that the general labor supply model presented above could be identified and estimated using additional sources of exogenous variation, e.g., other discontinuities affecting the financial gans 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 external validity of our structural model should be tested, 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 level 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

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

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 counterfactual situation C^0 (no RMI):

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

The policy effect at the cutoff is therefore:

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

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 Comparing Datasets, Model Estimates and Wage Estimations

Table B.1: Wage Estimation on LFS Data

Variables	Log wage		Employment	
Age	-0.048	-0.023	0.079	-0.055
Age square / 100	0.001	0.000	-0.002	-0.001
Female	-0.112	-0.007	0.042	-0.022
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	

Table B.2: Estimates: RD and Participation Models

	RD		Model P		Model P0		Model P25		Model P50		Model D	
	Coefficient	s.e.	Coefficient	s.e.	Coefficient	s.e.	Coefficient	s.e.	Coefficient	s.e.	Coefficient	s.e.
Preference for work												
Age	0.721	0.221	2.837	1.217	2.893	1.332	2.875	1.355	3.047	1.350	-1.459	1.022
Age2	-0.027	0.009	-0.104	0.049	-0.105	0.054	-0.104	0.055	-0.111	0.055	0.075	0.041
Age3	0.000	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001	-0.001	0.001
Age*educated	-0.366	0.244	-1.206	1.421	-1.070	1.545	-1.097	1.566	-1.195	1.560	-0.181	1.200
Age2*educated	0.015	0.010	0.051	0.057	0.046	0.062	0.048	0.063	0.052	0.063	0.008	0.048
Age3*educated	0.000	0.000	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001	0.000	0.001
Male	0.061	0.005	0.557	0.059	0.664	0.063	0.799	0.062	0.805	0.061	0.275	0.033
Male*educated	-0.031	0.005	0.043	0.029	0.038	0.032	0.150	0.032	0.130	0.032	0.016	0.029
Educated	3.228	1.994	10.869	11.635	9.836	12.652	10.318	12.821	10.866	12.777	2.565	9.941
Constant	-5.857	1.805	-27.041	9.953	-28.051	10.892	-28.181	11.079	-29.195	11.040	7.297	8.447
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	-0.036	0.024	-0.036	0.024	-0.032	0.024		
Male			0.011	0.008	0.013	0.009	0.003	0.009	0.003	0.009		
Constant			0.047	0.020	0.062	0.022	0.066	0.022	0.062	0.022		
Coefficients on Income when H=39 hours/week (divided by 100)												
Educated			-0.070	0.007	-0.083	0.007	-0.142	0.007	-0.123	0.007		
Male			-0.039	0.005	-0.046	0.005	-0.071	0.005	-0.069	0.005		
Constant			0.214	0.007	0.247	0.008	0.309	0.007	0.264	0.007		
Coefficients on [Income (H=39) - Income (H=0)] (divided by 100)												
Educated											-0.060	0.006
Male											-0.027	0.004
Constant											0.192	0.007
Log Likelihood			-91701		-105129		-105025		-105776		-91778	
prob > chi2			0		0		0		0		0	
Observations	202093		202093		202093		202093		202093		202093	

Model P is a participation model estimated by simulated ML with conditional probabilities averaged over ten wage draws. Model P0, P25 and P50 additionally include unobserved heterogeneity assumed to be potentially correlated with wage error terms; the correlation is 0, 0.25 and 0.50 respectively. Model D uses the difference between disposable income at 39 and 0 hours to model participation

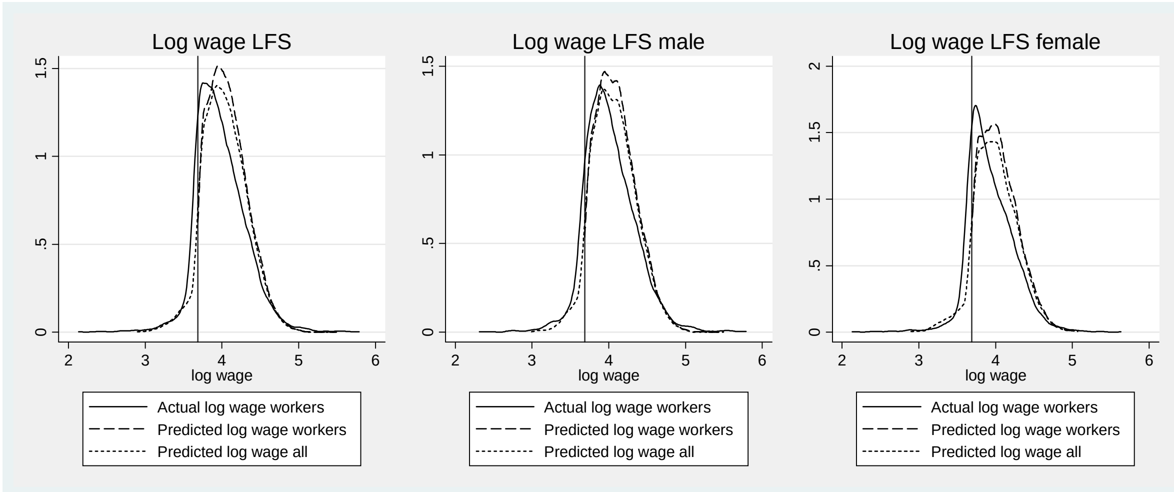

Figure A.1: Predicted and Actual Log Wage Distributions in LFS

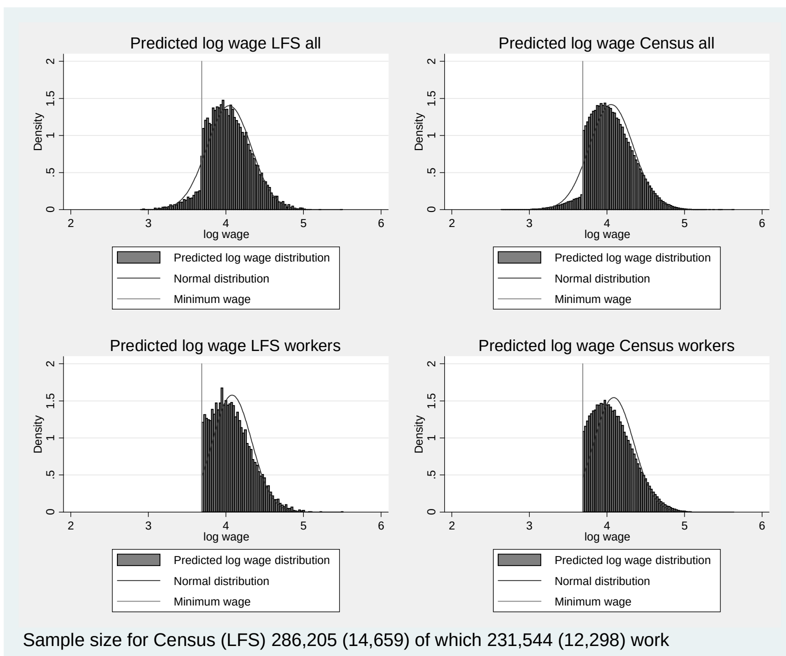

Figure A.2: Comparing Predicted Log Wage Distributions in LFS and Census Data (All)

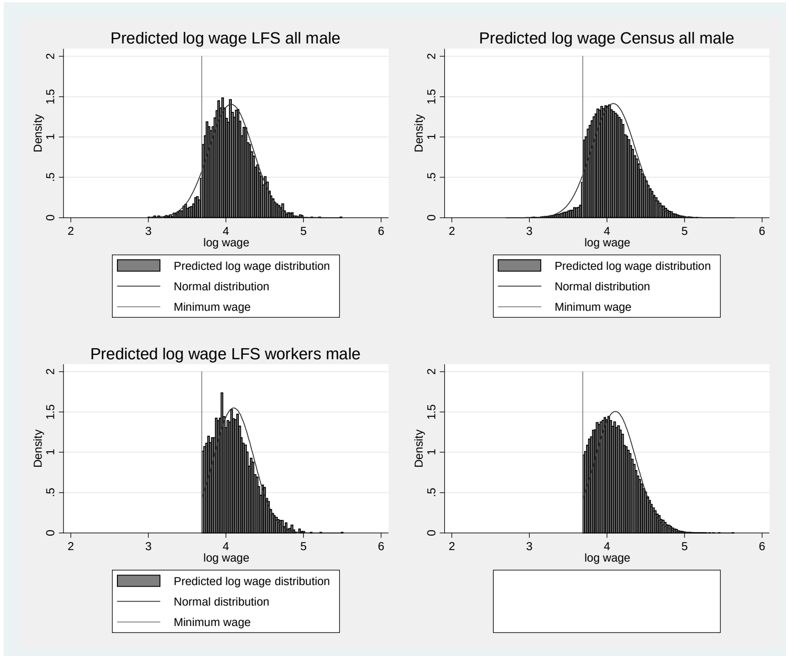

Figure A.3: Comparing Predicted Log Wage Distributions in LFS and Census Data (Men)

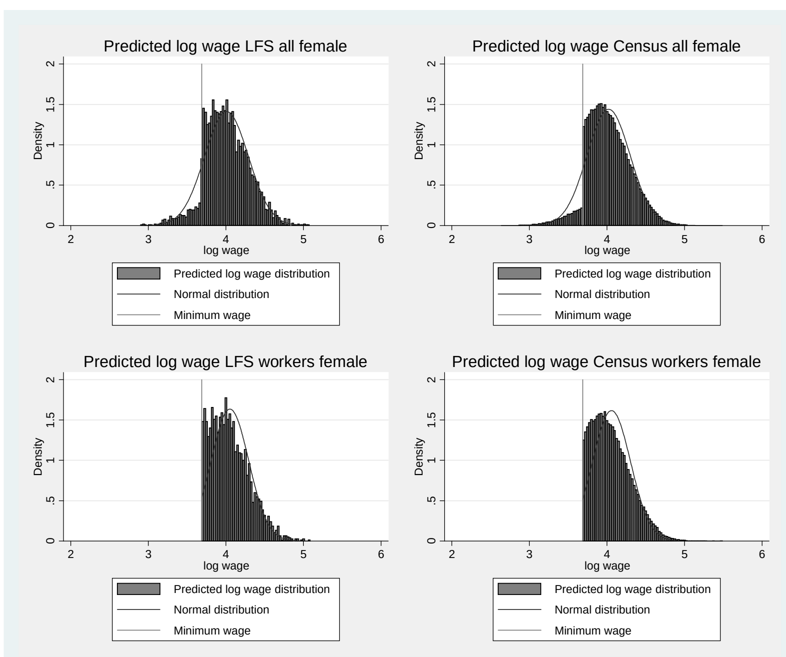

Figure A.4: Comparing Predicted Log Wage Distributions in LFS and Census Data (Women)

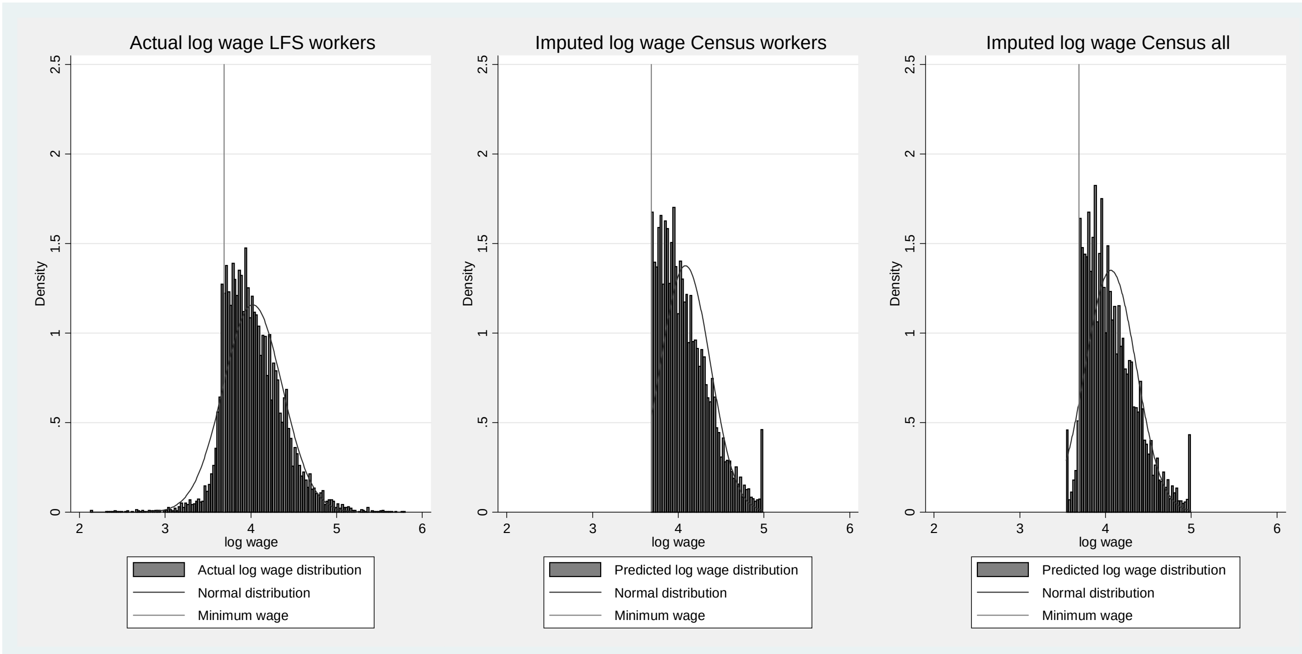

Figure A.5: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (All - matched)

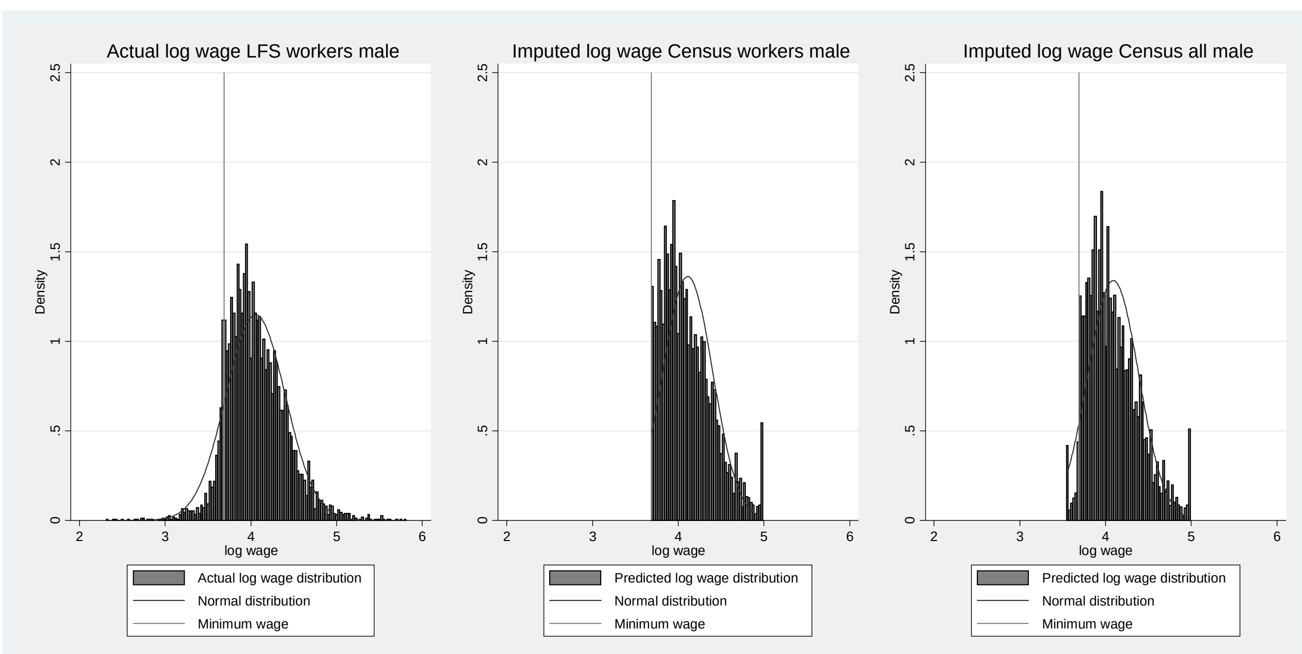

Figure A.6: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (Men - matched)

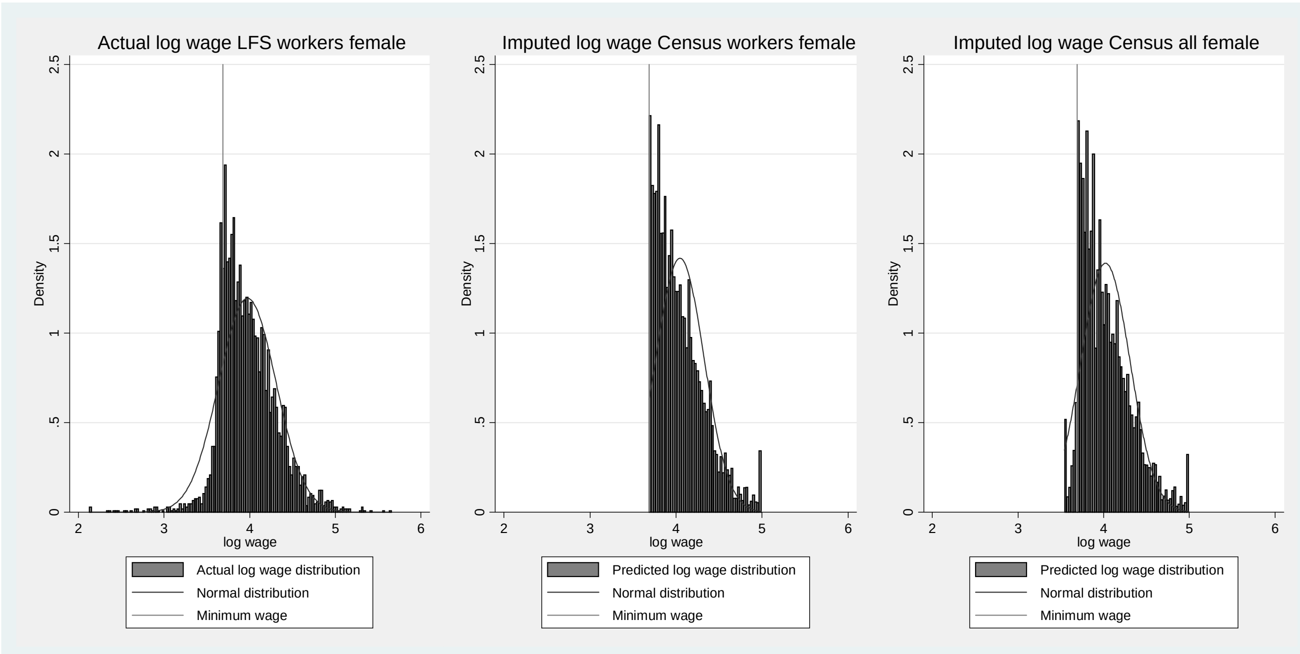

Figure A.7: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (Women - matched)

C Results with Matching Wage Imputation

The wage estimation outlined in section 4.3 appears to give reasonable estimates of the wage distribution in the LFS and, correspondingly reasonable predictions of the wage distribution in the census data (see figures $A1$ to $A4$). However, the insignificance of the age-25 dummy in the first stage of the selection model with the LFS data may lead to erroneous estimates. For this reason, we suggest an alternative, simpler, wage estimation strategy and compare the results of the two methods.

To re-estimate wages in the census data, we define mean wages in the LFS data by detailed characteristic category (age, sex, education). For example, we define a mean wage for all 20 year old female dropouts, for all 20 year old male dropouts, etc. The empirical variance of wages by detailed category is retrieved from the wage distribution in the LFS and used to impute a random component $\widetilde{\nu}_i$, to be added to each mean wage imputed in the Census. Once again, we discard draws that lead to $\widetilde{w}_i < MW$ for those who are observed working in the Census, while those who do not work can earn any wage in the random distribution of wages. The estimated distribution of wages in the LFS and the census using this methodology is depicted in figures $A5$ to $A7$. The imputed distributions are more concentrated in the census data than the actual distribution in the LFS due to the fact that we simply draw means by category.

Next, we compare the baseline results using the wage estimation technique to the baseline results using this new wage imputation technique. Figure 8 shows that this model predicts employment levels well, as does the model using the former wage estimation technique (see Figure 3). Table C.1 shows that the participation models' estimates of the employment drop is the same for the whole selection of men and women ($-1.5ppt$), regardless of the wage imputation method used. For the group of highschool dropouts, the estimations are very close at $-3.4ppt$ using matched wages compared to $-3.9ppt$ with the former wage estimation technique. Both are comparable to the RD estimation of $-3.9ppt$ for HS dropouts.

Figure 8: Employment Rate of Single Childless Individuals: Actual vs. Simulated with matched wages

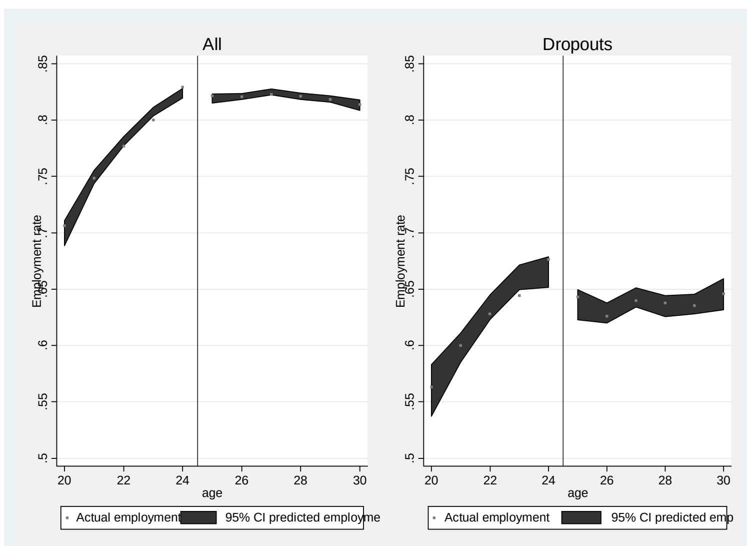

Table C.1: Employment Effects of the RMI: RD vs. Structural Model with alternative "matched" wage draws

	Actual Participation Rates			Treatment Effect			Treatment Effects		
	Age 24	Age 25	Difference	**RD**	s.e.		**Model P**	s.e.	
All education groups									
All	82.9%	82.2%	-0.7%	**-1.6%**	0.00	***	**-1.5%**	0.002	***
Male	83.4%	83.3%	-0.1%	**-0.7%**	0.01		**-1.6%**	0.003	***
Female	82.4%	80.8%	-1.6%	**-2.5%**	0.01	***	**-1.5%**	0.003	***
HS Dropouts									
All	67.7%	64.3%	-3.4%	**-3.9%**	0.01	***	**-3.4%**	0.002	***
Male	70.5%	66.5%	-4.0%	**-4.2%**	0.02	**	**-3.4%**	0.002	***
Female	63.1%	60.8%	-2.3%	**-3.4%**	0.02		**-3.4%**	0.002	***

Model P is a participation model estimated by simulated ML with conditional probabilities averaged over