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

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Abstract

Structural models, e.g. labor supply models, make explicit the behavioral assumptions underlying ex post evaluations and are useful for policy makers since they allow predicting the effects of hypothetical or future policies. However, their identification is often put into question. In this study, we focus on the identification of a structural model stemming from a natural experiment or policy feature (discontinuity). We exploit the fact that childless single individuals under 25 years of age are not eligible for social assistance in France. The negative employment effect expected at age 25 is measured by a regression discontinuity (RD) and, alternatively, by adding structure to this model using simple behavioral assumptions. We check the validity of this model and investigate the role of the discontinuity in the identification of preferences. The model is used to predict important counterfactual policies including the extension of social assistance to young people in France and the role of in-work benefit components.

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

The economic literature rarely reconciles the approach based on randomized or natural experiments for (ex post) policy evaluation with that relying on structural, behavioral models (ex ante evaluation). Causal inference of actual policy effects preferably relies on the former approach. Indeed, structural models are suspected to rely on weak identification strategies. These models are nonetheless useful for at least two reasons. First, experiments and natural experiments are themselves designed and interpreted according to implicit behavioral assumptions, which are in fact made explicit in structural models and can be tested. Second, structural models allow ex ante analysis of future or hypothetical policies and, hence, are extremely useful for policy recommendations.

One of the most prominent examples is the case of tax and benefit policies, and how they affect labor supply choices. In recent years, a very large number of policy studies have relied on cross-sectional 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; Bargain et al., 2013). Despite the popularity of these models for policy analysis, the validity of their predictions to policy changes is not 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 are used to account for the effect of the complete tax-benefit system on individual budget constraints. Cross-sectional identification thus relies on exogenous variation in tax-benefit rules across regions (for instance, across US states in Hoynes, 1996) or simply on the nonlinearities and discontinuities in tax-benefit rules together with variation in demographic characteristics of the sample.[1] With panel data or repeated cross-sections, truly exogenous variation can be obtained from changes in net wages over time due to tax-benefit reforms (Blundell et al., 1998), bringing structural modeling closer to natural experiments.[2]

[1] That is, 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 and non labor income 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 the present 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.

[2] Similar identification strategies relying on tax reforms over time are used in the more reduced-form approach consisting in the estimation of the elasticity of taxable income (see Saez et al., 2012, for an

In this study, we combine the two approaches in a static framework, focusing on the labor supply effect of tax-benefit policies. Precisely, we compare an estimation drawn from a regression discontinuity (RD) design to the prediction of a structural model obtained on the same French data. We exploit the fact that childless single individuals under 25 years of age are not eligible for the main social assistance program in France (the *Revenue Minimum d'Insertion*, RMI). First, we draw from former results that the negative employment effect expected at age 25 is around 7% for the group of uneducated childless singles as measured by RD (Bargain and Doorley, 2011). Then, we add structure to the RD model using simple behavioral assumptions, i.e. optimizing agents making labor supply decisions based on disposable income in a static framework. We check the external validity of this structural model. 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. We focus on the same identification source as in the RD design, i.e. the age discontinuity in benefit rules. In this way we can isolate the role of truly exogenous variation in the identification of labor supply models and the characterization of underlying preferences. The behavioral model allows us to predict important counterfactual policies, notably the extension of social assistance to young people in France or the introduction of an in-work benefit component that would also concern the youth. These types of policy reforms are at the core of the political debate in France (Cahuc et al., 2008). Indeed, youth unemployment is very high in this country and is suspected to have dramatic consequences, including very high poverty rates among the young and possibly some effect on crime (Fougère et al., 2009). While the RD design can provide estimates of the RMI effect only around the discontinuity, structural modeling is being used to predict whether extending social assistance to the youth may actually aggravate youth inactivity.

The paper is structured as follows. Section 2 provides a brief review of the structural and reduced form approches to estimate labor supply in the presence of taxation. Section 3 presents the data used and section 4 explains the empirical strategy in detail. Section 5 reports and analyzes the results while section 6 concludes.

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overview).

2 Comparing Methods: an Overview

2.1 Structural Models and Natural Experiments

Many discrete choice labor supply models, which account for the full tax and benefit system affecting household budget constraints, have been used in the literature (see for instance Aaberge et al., 1995, van Soest, 1995, Hoynes, 1996 or Blundell et al., 2000, Heim and Meyer, 2003). The discrete choice approach solves several problems encountered with the Hausman method, which explains its relative success over the years. First, discrete choice models require the explicit parameterization of consumption-leisure preferences as they assume that labor supply decisions can be reduced to choosing among a discrete set of possibilities (e.g., inactivity, part-time and full-time). Thus, there is no need to restrict preferences and, in particular, to impose their convexity. Second, consumption (disposable income) needs to be assessed only at certain points of the budget curve so that complex tax-benefit systems, that generate nonlinear and possibly discontinuous budget constraints and nonconvex budget sets, can easily be dealt with. Accounting for tax-benefit rules in a comprehensive way is important since most of the identification in these models relies on such nonlinearities, as discussed in the introduction. Third, discrete-choice models directly account for both participation and working-time decisions (non-participation is just one of the discrete options). This is important, as most labor supply adjustments occur along this margin (Heckman, 1993). In the present paper, we actually focus on the participation margin, as in Laroque and Salanié (2002), which is the essential margin affected by the discontinuity under study.

In parallel, and 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 behavioral parameters. 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. Using a 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. Much less evidence is available for continental Europe and, in particular, for France. Due to the lack of major tax-benefit reforms in this country, most of the evidence comes from estimates of structural models.[3] An exception

[3]These studies make use of the Hausman model with convexified budget sets (Blundell and Laisney, 1988; Bourguignon and Magnac, 1991) or discrete choice modeling (Laroque and Salanié, 2002; Choné

is the study of Wasmer and Chemin (2012), who 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. Another exception is the use of a policy feature as in Lemieux and Milligan (2008) in the French context. Extensively analyzed 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 RMI. Under 25 and when out of work, this group could only avail of housing benefits. Out-of-work payment would then increase by 160% as they turned 25 years old and became eligible for the RMI (1999 figures). 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 (notably, 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 transfer to the under 25, as motivated in the introduction.

2.2 The Limited Literature Comparing Methods

Using methodologies such as RD in the case of natural experiments 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 can possibly generate similar discontinuities.

A systematical comparison of natural experiments and structural modeling is not evident in the literature. A few studies, nonetheless, compare specific tax-benefit policy events using both natural experiment methodologies and structural modeling. Some studies report relatively encouraging results concerning the out-of-sample predictions of policy changes

et al., 2004; Gurgand and Margolis, 2008). Only a few papers have used tax-benefit changes to evaluate the responsiveness of the labor force (a small tax credit in Stancanelli, 2008, time change in income tax schedule in Carbonnier, 2008, rules allowing to cumulate welfare payment for lone mothers and earnings in González, 2008, and age condition on children for a replacement income targeted at low-income mothers who opt for full-time childcare, in Piketty, 1998).

using a structural model compared to difference-in-difference estimates. For the UK, Blundell (2006) focuses on the effects of the Earned Income Tax Credit on lone mothers' working decisions. Pronzato (2008) compares the effect of the 1998 Norwegian welfare reform on lone mothers' earnings. Cai et al (2007) analyse the work incentive effects of a change in the Australian tax and transfer system on lone parents (predictions from behavioral microsimulation are compared to evaluation based on matching). Geyer et al. (2012) estimate an intertemporal structural model of labor supply for mothers with young children and compare predictions to a parental leave reform with an ex post evaluation. Thoresen et al. (2011) assess the effects of the substantial reductions of marginal tax rates according to the Norwegian tax reform of 2006. Closer to our own contribution, Hansen and Liu (2011) exploit the policy features analyzed in the aforementioned RD design of Lemieux and Milligan (2008). They first estimate a discrete choice labor supply model on a sub-sample of the 1986 Census (i.e. 3 years before the end of the age discontinuity) and use the estimated preference parameters to predict employment and welfare participation in the case where the discontinuity is abolished. They then compare the estimated impacts on these outcomes with those obtained using the RD approach. All these studies find close correspondence between the predictions of structural models and the treatment effects obtained by quasi-experiments. Yet other studies are more skeptical. In the US, Choi (2010) studies the labor supply effects of two pre-PRWORA state welfare reform experiments during the mid 1990s. She concludes that her structural model fits the estimation sample very well but is unable to replicate the experimental treatment effects of each reform. Keane and Wolpin (2007) estimate a dynamic structural model of female behavior in the US, in which work, welfare participation, marriage and fertility decisions are jointly considered. They check the validity of the model using a "holdout" sample and find that multinomial logits perform badly in this setting while dynamic programming models perform better. Overall, however, these comparaisons may bear the risk to treat one or the other approach in a biased way. Our approach does not follow this path and, rather than a mere comparison of structural models versus RD designs, consists in exploring the potential of structural models identified on the discontinuity.

3 Institutional Background, Data and Selection

The policy we study, the RMI, acts as a 'last resort' benefit for those who are ineligible for (or have exhausted their right to) other benefits in France. 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 can represent up to a third of the total transfer to those living purely on welfare.

RMI recipients are also entitled to additional benefits, including a full exemption from the local residence tax, access to free universal healthcare insurance and lower fares on public transport. In practice, entitlement to RMI does not include any obligation to actively seek work and is time unlimited. 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 becomes 100%.[4] 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, and Bargain and Doorley, 2011).

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 was universal and samples of 1/20 or 1/4 of the population are publicly available from INSEE. To be able to create cells large enough for robust analysis, we opt for the 1/4 of the population data, which corresponds 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, past year employment and receipt/amount of RMI or other benefits is unfortunately not available. For this reason, wage estimations are conducted using the French *Labor Force Survey* (LFS). This panel survey is conducted on an annual basis for the periods 1982-1989 and 1990-2002 by the French Statistical Office (INSEE). 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. To obtain a large enough sample, we select LFS datasets for years 1997-2001; additionally, we check the wage profiles when decreasing the sample size by just using the 1999 dataset.

The selection is applied to both Census and LFS data. We retain individuals aged 20-35 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. They also allow for clearer interpretations of the potential labor supply effects, in contrast to individuals in couples.[5]

[4] In the year under investigation, 1999, the benefit reduction rate is 50% for the first 750 hours worked after resuming activity. This corresponds to around four and a half months of full-time work, which makes these conditions close to the pre-1996 AFDC program in the US (benefit reduction rate of 67% for the first four months, then 100%). A difference is that the AFDC included a disregard of $90 per month.

[5] The partner may already work; the discontinuity concerns the age of the older spouse; joint labor supply decisions in couples is a relatively complicated problem.

The selection of individuals without children is obviously due to the fact that a parent is eligible for the RMI regardless of age. Finally, and differently from Bargain and Doorley (2011), we shall consider both female and male singles as well as all education categories. However, our results shall differentiate the employment effect for all and for a specific group, the *high school (HS) dropouts*, who have the lowest financial gains to work in the short term and may also have 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.

Both Census and LFS data have comparable definitions of education categories, which is crucial for wage imputations.[6] Table A.1 in the Appendix 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 20-35. The LFS shows larger employment rates (as reflected in the average employment figures in Table A.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 an important aspect in our context.

For both samples, we also calculate disposable income C (consumption) for each individual in the data, which essentially corresponds to labor income decreased by social contributions and taxes paid on labor income and augmented with benefits received. Capital income is ignored as very small amounts are reported in this age group, especially for the low-educated youths that we focus on. 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. Importantly, Table A.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 (imputed) wage rate $\widetilde{w}$, $C(\widetilde{w}H, A)$.[7] Function C depends on

[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".

[7] Since we focus on the participation margin, we set H to 39 hours per week, the institutionally set full time option in France in 1999. Function C is approximated by numerical simulation of tax-benefit rules using the microsimulation model EUROMOD. This calculator allows the computation of all social

age, denoted A, since benefits, like the RMI, are conditional on age. Finally, we can also calculate disposable income under hypothetical, counterfactual scenarios where (i) RMI is completely withdrawn from the French social system, C^0, or (ii) there is no more age condition in eligibility, C^1.

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 and predicted on the basis of wages calculated for workers.[8] Calculated as earnings divided by worked hours, they may be contaminated by the same measurement error as those contained in worked hours. They are also a function of omitted variables that are associated with preferences, as discussed in the introduction. These two issues, division bias and endogeneity, are the major reasons behind the rejection of the Hausman labor supply model. As previously discussed, discrete choice models, fully accounting for tax-benefit policies, are more robust given variation across space (US states), over time (policy reforms) or across sub-groups (e.g. different age groups subject to different tax-benefit treatment as exploited here). Tax-benefit nonlinearities combined with different socio-demographic groups cannot be used in our context, given the homogeneity of the group under study (childless single individuals aged 20-35). However, demographic variation in age is used together with the discontinuity (age condition) of the RMI as the key source of identification.

4.1 RD Design

Before turning to the structural model, we recall the age discontinuity in the RMI program can be exploited using Census information. Consider the regression model with the propensity to be employed:

$$Y_i^* = \beta_0 + \eta.I(A_i \geq 25) + \beta_1.\delta(A_i) + \varepsilon_i. \tag{1}$$

The model is easily estimated by logit or probit techniques, denoting employment $Y_i = 1$ for those with $Y_i^* > 0$ and 0 otherwise.[9] The effect of age A_i (the "forcing" variable) on the

contributions, direct taxes and transfers to yield household disposable income (see Bargain, 2006 ed.).

[8]Even for workers, net gains to work are not known since they typically depend on unobservables like compensating differentials, fixed costs of work, stigma from receiving welfare payments, etc.

[9]It can also be estimated by linear probability model using directly $Y_i^* = 1$ for the workers and 0 for others, or by grouping observation into age cells A (so that the left-hand side $\overline{Y}_A$ becomes the average

outcome variable is captured by a smooth function $\delta(A_i)$ and by $I(A_i \geq 25)$, a treatment dummy that takes the value 1 if the individual is aged 25 or above (and can avail of the RMI if unemployed) and 0 otherwise. In this way, we can estimate the effect η of the treatment, the availability of the RMI, on employment. The key identification assumption of the RD approach is that $\delta(\cdot)$ is a continuous function. Under this assumption, 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 (age 25). For $\delta(\cdot)$, we use a cubic form which is flexible enough for our purpose.[10] The main argument for assuming that $\delta(\cdot)$ is a smooth function is that employment or work hours typically exhibit regular age profiles. Function $\delta(\cdot)$ should certainly be flexible enough to accommodate nonlinearities in age profiles, but there is no reason – in human capital or related theories of behavior over the lifecycle – to expect an abrupt change at age 25. 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, the treatment variable is a deterministic function of age and we have a "sharp" RD design.

We also add covariates Z_i to control for other dimensions than age (gender, region).[11] Because of a weaker attachment to the labor market, HS dropouts may also behave differently from other education groups. Therefore, we differentiate the employment effect for HS dropouts and for those with some education. The model becomes:

$$Y_i^* = \beta_{0i} + \eta_i.I(A_i \geq 25) + \beta_{1i}.\delta(A_i) + \beta_{2i}.Z_i + \varepsilon_i \tag{2}$$

with $\beta_{0i}, \beta_{1i}, \beta_{2i}$ and η_i varying with a dummy edu that take value 0 if the individual is a HS dropout and 1 otherwise. We refrain from using more detailed education categories for comparability with the next model, as explained below.

4.2 Adding Structure: Participation Model

We reduce the labor supply decision to a participation choice, and adopt a purely static perspective here, as in Laroque and Salanié (2002).[12] The participation model is in

participation rate in age group A), weighted by sample size in each cell (see Lemieux and Milligan, 2008). Results are not sensitive to these different methods.

[10] We have used several alternative flexible functions, including various polynomial forms, linear and quadratic spline and non-parametric methods. Results do not change much with the specification, as soon as sufficiently flexible forms are used (cf. Bargain and Doorley, 2011).

[11] Region is not available in the largest Census data (1/4), only in the 1/20 Census. Our favorite estimation on the large sample therefore relies only on gender as additional source of variation.

[12] Thus we neglect the fact that taking a job today may increase the probability of having one tomorrow. Some of these dynamic effects may appear in the estimated coefficients of the model.

principle very similar to the RD model in equation (1). The utility when working is written

$$U_i(H) = \alpha_0 + \gamma_1.C(\widetilde{w}_i H; A_i) + \alpha_1.\delta(A_i) + \epsilon_{1i} \tag{3}$$

while the utility when not working is simply:

$$U_i(0) = \gamma_0.C(0; A_i) + \epsilon_{0i}. \tag{4}$$

Only the coefficients of the terms varying with the labor supply choice are identified, i.e. γ_1 and γ_2, while the other ones are normalized to zero for the non-working option. The deterministic utility levels are completed by i.i.d. error terms ϵ_{ki} for each choice $k = 0, 1$. They are assumed to follow an extreme value type I (EV-I) distribution and to represent possible observational errors, optimization errors or transitory situations. The propensity to be employed is written as the difference between these two utility levels:

$$Y_i^* = \alpha_0 + \gamma_1.C(\widetilde{w}_i H; A_i) - \gamma_0.C(0; A_i) + \alpha_1.\delta(A_i) + \epsilon_i. \tag{5}$$

with $\epsilon_i = \epsilon_{1i} - \epsilon_{0i}$. The model is very similar to the RD model in equation (1), as it contains the same smooth function of age $\delta(A_i)$. There are two main differences however. Firstly, imputed wage $\widetilde{w}_i$ are also a smooth function of age, and this must be taken into account when extracting the policy/treatment effect, as explained below. Secondly, and most importantly, the treatment effect is here captured by the financial gain to work, as measured by the distance between disposable income when employed, $C(\widetilde{w}_i H; A_i)$, and disposable income when not working, $C(0; A_i)$. In practice, as can be seen in equation (5), 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 stigma effect when living on welfare). The structural, behavioral assumption in this model is the same as in the RD model: (statically) optimizing agents decide upon their labor supply function of financial incentives, and those aged 25 have lower incentives to work than similar persons aged 24. The discontinuity is not in a reduced form here but accounted for by different levels of income when unemployed, i.e. $C(0; 25) >> C(0; 24)$.

As above, we add observed heterogeneity Z_i and suggest a specific treatment for the HS dropouts. In addition to lower wage prospects, that should be reflected in predicted wages $\widetilde{w}_i$, those with no education have indeed lower attachment to the labor market (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 Euwals and van

Soest, 1999).[13] In our simple participation model, and as in the RD model of equation (2), we interact the coefficients with a dummy edu to account for specific behavior among the HS dropouts. This gives individual-specific coefficients and, hence, the following model:

$$Y_i^* = \alpha_{0i} + \gamma_{1i}.C(\widetilde{w}_i H; A_i) - \gamma_{0i}.C(0; A_i) + \alpha_{1i}.\delta(A_i) + \alpha_{2i}.Z_i + \epsilon_i. \tag{6}$$

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 and Das, 2001). In variants of the main model, we shall also add unobserved heterogeneity to the utility function when working, taking the form of a random, normally distributed term u_i (with zero mean and variance σ_u^2) added to ϵ_i^1. This term would correspond 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. The treatment of this additive heterogeneity is explained in detailed in the results section.

4.3 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.[14] Ignoring observed heterogeneity as in (1) and assuming we use linear probability model to ease the notation below, the treatment effect γ in the RD design is written:

$$\gamma = \overline{Y}_{25} - \overline{Y}_{24} + \beta_1.[\delta(25) - \delta(24)] \tag{7}$$

with $\overline{Y}_A$ the average participation level at age A, and it depends on the choice of the smooth function $\delta(\cdot)$. By analogy, we could define the treatment effect in the structural model as:

$$\overline{Y}_{25} - \overline{Y}_{24} + \alpha_1.[\delta(25) - \delta(24)] \tag{8}$$

which also corresponds to the change in financial gains to work between 25 and 24 years of age. Assuming $\gamma_1 = \gamma_0 = \gamma > 0$, this is indeed:

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

[13] More advanced modeling should incorporate both demand and supply side. Data limitation makes this type of extension very rare in the literature (a notable exception is Peichl and Siegloch, 2010).

[14] This is so even in the RD design because the forcing variable (age) is discrete. In this case, the treatment effect is not identified non-parametrically since we cannot compare observations "close enough" on both sides of the cutoff point. We must rely on parametric functions $\delta(A)$ to obtain the appropriate extrapolation (see Lee and Card, 2008, for a detailed discussion).

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 in the behavioral model requires the evaluation of the employment gap at age 25, accounting for the counterfactual situation C^0 (no RMI):

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

This corresponds to:

$$\overline{Y}_{25} - \overline{Y}_{24} + \alpha_1.[\delta(25) - \delta(24)] + \gamma\{C(\widetilde{w}_i H;24) - C(0;24)\} - \gamma\{C^0(\widetilde{w}_i H;25) - C^0(0;25)\}$$

or using specific effect of in-work and out-of-work income:

$$\overline{Y}_{25} - \overline{Y}_{24} + \alpha_1.[\delta(25) - \delta(24)] + \gamma_0\{C(0;25) - C^0(0;24)\} - \gamma_1\{C^0(\widetilde{w}_i H;25) - C(\widetilde{w}_i H;24)\} \quad (9)$$

In this formula, $C(0;25) - C^0(0;24)$ is zero by definition, and compared to (8), we essentially correct for different wage levels at age 25 and 24 in the last r.h.s. term.

4.4 Wage Estimations

Part of the structural model estimation is the wage equation for the imputation of $\widetilde{w}_i$ for all observations in the Census. In general, a two-stage approach is used for convenience in the literature (Laroque and Salanié, 2002, is one of the only exceptions we are aware of). In this case, we cannot proceed with a simultaneous estimation of wages and the labor supply model given that we rely on two different datasets. It is important to recall, however, that "actual" wages, i.e. wages calculated as earnings divided by hours, should not be used directly, even if we disposed of wage information in the Census. Indeed, they pose the risk of division bias and of introducing measurement errors in the model. Instead, wage rates should be predicted for all observations, workers and non-workers. The LFS can provide estimates which are accurate enough for this purpose, i.e. for predicting wage rates for all Census observations.[15] Robust information on earnings (base salary plus all bonuses and extra time payment) and work hours in the LFS is used to calculate wages for the workers.

The wage equation is specified as:

$$w_i = \theta(A_i) + \kappa.Z_i + \zeta.EDUC_i + \rho\lambda_i + \nu_i \quad (10)$$

with explanatory variables essentially reduced to the variables available in both Census and LFS datasets. This includes a smooth function of age $\theta(A_i)$, Z_i the controls used

[15] Elbers et al (2003) set out procedures for imputing information from a small dataset to a larger one.

also in the labor supply model and $EDUC_i$, the set of detailed education categories. 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 an instrument corresponding to disposable income at zero hours $C(0; A_i)$, hence relying again on the discontinuity at age 25 for identification. Unobserved productivity ν_i is assumed to follow a normal distribution with zero mean and variance σ_ν^2. The empirical variance is retrieved from the wage distribution in the LFS and used to impute a random component $\widetilde{\nu}_i$ when predicted wages in the Census. Since workers cannot possibly be paid below the minimum wage (MW), 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.

As an additional robustness check, we will use an alternative wage imputation based on random draws of actual wages from the LFS by detailed age, gender and education category. The results from this imputation are very similar to this obtained using the wage model defined above.

4.5 Estimation Method and Discussion

Model (6) is estimated by simulated maximum likelihood. Under the assumption that error terms ϵ_{ki}, $k = 0, 1$, 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 the present case. However, because the model is nonlinear, the wage-rate 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 $\widetilde{\nu}_i$ (and recalculating disposable income each time), and the simulated likelihood function can be maximized to obtain all estimated parameters. 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.

In the case when unobserved heterogeneity in preferences is accounted for, conditional probabilities are averaged over a number of draws for both the wage residuals ν_i and preference terms u_i. Non-employment can be rationalized by (i) a low utility (or high disutility) of work, i.e. u_i very small or negative; (ii) weak financial incentives (low productivity ν_i and, hence, low financial gain to work); (iii) demand-side constraints (productivity ν_i below the MW so the person is rationed out of the labor market); (iv) "other" non-employment. It is, a priori, not possible to distinguish between these four explanations,

unless extreme identifying assumptions are made. Laroque and Salanié (2002) model participation in a similar way as we do here, at least as far as supply-side aspects (i) and (ii) are concerned. For classical non-employment (iii), they estimate the wage equation jointly with the employment model and account for the probability of being rationed (i.e., below the MW) in the individual likelihood. We cannot proceed in this way in our two-stage approach, but simply assume that workers cannot be paid below the MW while non-workers may have such low productivities, as explained above. Concerning (iv), "other" non-employment is an heterogeneous category that covers frictional non-employment (the person is between two jobs) or cyclical non-employment (e.g. of a Keynesian nature) among other things. Laroque and Salanié explicitly model a probability for this "other" non-employment, identified using diploma and age as explanatory variables. We make a different parametric modeling choice here, but the information content is the same.[16]
As recalled above, the important point is that cross-sectional wage variation, as used here or in Laroque and Salanié (2002), may not isolate financial incentives from preferences well if endogeneity is an issue. In the next section, we shall see that in wage estimations on the LFS, we do not find a significant coefficient on the selection term λ_i. This may be due to the small sample size in the LFS, however, or the use of an incomplete participation model in the Heckman correction. If we assume that endogeneity exists and that hard workers are also better paid people, then identification of the model may be very weak unless exogenous variation is found. For our population of workers aged 20-35, this variation is precisely the discontinuity in benefit rules at age 25. We suggest three additional models which assume different degrees of endogeneity, and we check how these models perform when benefiting from the discontinuity for identification. In these models, we draw wage residuals ν_i and preference terms u_i simultaneously in a bivariate normal distribution, with variance σ_v taken from the empirical wage distribution in the LFS, variance σ_u normalized to 1 and correlation $corr(u_i, v_i)$ arbitrarily fixed at certain levels: 0, .25 and .50. The corresponding models are denoted P0, P25 and P50 in what follows.

5 Results

5.1 Wage estimation

Log hourly wage estimations using the LFS data are reported in Table A.2 in the Appendix. A significant gender gap can be observed, in line with the existence of a "sticky

[16]Indeed, we account for age and education (HS dropout versus educated workers) in the scaling factors α_{0i}, α_{1i} and α_{2i}. In terms of interpretation, we do not attempt to separate these effects from more "supply-side" explanations (preferences, fixed costs, stigma) or structural rending of demand-side or "other" non-employment explanations (job search costs).

floor" effect in France (Arulampalam et al, 2007) as well as a regular wage progression with the level of education. As mentioned above, 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. We have indeed observed that in this survey, the discontinuity does not appear to affect employment, which is certainly due to the erratic employment-age pattern discussed in Section **??**. We then 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 A.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 Basic Comparisons: RD vs Participation Model

We first present a graphical representation of the RMI effect. In Figure 1, 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 5 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.

First columns of Table 1 report the actual employment rates at 24 and 25 years of age. The difference is -0.7 ppt in the broader group against -3.4 ppt among HS dropouts.

Figure 1: Employment Rate of Childless Singles and Discontinuity

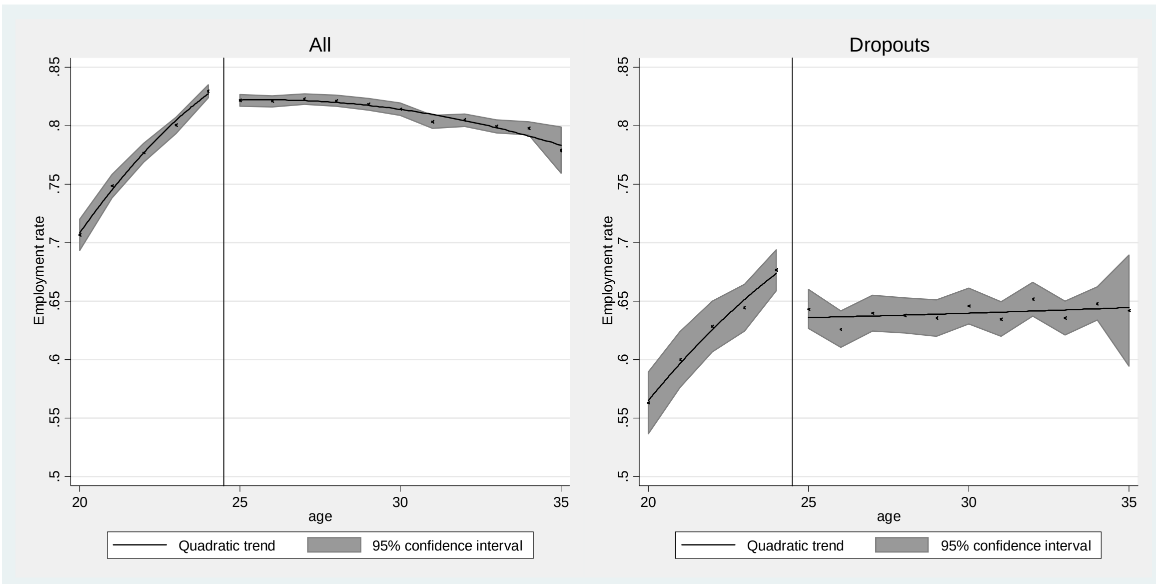


Table 1: Employment Effects of the RMI: RD vs. Structural Model

	Actual Participation Rates			Treatment Effect	Predicted Participation Rates (Model P)			Treatment Effects			
	Age 24	Age 25	Difference	RD	Age 24	Age 25	Difference	Model P	Model P0	Model P25	Model P50
All education groups											
All	82.9%	82.2%	-0.7%	**-1.9%**	81.8%	81.2%	-0.6%	**-1.6%**	-1.6%	-1.5%	-1.5%
Male	83.4%	83.3%	-0.1%	**-2.5%**	82.8%	81.9%	-0.8%	**-1.8%**	-1.8%	-1.7%	-1.7%
Female	82.4%	80.8%	-1.6%	**-1.2%**	80.7%	80.3%	-0.4%	**-1.3%**	-1.4%	-1.3%	-1.3%
HS Dropouts											
All	67.7%	64.3%	-3.4%	**-5.0%**	66.0%	62.2%	-3.8%	**-4.7%**	-4.8%	-4.6%	-4.4%
Male	70.5%	66.5%	-4.0%	**-5.5%**	68.3%	64.3%	-4.1%	**-4.8%**	-4.9%	-4.8%	-4.5%
Female	63.1%	60.8%	-2.3%	**-4.2%**	62.2%	58.8%	-3.3%	**-4.4%**	-4.5%	-4.4%	-4.2%

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.

When accounting for the age trends to extrapolate toward the threshold, we obtain treatment effects of -1.9 ppt and -5.0 ppt for these two groups respectively. Both effects are statistically significant and confirm a substantial negative effect of the RMI on the 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 7% 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. The overall effect, however, is in line with the RD results: -1.6 and -4.7 ppt for the whole selected sample and for HS dropouts respectively. These effects are not significantly different from those of the RD approach. The last columns of Table 1 report the treatment effects for models accounting for unobserved heterogeneity as discussed in section 4 (models P0, P25 and P50). Under the assumption of no correlation between wages and unobserved preference heterogeneity, the change in financial gains to work due to the RMI availability has a similar effect (a drop in employment of 4.8 ppt for the dropouts). With high correlation (P50), the elasticity of labor supply to financial incentives mechanically decreases, as could be expected. As a result, the employment effect becomes smaller in absolute value (-4.4 ppt, still close to the -5.7 in the baseline model P). The discrepancy under this extreme assumption is, however, not so large in absolute terms. Alternative specifications of the smooth function of age (linear and quadratic splines) do not affect these conclusions qualitatively, and quantitative differences are relatively small (results available from the authors).

Table A.3 in the Appendix shows the estimates of the RD model and of the four participation models P, P0, P25 and P50 (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 1 (-4.3). Looking at the constant in the coefficients on in-work and out-of-work income, 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 (except for the model with high correlation between work preferences and wages, where it is about 3 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 almost reduced to zero in models P and P25. The second observation is that the effect of income at zero is relatively constant across models. This explains the results above, that model predictions do not vary too 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 preference 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.

5.3 Identification based on the Discontinuity: 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 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 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 rate at all ages, as well as the treatment effect, on the other half (the holdout sample).

Results are reported in Table 2. 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 (-1.4 and -4.2 for the whole selection and for HS dropouts respectively). The participation model seems to perform relatively well, even if treatment effects are larger the "true" response as measured by the RD at $.-2$ and -5.8 for the whole selection and for HS dropouts respectively This is possibly due to a combination of "bad" predictions at age levels far from the discontinuity and of the use of the cubic form of age. Indeed, if we look closely at the mere predictions of employment levels at age 24 and 25, the participation

Table 2: Employment Effects of the RMI: using Cross-validation Samples

	Actual Participation Rates			Treatment Effect	Predicted Participation Rates			Treatment Effects
	Age 24	Age 25	Difference	RD	Age 24	Age 25	Difference	Model P
All education groups								
All	82.6%	82.2%	-0.4%	**-1.4%**	82.2%	81.1%	-1.1%	**-2.0%**
Male	82.6%	83.0%	0.5%	**-1.9%**	83.3%	81.9%	-1.4%	**-2.3%**
Female	81.0%	80.2%	-0.8%	**-0.8%**	81.0%	80.2%	-0.8%	**-1.7%**
HS Dropouts								
All	67.2%	63.5%	-3.7%	**-4.2%**	66.8%	62.1%	-4.7%	**-5.8%**
Male	69.4%	66.1%	-3.3%	**-4.4%**	69.0%	63.9%	-5.1%	**-6.1%**
Female	63.6%	59.2%	-4.3%	**-3.8%**	63.2%	59.0%	-4.2%	**-5.3%**

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

model provides relatively accurate measures. The predicted difference between the two age groups is close to the actual one, excepted for males. The model nonetheless 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.

5.4 Counterfactual Simulations

Youth unemployment is an important issue, particularly in France where it stood at around 27% in 1999. It has received renewed attention recently as it becomes even more accentuated in a recessionary context (Bell and Blanchflower, 2010). 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. Extrapolating results based on the RD is difficult. Structural models may help to quantify the likely effect – assuming they provide enough external validity –

as they are based on the precise financial gains at each age level and offer the possibility to simulate counterfactual policies.

To investigate this question, we first check the predictive power of the participation model at all age levels and not only around 25 years of age. The top l.h.s. graphs in Figures 2 and 3 report 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
The next two graphs in Figures 2 and 3 simulate counterfactual situations as explained above: (i) abolishing the RMI (as defined at the end of section 3: $C(0, A)$ replaced by $C^0(0, A)$), (ii) 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 (iii) 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 *Revenu de Solidarite Active* (RSA). While these hypothetical reforms have little effect on the whole sample, the HS dropouts show much response to both reforms. Interestingly, 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. 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 which fades away after around age 30. For the group of HS dropouts, it has a larger positive effect on employment rates of about 3 points which fades towards age 35.

5.5 The current French policy debate

One further simulation, which is relevent 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 4, 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

Figure 2: Employment Rate of Single Childless Individuals: All Education Groups

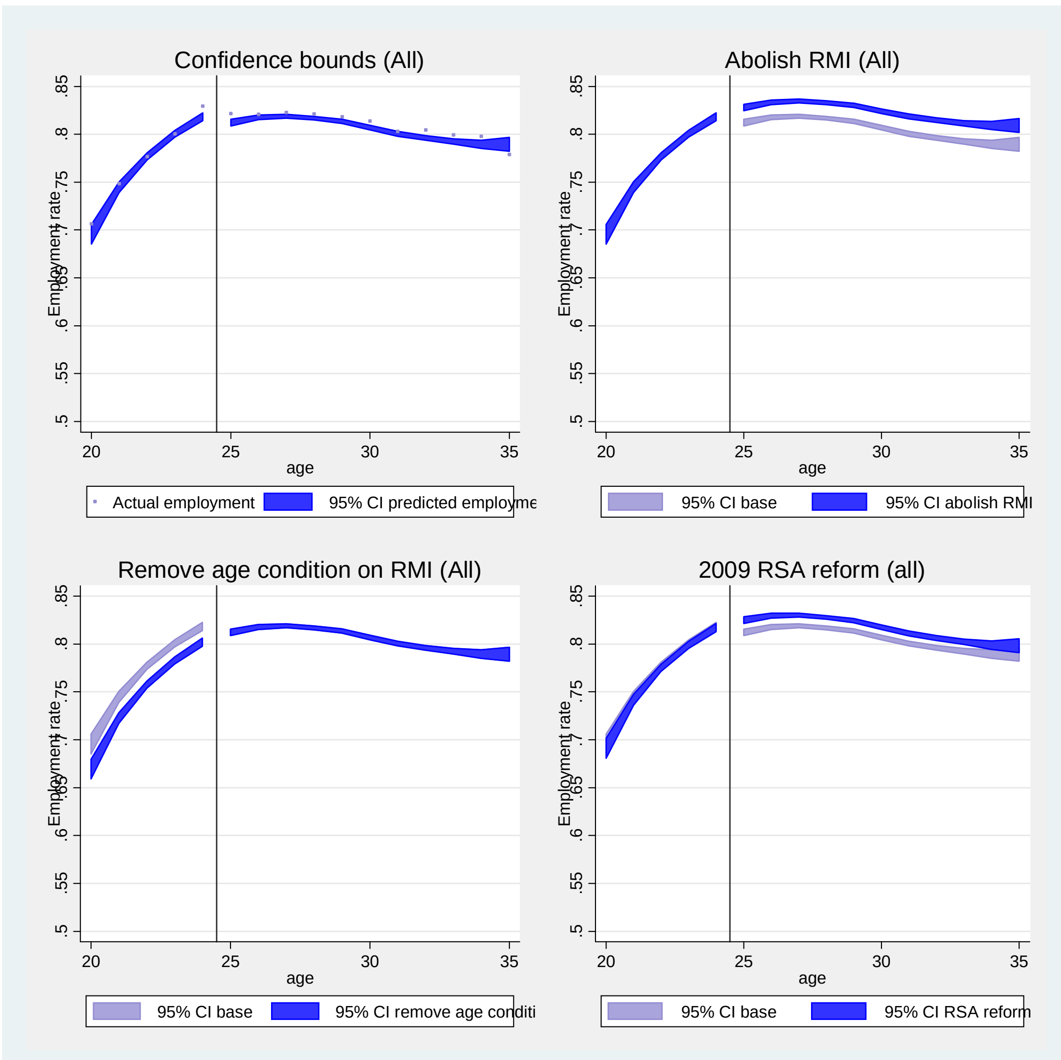
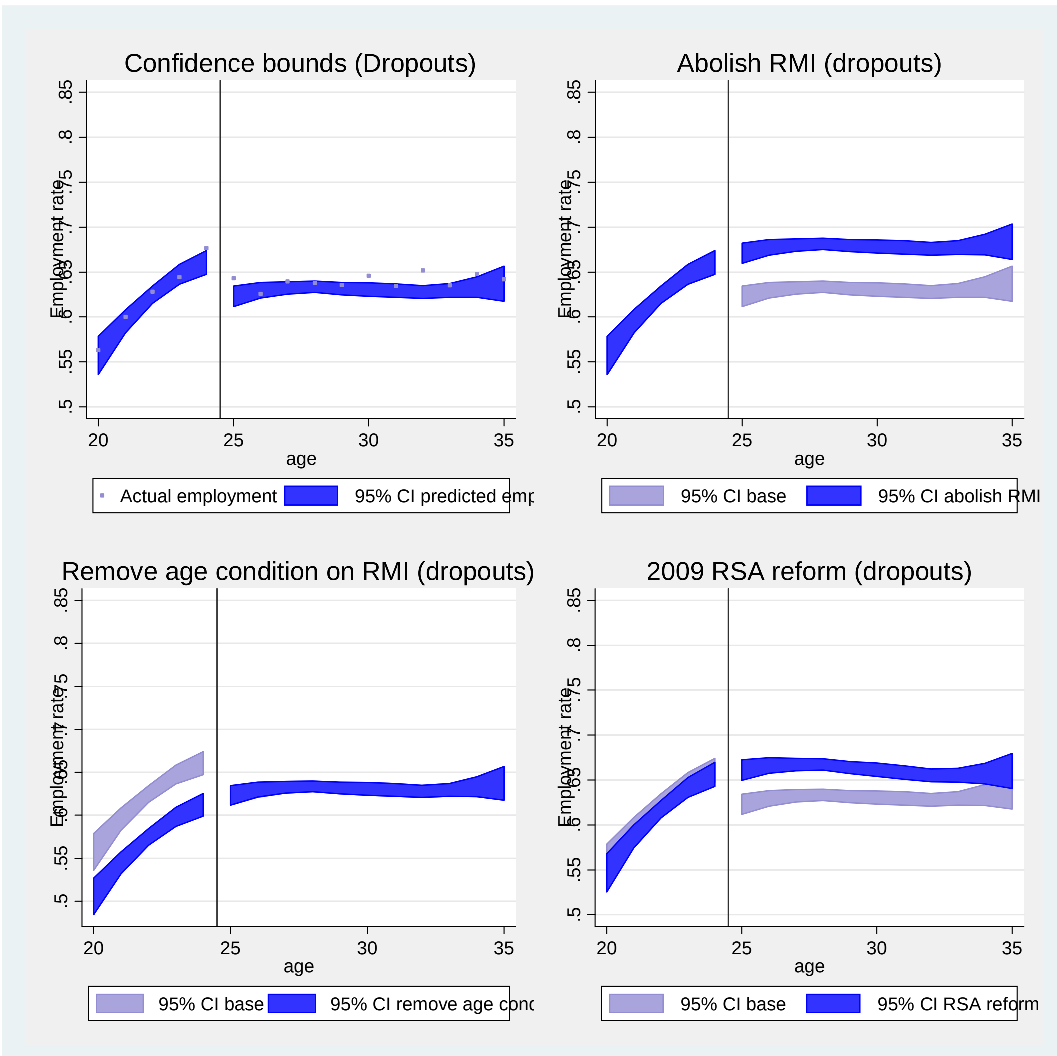
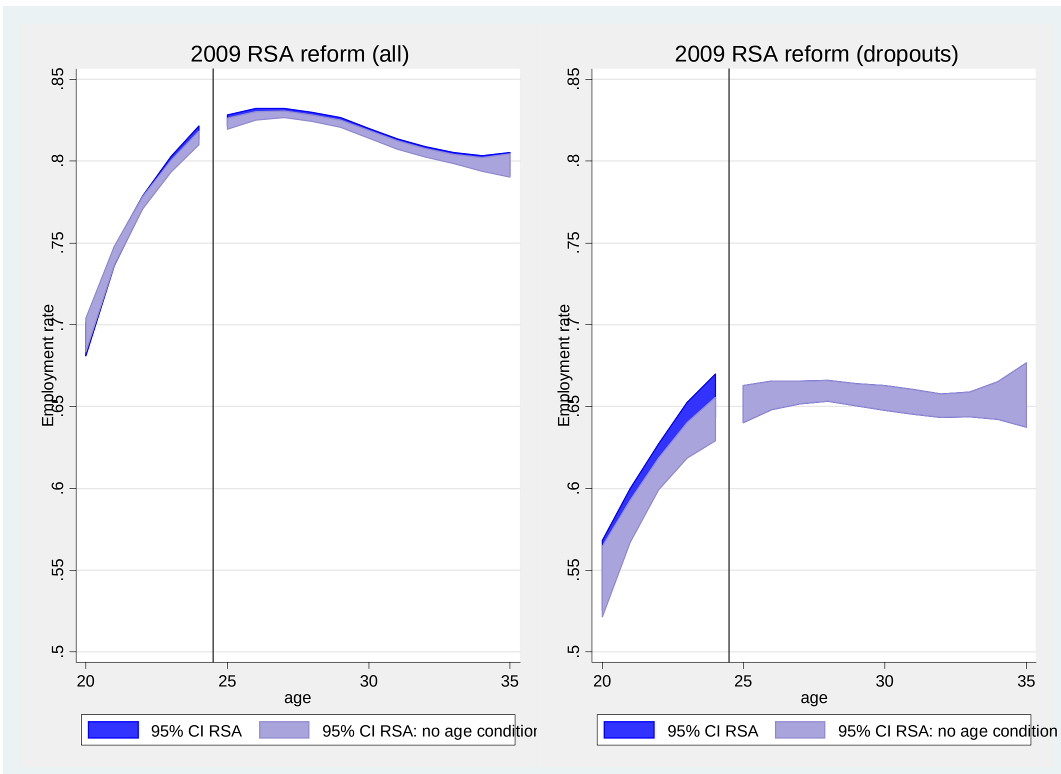


Figure 3: Employment Rate of Single Childless Individuals: HS Dropouts



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 A.3 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 the bottom left panels of Figures 2 and 3. As the RMI lacks any in-work incentives, the employment effect is negative and becomes large for the population of HS dropouts.

Figure 4: The employment effect of extending the RSA to under-25 year olds



6 Robustness checks

6.1 Alternative wage estimation

The wage estimation outlined in section 4.4 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 erronous 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. Results are shown in figure 3. The participation models' estimates of the employment drop is the same for the whole selection of men and women ($-1.6ppt$), regardless of the wage imputation method used. For the group of highschool dropouts, the estimations are very close at $-4.5ppt$ using matched wages compared to $-4.7ppt$ with the former wage estimation technique. Both are comparable to the RD estimation of $-5ppt$ for HS dropouts.

Table 3: 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	Model P
All education groups					
All	82.9%	82.2%	-0.7%	**-1.9%**	**-1.6%**
Male	83.4%	83.3%	-0.1%	**-2.5%**	**-1.7%**
Female	82.4%	80.8%	-1.6%	**-1.2%**	**-1.4%**
HS Dropouts					
All	67.7%	64.3%	-3.4%	**-5.0%**	**-4.5%**
Male	70.5%	66.5%	-4.0%	**-5.5%**	**-4.6%**
Female	63.1%	60.8%	-2.3%	**-4.2%**	**-4.4%**

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

6.2 Identification based on the gains to work

As a further robustness check, we modify our participation model so that it is the difference in disposable income at 0 and 39 hours that predicts the propensity to work. Model 6 becomes:

$$Y_i^* = \alpha_{0i} + \gamma_{2i}.[C(\widetilde{w}_i H; A_i) - C(0; A_i)] + \alpha_{1i}.\delta(A_i) + \alpha_{2i}.Z_i + \epsilon_i. \tag{11}$$

The estimated parameters of this model, Model D, are shown in the last column of Table A.3 in the Appendix. The coefficient on the *difference* between in-work and out-of work income for female HS dropouts is 0.24. Comparing this to the implied coefficient of 0.21 estimated by model P (by subtracting the coefficient on out-of-work income from that of in-work income), it is clear that this model will give slightly different results as the estimate of the work incentive provided by in-work income, compared to out-of-work income is higher in model D. The estimated treatment effects from Model D, are shown in table 4 and they are, indeed, incomparable to the RD estimates. As age changes from 24 to 25, the gain to work decreases. As the coefficient on this gain to work is higher in model D than in model P, the probability of working, given a drop in the gains to work, should decreases in model D by more than it does in model P. From table 4, we see that this is indeed the case. The estimated treatment effects from Model D, are much larger and incomparable to the RD estimates.

7 Conclusions

In this study, we study the labor supply effect of the French social assistance using age discontinuity in eligibility. We primarily compare the policy effect measured by RD and by the predictions of aa structural model identified on the same discontinuity. By focusing on a homogenous group of the population, i.e. childless singles, we rule out most of the usual sources of identification stemming from the nonlinearity of tax-benefit systems combined with variation in demographic composition. We isolate the role of the specific exogenous variation in the identification of labor supply models and the characterization of underlying preferences. Structural models show satisfying results for both internal and external validity as long as predictions relate to responses to variations in out-of-work income or in-work income rather than the difference between the two. Inversely, this may suggest that identification strategies relying on changes in financial incentives for wealthy tax payers (e.g., changes in tax schedules) cannot be used to infer behavioral parameters for the analysis of reforms concerning other income groups, and notably the working poor concerned by EITC-types of reform.

Table 4: Employment Effects of the RMI: RD vs. Structural Model with identification based on the difference in disposable income at 0 and 39 hours

	Actual Participation Rates			Treatment Effect	Predicted Participation Rates (Model P)			Treatment Effects			
	Age 24	Age 25	Difference	RD	Age 24	Age 25	Difference	D	Model D0	Model D25	Model D50
All education groups											
All	82.9%	82.2%	-0.7%	**-1.9%**	84.2%	79.4%	-4.8%	**-6.3%**	-6.2%	-6.2%	-6.1%
Male	83.4%	83.3%	-0.1%	**-2.5%**	84.2%	80.4%	-3.8%	**-5.4%**	-5.3%	-5.3%	-5.3%
Female	82.4%	80.8%	-1.6%	**-1.2%**	84.2%	78.2%	-6.0%	**-7.4%**	-7.3%	-7.2%	-7.2%
HS Dropouts											
All	67.7%	64.3%	-3.4%	**-5.0%**	71.1%	58.4%	-12.7%	**-14.9%**	-13.0%	-12.8%	-12.6%
Male	70.5%	66.5%	-4.0%	**-5.5%**	72.1%	60.8%	-11.3%	**-13.4%**	-11.8%	-11.6%	-11.4%
Female	63.1%	60.8%	-2.3%	**-4.2%**	69.4%	54.4%	-15.0%	**-17.4%**	-15.0%	-14.7%	-14.5%

Model D is a participation model estimated by simulated ML on the gains to work with conditional probabilities averaged over ten wage draws. Model D0, D25 and D50 additionally include unobserved heterogeneity assumed to be potentially correlated with wage error terms; the correlation is 0, 0.25 and 0.50 respectively.

Predictions of employment responses to counterfactual scenarios where the RMI is abolished show that inactivity traps may be limited to individuals just above 25 and with low wage prospects. Similarly, extending the RMI to the under-25 year olds may generate greater unemployment, and possibly long-term poverty, among the youngest workers. This reinforces the concern that reducing poverty in this group should be done without further weakening their attachment to the labor market (cf. Cahuc et al., 2008). Conversely, extending the RSA, a new formulation of the RMI including an in-work benefit component, could increase the labour market attachment of the under-25 year olds.

Importantly, we have focused on a structural participation model. While extension of the present work could incorporate discrete work options like part-time or over-time, we believe that the extensive margin is the primary dimension that had to be investigated. This is surely the margin with the greatest degree of potential response, 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). It is therefore the best ground for comparing and possibly reconciling structural models and natural experiments. Also, 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 (see Peichl and Siegloch, 2010). Finally, the assumption of normality made for wage rates is certainly an issue, which is broadly ignored in the labor supply literature.

Uneducated workers may indeed have a specific wage distribution with a certain density at very low productivity levels, which would explain why a large portion of this group is not working. In this case, structural models would surely overstate the extent of inactivity traps.

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A Appendix A: Data Sources, Model Estimates and Wage Estimations

Table A.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	0.58	0.60	0.59	0.51	0.54	0.50	0.60	0.61	0.61
Age	28	28	30	23	23	23	29.3	30	30
Junior vocational qualification	0.28	0.27	0.27	0.29	0.25	0.27	0.27	0.28	0.27
Highschool	0.06	0.06	0.06	0.07	0.07	0.08	0.05	0.06	0.06
Vocational highschool	0.12	0.11	0.10	0.17	0.16	0.17	0.11	0.10	0.09
Graduate qualification	0.37	0.35	0.36	0.28	0.32	0.29	0.39	0.36	0.38
Dropouts	0.17	0.20	0.20	0.19	0.19	0.18	0.17	0.21	0.20
Work hours	30	33	28	29	32	31	30	33	33
Employment rate	0.81	0.84	0.83	0.79	0.84	0.82	0.81	0.84	0.83
Employment income	1,395	1,516	1,528	1,194	1,268	1,227	1,439	1,578	1,602
Disposable income	**962**	**944**	**940**	**796**	**827**	**806**	**1,000**	**972**	**971**
Sample size	286,205	14,659	2,972	53,048	2,785	561	233,157	11,874	2,411

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)

Table A.2: Wage Estimation on LFS Data

Variables	Log wage		Employment	
Age	0.005	-0.011	0.100	-0.055
Age square / 100	0.000	0.000	-0.002	-0.001
Female	-0.117	-0.006	0.064	-0.022
Junior vocational qualification	0.070	-0.009		
Highschool diploma	0.205	-0.014		
Vocational highschool dipl.	0.166	-0.011		
Graduate qualification	0.400	-0.009		
Disposable income 0 hours/100			-0.005	-0.017
Inverse Mills ratio	-0.005	0.063		
Constant	3.491	-0.155	-0.594	-0.730
Observations	10,287		14,909	

Table A.3: 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	0.383	0.064	1.579	0.356	1.744	0.389	1.767	0.389	1.800	0.388	1.738	0.355
Age2	-0.013	0.002	-0.053	0.013	-0.058	0.014	-0.059	0.014	-0.061	0.014	-0.052	0.013
Age3	0.000	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.001	0.000	0.000	0.000
Age*educated	-0.055	0.072	0.116	0.423	0.289	0.459	0.379	0.459	0.412	0.458	0.193	0.420
Age2*educated	0.002	0.003	-0.003	0.016	-0.009	0.017	-0.012	0.017	-0.013	0.017	-0.008	0.016
Age3*educated	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Male	0.068	0.005	0.676	0.051	0.789	0.055	0.794	0.054	0.805	0.054	0.312	0.027
Male*educated	-0.039	0.004	0.087	0.024	0.091	0.027	0.068	0.027	0.048	0.026	0.060	0.024
Educated	0.709	0.650	0.316	3.786	-0.917	4.116	-1.918	4.113	-2.380	4.102	-0.093	3.755
Constant	-3.116	0.577	-17.018	3.181	-18.987	3.477	-18.759	3.476	-18.697	3.468	-19.568	3.166
Coefficients on Age >=25												
Educated	0.037	0.010										
Male	-0.012	0.004										
Constant	-0.043	0.009										
Coefficients on Income when H=0 (divided by 100)												
Educated			-0.040	0.018	-0.050	0.019	-0.049	0.019	-0.046	0.019		
Male			0.009	0.008	0.011	0.008	0.010	0.008	0.010	0.008		
Constant			0.061	0.016	0.074	0.018	0.072	0.018	0.069	0.018		
Coefficients on Income when H=39 hours/week (divided by 100)												
Educated			-0.105	0.005	-0.126	0.005	-0.112	0.005	-0.099	0.005		
Male			-0.057	0.004	-0.065	0.004	-0.062	0.004	-0.060	0.004		
Constant			0.262	0.005	0.305	0.006	0.254	0.005	0.215	0.005		
Coefficients on [Income (H=39) - Income (H=0)] (divided by 100)												
Educated											-0.096	0.005
Male											-0.045	0.003
Constant											0.242	0.005
Log Likelihood			-130179		-149262		-151009		-152064		-130326	
prob > chi2			0		0		0		0		0	
Observations	286205		286205		286205		286205		286205		286205	

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

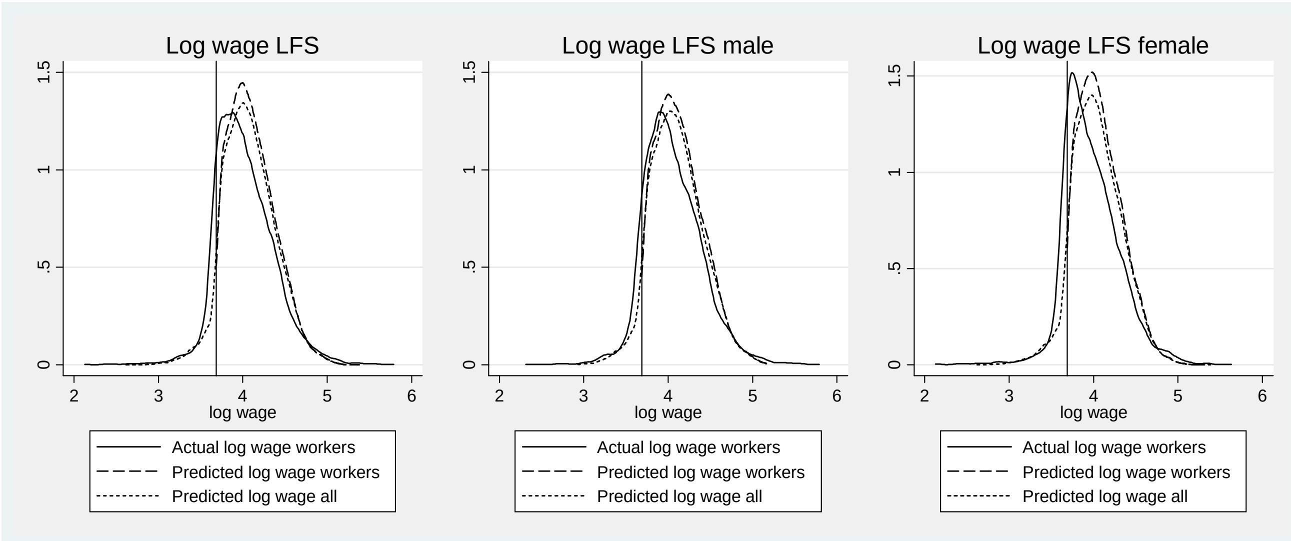

Figure A.1: Predicted and Actual Log Wage Distributions in LFS

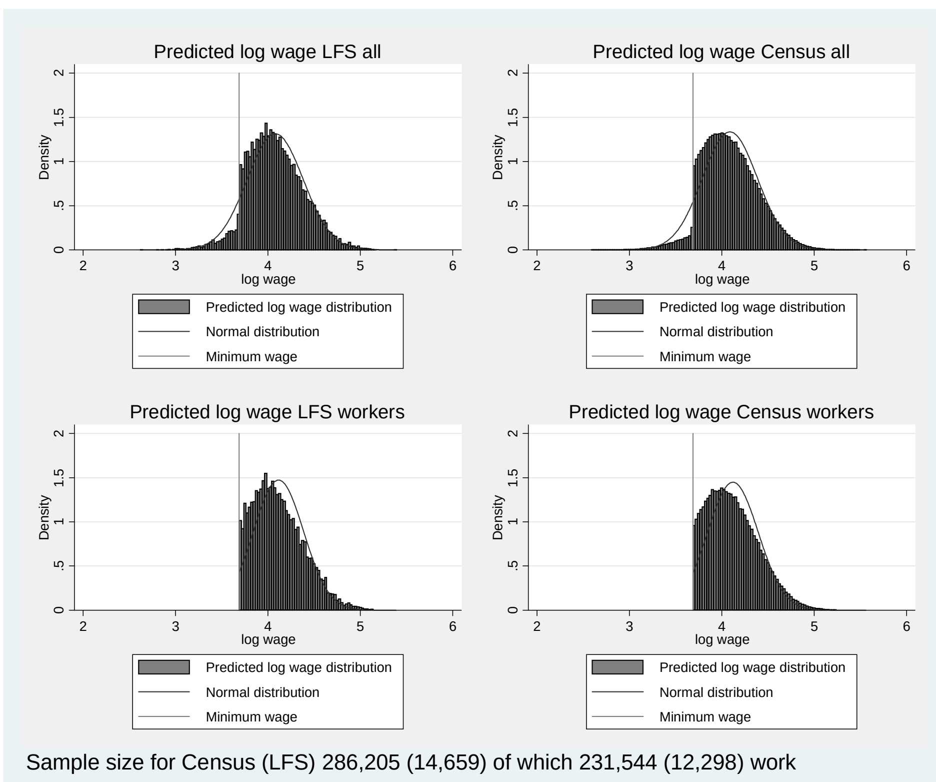

Figure A.2: Comparing Predicted Log Wage Distributions in LFS and Census Data (All)

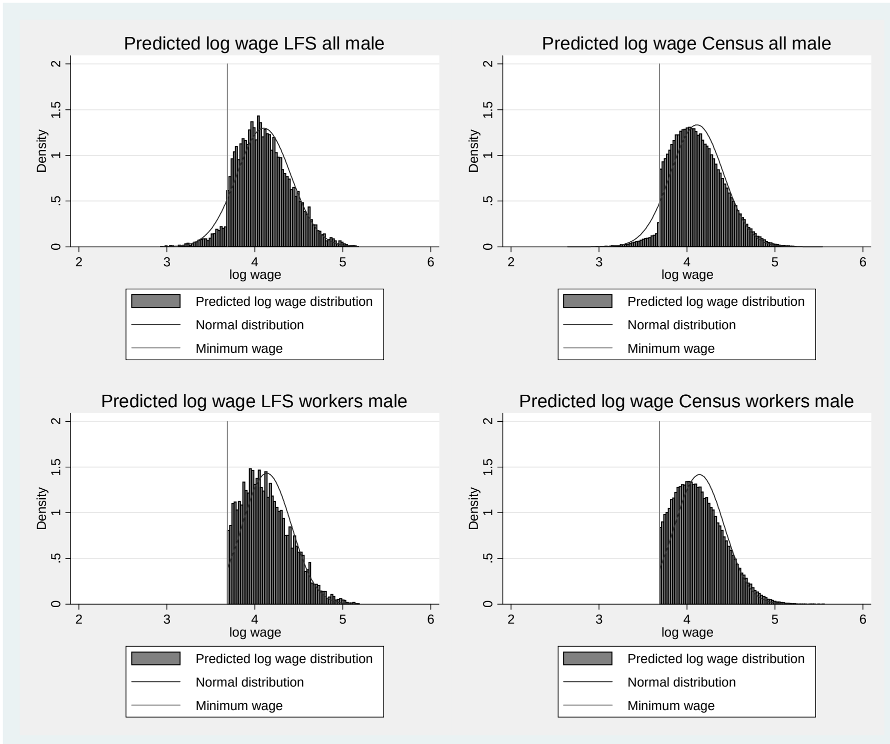

Figure A.3: Comparing Predicted Log Wage Distributions in LFS and Census Data (Men)

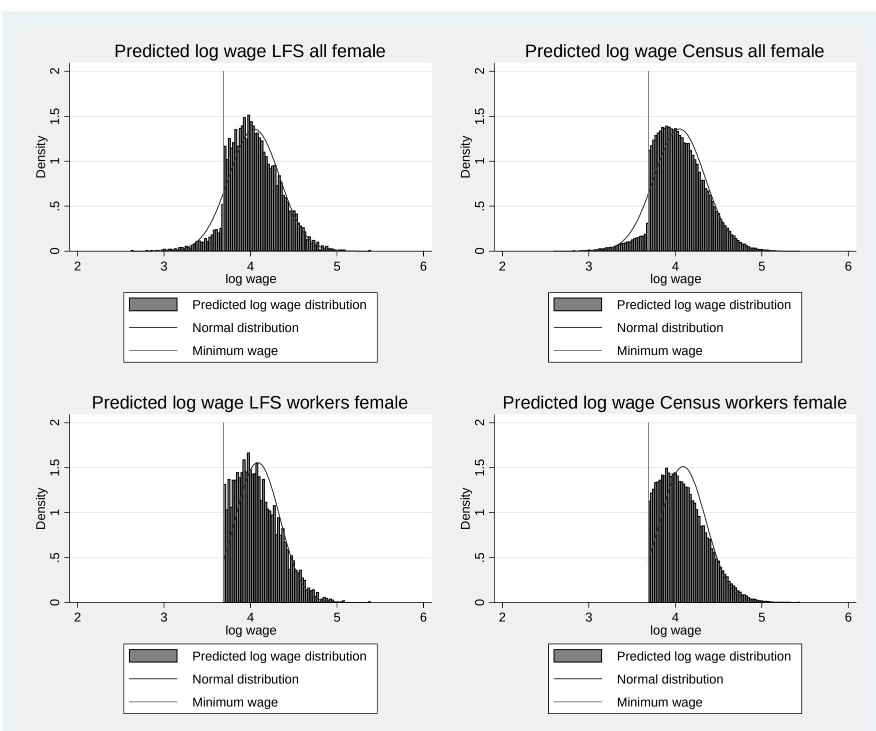

Figure A.4: Comparing Predicted Log Wage Distributions in LFS and Census Data (Women)

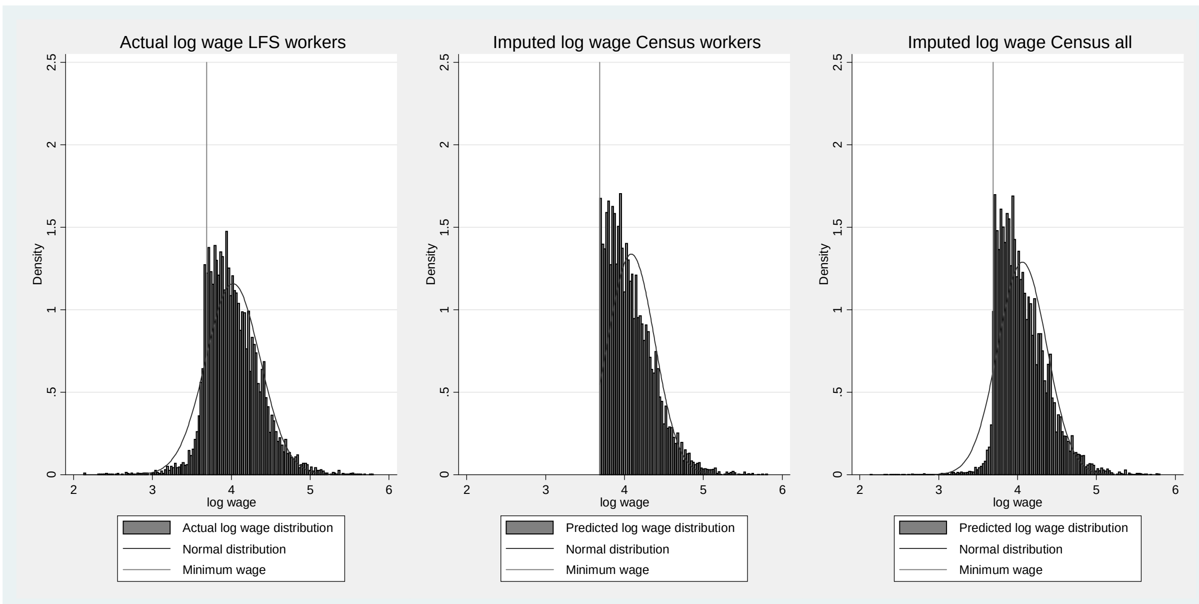

Figure A.5: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (All - matched)

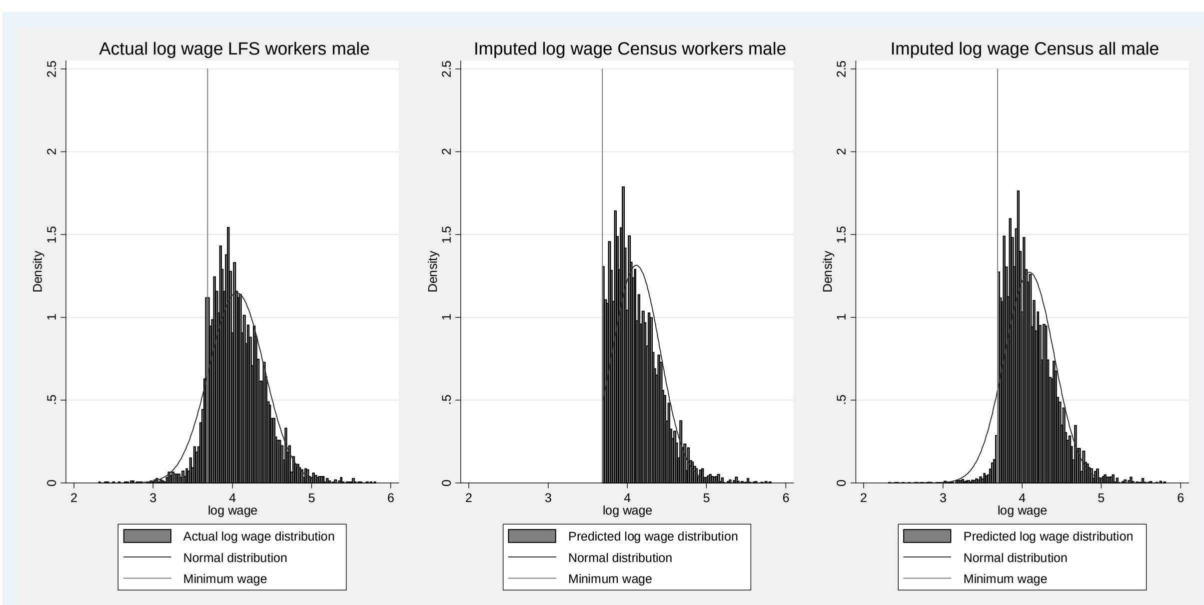

Figure A.6: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (Men - matched)

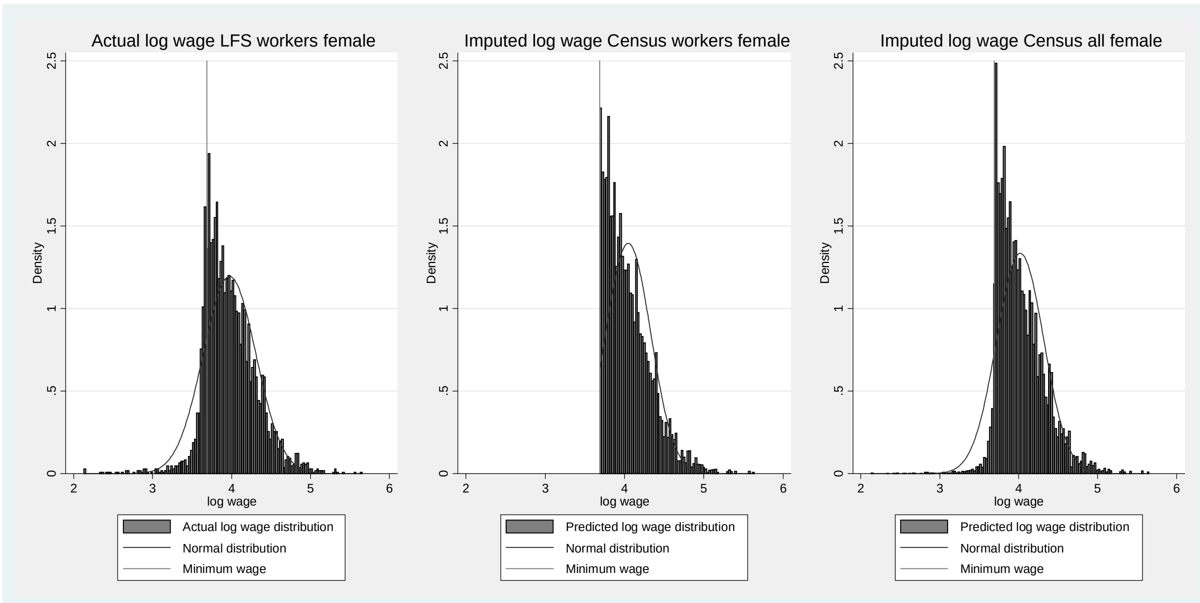

Figure A.7: Comparing Log Wage Distributions in LFS and Imputed Log Wage Distributions in the Census Data (Women - matched)