Caught in the trap? Welfare's disincentive and the labor supply of single men

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A B S T R A C T

Youth unemployment is particularly large in many industrialized countries and has dramatic consequences in both the short and long-term. While there is ample evidence about the labor supply of married women and single mothers, little is known about how young (childless) singles react to financial incentives. The French minimum income (Revenu Minimum d’Insertion, RMI), often accused of generating strong disincentives to work, offers a natural setting to study this question since childless single individuals, primarily males, constitute the core group of recipients. Exploiting the fact that childless adults under age 25 are not eligible for this program, we conduct a regression discontinuity analysis using French Census data. We find that the RMI reduces the participation of uneducated single men by 7–10% at age 25. We conduct an extensive robustness check and discuss the implications of our results for youth unemployment and current policy developments.

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1. Introduction

There has been an on-going debate in policy and academic circles about the nature and generosity of social transfers that should be put in place. Countries have made obviously different choices in this respect. For instance, in the US and the UK, transfers to the working poor play a major role, through programs such as the US Earned Income Tax Credit (EITC) and the UK Working Family Tax Credit (WFWC). In the US in particular, the perception that traditional welfare programs, like the Aid to Families with Dependent Children (AFDC), encouraged inactivity and fostered dependency on public assistance culminated with the 1996 Personal Responsibility and Work Opportunity Act (PRWORA) and the repeal of the AFDC. The latter has been replaced by the Temporary Assistance to Needy Families (TANF), which imposes work requirements, tightened eligibility conditions and time limits (Scholz and Levine, 2001). In contrast, continental European welfare states still rely on and large on traditional social assistance programs, which are time-unlimited, practically unconditional on work, training or job search, and not specifically targeted at families with children. Like in the US before 1996, these programs have been accused of creating strong work disincentives. This is particularly true for the French guaranteed minimum income (Revenu Minimum d’Insertion, RMI), which imposes very high implicit taxation on earnings (Gurgand and Margolis, 2008). As we learn from the applications of optimal tax theory (e.g., Saez, 2002), different institutions across countries may not only reflect profound differences in social preferences but also different labor market conditions and degrees of response to tax-benefit incentives. Labor supply is therefore a key issue that has received enormous attention. Mainly for the US and the UK, a variety of methods have been used to estimate labor supply behavior, including structural approaches (Hoynes, 1996; Meyer and Rosenbaum, 2001), experiments (e.g., Robins, 1985) or natural experiments that have precisely exploited the important US/UK tax-benefit reforms to identify behavioral parameters (e.g., Eissa and Liebman, 1996; Blundell et al., 1998). 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 (e.g., Laroque and Salanié, 2001) which necessarily rely on particular distributional assumptions and have not often been validated against natural experiments (the situation is therefore similar to the pre-1996 US literature). Another important observation is that most of the evidence available in the US and the UK concerns married women and single mothers, two groups known to be the most responsive to financial incentives. Indeed, the aforementioned welfare policies in these countries, and their reforms, were essentially targeted at these groups. In contrast, much less is known about the participation elasticity of childless single individuals, as these are excluded from most welfare programs in the US. Even if the traditional literature points to very small elasticities among singles, it is suspected that the response of low-skilled single individuals is significant, at least...
compared to higher education singles or married men, and deserves to be quantified.

Interestingly, childless single individuals, primarily males, constitute the core group of recipients of the RMI program in France. The main reason for this is that young workers’ contribution spells are too short for them to be eligible for unemployment benefits. It turns out that the RMI program offers an interesting setting to study the labor supply response of this demographic group. In this paper, we exploit a specific feature of the program, namely the fact that childless adults under age 25 are not eligible for the RMI. A regression discontinuity (RD) approach is particularly suited to establish whether the RMI generates a significant drop in labor market participation at age 25, as a static labor supply model would predict. Hahn et al. (2001) argue that studying specific policy discontinuities 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.1 Using this approach with the 1999 French Census data, we find compelling evidence that the RMI reduces labor market participation among uneducated childless single men at age 25. Our estimates point to a drop of 7–10% in men’s participation. As a falsification test, we verify that groups not affected by the age threshold do not show any response, in particular lone parents (for whom no age restriction applies) and uneducated men observed prior to the introduction of the RMI (i.e., using the 1982 Census). We also find that the RMI has no effect on single men with more education. We provide an extensive robustness analysis and check in particular (i) that no other policy could generate a similar discontinuity in 1999, and (ii) that agents cannot manipulate the ‘forcing variable’, at least not in a discontinuous way. Under these conditions, RD estimates offer a credible alternative to randomized experiments since, according to Lee and Lemieux (2010), assignment to treatment is ‘as good as random’ in the neighborhood of the discontinuity.

The paper brings the literature forward in several ways. Firstly, we examine a group – single individuals without children – whose labor supply behavior has received very little attention in the empirical literature on transfer programs. We focus on men because their labor supply decisions, and potential eligibility for the RMI around the discontinuity, are less affected by early fertility and marriage decisions compared to single women. The evidence we provide is based on large variations in budget constraints. In 1999, unemployed singles aged 25 and over could receive welfare payments of up to EUR 539 per month, i.e., 162% more than those aged under 25. Variations of this magnitude help us to estimate behavioral effects with better precision. Another advantage of the RD analysis in the present context is that we can evaluate the effect of the whole welfare program on labor supply, and not just an extension of it.2 For uneducated singles, the participation elasticity to welfare payments derived from our results is relatively modest, yet in line with past estimates for the US and strikingly similar in magnitude to the results of Lemieux and Milligan (2008).3

Secondly, the finding that only uneducated men respond to the transfer is important. It corroborates the idea that participation responses may be strongest for those with the lowest potential earnings (see suggestive evidence in Eissa and Liebman, 1996). Our results add to the limited evidence currently available that supports this assumption. Heterogeneous elasticity across different earnings groups is crucial for welfare analysis. Eissa et al. (2008) show that relaxing the opposite assumption (uniform participation elasticity) across the income distribution – and putting largest elasticities at the bottom – completely changes the conclusions of a normative assessment of four major reforms in the US.

Thirdly, we provide one of the first estimations of participation responses to transfers in France which is based on a quasi-experimental approach. Even if a specific group is examined, our results tend to support findings in Gurgand and Margolis (2008) concerning the RMI and the fact that inactivity traps may be confined only to certain household types.

Fourthly, our results add to the aforementioned policy debate on the design of optimal redistributive programs. In fact, the RMI has recently been extended to incorporate an EITC-type of earnings subsidy aimed at correcting (some of the) disincentive effects. Importantly, the 25-year-old discontinuity was maintained in the first years of application. While future research should extend the present approach to evaluate the labor supply response of this new program in a precise way, we provide tentative comments on what can be expected for the core group of RMI recipients. More generally, we also discuss its essential difference of scope to the EITC (or the UK WFTC before 2003), namely that the new program, just like the RMI, targets all household types and not only those with children.

Finally, even if our results are difficult to generalize to a broader population, the particular age group we focus on is extremely important. Indeed, youth unemployment is a recurrent problem in many OECD countries and appears even more acute during recessions (Bell and Blanchflower, 2010).4 It is structurally very large in France – around 27% in 1999, our main year of analysis – which explains the numerous government interventions over the past 30 years, including state-subsidized employment and training programs discussed in the paper. Among its dramatic consequences, youth unemployment leads to very high poverty rates in this population and has been shown to have a causal effect on crime in France (Fougère et al., 2009). In this context, it is particularly important to evaluate how social transfers may affect the labor supply of young workers, and we discuss the risks of extending the RMI eligibility to 16–24 year olds.

The paper is structured as follows. In Section 2, we provide a brief review of the scant evidence on the single individuals’ labor supply, a short overview of the literature on France and institutional background information. In Section 3, we use the French Labor Force Survey (LFS) to examine suggestive evidence of an inactivity trap at age 25 for uneducated singles. Section 4 reports the core RD results based on Census data and Section 5 provides additional results and extensive robustness checks. Section 6 concludes.

1 In particular, the period following the enactment of PRWORA in the US coincided with a historic economic boom, making it difficult to determine how much of the decline in caseloads was due to the economic upturn or to the reform (Schoeni and Blank, 2000). Lemieux and Milligan (2008) actually find that commonly used difference-in-differences estimates 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, yet they must address a similar difficulty pertaining to the existence of confounding policy reforms. That is, double difference approaches must disentangle the policy effect under study from other policy changes possibly occurring at the same time, while RD analyses must verify if other policies can possibly generate similar discontinuities. We discuss the latter issue in much detail in this study.

2 According to Moffitt (2002), the major methodological problem with estimates of the labor supply impact of the AFDC is that they are not based on any data in which AFDC was literally absent. They are obtained from cross-state variation in maximum benefit levels, and the total impact is a mere extrapolation based on these estimates. As for time variation, the repeal of the AFDC was also accompanied by many other policy changes. There are few attempts to disentangle their relative effects, and Meyer and Rosenbaum (2001) are a notable exception.

3 These authors exploit the fact that prior to 1989, in Quebec, childless recipients under 30 received much lower benefits than older recipients. They find strong evidence that more generous transfers reduce employment.

4 During the period of transition from school to work, young entrants into the labor market go through a sequence of temporary jobs, unemployment spells or training periods. This period is particularly long in France, and there is evidence that it is only at the end of their twenties that most workers have stable employment relationships (Magnac, 2000).
2. Background

2.1. Transfer programs and labor supply

2.1.1. The labor supply of single individuals

Despite the large increase in the number of single individuals over the last few decades, the labor supply behavior of single women and men has received relatively little attention. This is especially true when compared to the vast literature on the labor supply of married women and, to a lesser extent, married men (surveyed in Blundell and MaCurdy, 1999).\(^5\) Single mothers have also received greater coverage, but this literature has typically been focused on the effects of tax credits (EITC and WFTC) or welfare programs (AFDC and TANF) on their labor supply (see the survey of Hotz and Scholz, 2003). However, lone parents comprise only a small fraction of all single individuals (even if this group is important for welfare considerations).

One reason for the lack of evidence concerning childless single individuals is the fact that most of the welfare programs in the US and the UK, and hence most of the policy changes that could be used to measure behavior, concern families with children. Childless singles have in fact often been used as control groups to evaluate net responses by single mothers to, for instance, the EITC (Eissa and Liebman, 1996). To our knowledge, only a few studies report estimates for singles. Several studies are based on the estimation of structural models which account for complete tax-benefit systems. Labor supply responsiveness is often measured using (hour or participation) wage elasticities. For a comparison with childless singles, note that elasticities for married women and single mothers are usually larger than .5 while elasticities for married men are often close to zero or even negative (Blundell and MaCurdy, 1999). For Italy, the Netherlands and Germany, Aaberge et al. (2002), Euvals and Van Soest (1999) and Bargain et al. (2009) report wage elasticities for childless single individuals of around .08, .03—.18 and .1—.2 respectively. Bishop et al. (2009) study all single women over a long period (1979–2003), using a simple estimation of hours and participation on repeated cross-sections. Their study reports small elasticities (at least compared to typical estimates for married women) and, more specifically, a significant decline in hours wage elasticities for childless women (from .24 to .13). The limited evidence available thus points to a small labor supply responsiveness of childless single individuals, at least compared to married women and single mothers.

Yet, these studies often ignore the large heterogeneity that may exist among single people. Interestingly, a few studies based on structural models with taxation show that low-educated single men are significantly responsive to financial incentives. Such evidence is provided by Meghir and Phillips (2008) for the UK and Aaberge et al. (2002) for Italy. The former study reports a participation wage elasticity of .27 for unskilled single men and of zero for those with college education. The latter study reports participation elasticities as high as .5 for single men in the lower part of the income distribution and almost zero higher up. These findings are particularly interesting in the context of our study.

2.1.2. Labor supply in France and research on the RMI

Evidence about labor supply in France has essentially relied on the estimation of structural models and, as for other countries, has focused mainly on married women and single mothers. Using the Hausman model accounting for non-linear taxation and nonparticipation, Blundell and Laisney (1988) report wage and income elasticities equal to 2 and −0.7 respectively at the sample mean. Such large elasticities have been questioned in subsequent studies. Accounting for taxes and benefits in a reconverxified budget set, Bourguignon and Magnac (1990) find wage and income elasticities that range, depending on the specification used, from 0.05 to 1 and from −0.3 to −0.2 respectively. More recently, authors have relied on discrete-choice models accounting for the full complexity of tax-benefit systems and, notably, the budget set nonconvexity introduced by the RMI. In particular, Laroque and Salané (2001) and Choné et al. (2004), using a sub-set of families with children find relatively large participation elasticities for married women (close to 1), which are smaller for single women (0.36). Donni and Moreau (2007) find very moderate elasticities for working couples.

Only a few papers have used tax-benefit changes to evaluate the responsiveness of the labor force. Stancanelli (2008) use a difference-in-difference approach to capture the labor market impact of a modest tax credit implemented in 2001. Carbonnier (2008) uses time changes in the income tax schedule to assess behavioral responses and changes in the tax base. González (2008) studies a social transfer payment targeted at single mothers with very young children and the effect of a reform allowing eligible parents to cumulate benefits and labor earnings for a limited period of time. Piketty (1998) studies the reform of the “Allocation Parentale d’Education” (APE), a replacement income (60% of the net minimum wage) offered to mothers of three who leave the labor market until their youngest child reaches the age of 3. He finds that the extension of the scheme to parents of two children results in a sharp drop in the participation rate of newly eligible women, indicating a relatively elastic labor supply of women in couples with young children. More specifically for the RMI, most of the evidence has been provided using structural labor supply models.\(^6\) Laroque and Salané (2001) report potentially high disincentive effects among married women and single mothers due to the RMI. Gurgand and Margolis (2008) estimate monetary incentives for a representative sample of RMI recipients. Although they find that potential gains to work are small on average, their conclusions tend to minimize the inactivity trap explanation, except for single mothers. Trannoy and coauthors (see Hagneré and Trannoy, 2001, and references therein) evaluate several changes in RMI law, and Terracol (2008) studies the impact of the RMI on the hazard rate out of unemployment.

2.2. Institutional background

2.2.1. Social assistance in France

The French welfare system is structured in such a way that the RMI acts as a ‘last resort’ benefit for those who are ineligible for (or have exhausted their right to) other benefits. Young workers are especially concerned since their contribution spells to unemployment benefits may be too short. As a result, a majority of RMI recipients are young and single.\(^7\) For the long-term unemployed who have exhausted their entitlement to these benefits, a specific means-tested unemployment allowance (“Allocation de Solidarité Spécifique, ASS”) can be claimed for 2 years under certain conditions. For lone parents, a specific minimum income (“Allocation de Parent Isolé, API”) is available for 1 year or until the last child reaches the age of 3 — then the RMI takes over. The API is calculated in the same way as the RMI but the maximum theoretical amount is slightly larger.\(^8\)

\(^5\) Note that several studies focus on men, independently of their marital status, using grouped data estimations of the correlation between hours/participation and wages over a long period to address the problem of measurement error in hourly wages. Results for the US are not inconsistent with zero elasticities for men overall and negative elasticities for married men (Pencavel, 2002; Devereux, 2003).

\(^6\) We are not aware of evaluation based on any quasi-experimental approaches that could validate past results. The main difficulty has probably been the lack of radical changes in the RMI structure, over time or between demographic groups, that could be exploited.

\(^7\) The population of RMI recipients comprises 38% of single men, 20% of single women, 25% of lone parents and 17% of couples. Around 53% of all recipients are under 39 and 20% over 50 (DARES, 2001).

\(^8\) Around 170,000 single mothers receive the API and 250,000 receive the RMI. Note that the cost of the RMI, API and ASS programs in 1999 was 0.38%, 0.03% and 0.14% of GDP and targeted 3.5%, 0.5% and 1.5% of the working-age population respectively (DARES, 2001). In comparison, these figures were 0.17%, 0.34%, 0.23% and 1.6%, 13.6%, 4.7% for the TANF, EITC and Foodstamps respectively (Moffitt, 2002).
The RMI can be claimed by any French resident, aged at least 25 and not in education. It is paid at the household level according to the formula:

$$\max(0, G(n) - tY)$$

where the maximum amount $G(n)$ depends on household size $n$ according to an explicit equivalence scale (e.g., a childless couple or a single mother with a child receive 50% more than a childless single individual). The means-test depends on total household resources $Y$, which include all incomes after taxes and social security contributions. It also includes all other social/family benefits (e.g., child benefits), except housing subsidies, which are replaced by a lump-sum amount ($\textit{forfait logement}$), representing between 12 and 17% of the maximum amount $G(n)$. For RMI recipients who have just taken up a job, it is possible to cumulate earnings and some RMI for a short period. More precisely, the earnings top-up program (intérêtissement policy) introduced in 1997 sets the benefit reduction rate $t$ at 50% for the first 750 h worked after resuming activity. After this period, this rate $t$ becomes 100%.

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. These benefits have different schedules that depend on the rent or the interest paid, the size of the dwelling, taxable income and the number of children in the household. RMI recipients are also entitled to additional benefits, including a full exemption from the local residence tax ($\textit{Taxe d'Habitation}$), access to free universal healthcare insurance ($\textit{Couverture Médicale Universelle}$) and lower fares on public transport. Although entitlement to RMI is, in principle, conditional on an “integration” agreement ($\textit{Contrat d'Insertion}$), in practice it does not include any obligation to actively seek work.

The RMI significantly reduces the extent of poverty in France (it lifts a single person with no resources up to 91% of the poverty line, calculated as half the median of equivalized income, and a single parent household up to 93% of this line). Yet, it has also been accused of fostering welfare dependency and maintaining low-wage households in a state of under-employment. In recent years, the number of recipients has hovered around the one million mark and the scheme involves more than 3 million people when recipients’ dependents are included. The extent of voluntary sub-employment caused by the RMI has never been assessed exactly, due to the lack of proper information on households’ true ability to work, but also because of changing economic conditions and the role of the RMI as an automatic stabilizer. In fact, the number of RMI recipients quickly expanded after its introduction in 1989. This reflects the downturn in the economic situation during the early 1990s, but also the stricter rules governing unemployment insurance following its 1992–93 reform and the increased generosity of the RMI scheme. The economic recovery of the 1997–2001 period was characterized by declining unemployment, yet the number of RMI recipients decreased with a delay and only temporarily after reaching the one million mark in 1999. On the demand side, RMI status is negatively perceived by employers so that recipients were among the last to re-enter the labor market during upturns. On the supply side, it is possible that claiming behavior changed in the late 1990s due to better information about the program, lower program participation costs and lower social stigma associated with the claim. Some studies indeed point toward a self-reinforcing process whereby the number of claimants affects the perceived normality of the claim and the propensity to take-up in the following years (see Gustafsson, 2002). The non-take-up of the RMI in France is estimated at 30% by Terracol (2002), who indicates that stigma and information are the two main causes of less than complete takeup.

2.2.2. Education system

A key variable in the present study is the education level, hence a brief description of the system is useful. In France, pupils usually attend 4 years in the first cycle of highschool (i.e., junior school or lower secondary level), starting at age 11, then either 3 years in the secondary cycle of highschool (i.e., upper secondary level) or 2 years in vocational schools. Education is compulsory until age 16, which is generally the last year of junior school or the first year of highschool. Pupils with severe difficulties may however remain in junior school for several years and drop out before the last year. The end of junior highschool is concluded with a national exam leading to the Diplôme National du Brevet (BEPC). This diploma is of little or no value today and is not even necessary to continue to the second stage of highschool. Basic vocational/technical schools lead to a vocational qualification certificate (Certificat d'aptitude professionnelle, CAP, and Brevet d'Études Professionnelles, BEP), while the second cycle of highschool leads to a final national examination for the highschool diploma (Baccalauréat). This diploma is usually required to enrol in undergraduate university studies or advanced vocational/technical college.

Here, we focus primarily on those who leave highschool at any point without obtaining any qualification. These are essentially comparable to highschool dropouts in the US/Canadian terminology (HS dropouts hereafter). In addition, we identify in the data (i) those with a basic vocational diploma (BEP, CAP), (ii) those with a HS diploma, (iii) those with a first college degree or an advanced vocational degree (either of which normally takes 2 years) and (iv) those with higher degrees from universities or business/engineer “Grandes Écoles” (the French Ivy League institutions).

3. An informal look at the RMI effect using the LFS

We first provide an informal assessment of the potential effect of the RMI on uneducated single men using the French Labor Force Survey (LFS), 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 large representative sample of the French population aged 15 or over (sampling rate: 1/300), providing information on employment, income, education and demographics. Since there is no income information in the Census data used in the subsequent RD analysis, we first extract from the LFS some summary statistics to evaluate the financial incentive to work of different education groups. For consistency with the RD analysis, we focus on the year 1999 and on single men aged 25–30. Table A.1 in the Appendix A reports median wage rates for those who work as well as relative gains to work (see the much more extensive analysis of Gargaud and Margolis, 2008, on this aspect). HS dropouts have the lowest financial incentives to work, yet there is not much difference between them and those holding a basic vocational diploma or a HS diploma because (i) we focus on young workers (and those with more education naturally have less experience), (ii) wages are usually lower bounded by the minimum income and HS dropouts are highly concentrated in minimum wage jobs. For low-wage workers, the relative gain of taking up a part-time job is close to zero. It is larger for a full-time job, yet maybe not enough, in absolute terms, to make work pay if costs of work are large. The gains become more significant at higher educational attainment, even if it takes a college degree, in this age group, to double disposable income compared to social transfers. Fig. 1 illustrates the potential impact of the RMI on budget constraints for hypothetical single individuals with the lowest (HS
Indeed, a beneﬁt for the RMI upon turning 25 years old. Yet, it is unlikely that a young worker will reduce his working time for 4 months (provided that work hours can be adjusted, at the extensive (participation) margin, assuming that they are not eligible for RMI).

Now using all years of LFS, we plot the long-term employment trends of childless single men around the discontinuity at age 25. We distinguish between two age groups, 20–24 and 25–30, and two education groups, HS dropouts and those with basic education (basic vocational diploma or HS diploma). Results are reported on the l.h.s. graph of Fig. 2. The most striking observation is that all groups follow the business cycle, except HS dropouts aged 25–30. The two younger groups (dashed lines) show larger ﬂuctuations, with employment falling more rapidly in the downturn of the early 1990s and picking up faster in the late 1990s. The two older groups (solid lines) follow each other in the ﬁrst half, with a gradual decline in employment, but HS dropouts continue to decline in the late 1990s, i.e., when the RMI expands.¹¹ These trends could suggest that (i) the availability of the RMI has encouraged uneducated singles into a more permanent level of under-employment, (ii) even basic (vocational or general) qualiﬁcations would limit the RMI trap. Note that this interpretation tends to downplay the importance of gains to work since these are also relatively low for workers with a vocational degree. The r.h.s. graph compares these trends with employment levels of single mothers using the same age/education groups. In that case, both under-25 and over-25 HS dropouts show a marked decline over the whole period. Given that single mothers are not subject to the age clause for RMI eligibility, this reinforces the suspicion that the RMI affects men over 25.

Finally, we exploit the fact that the LFS is a rotating panel, each household remaining in the survey for three consecutive years with one third being replaced each year. For years 1990–2002, we track people who take up the RMI at some stage during their observation. To keep the largest possible sample, we include men and women from all educational groups and family types. However, we only keep individuals who are observed at least twice and for whom we have complete information on employment and RMI status. This represents an average attrition rate of 24% over the period 1990–2002. Fig. 3 ﬁrst reports the number of RMI recipients (l.h.s. axis), which peaks at age 25–27, then decreases gradually with age and becomes relatively stable after 35. Furthermore, Fig. 3 shows the proportion of “new RMI recipients” (as a percentage of all recipients in each age cell, r.h.s. axis). It differentiates between those who were unemployed or employed in the year prior to entering the program.¹² Firstly, the “new entrants from unemployment” curve somewhat mirrors the concave age-earnings proﬁle for France. This conveys that as the opportunity cost of staying out of the labor market to get the RMI increases, the incentive to do so decreases and thus fewer unemployed people choose that option. This point justiﬁes the RD approach in that it suggests that the latent enrollment rate at age 24 would be even higher than at age 25 but that the eligibility constraint is particularly binding.¹³ Importantly for policy implications, it also suggests that the response measured by the RD analysis should be taken as a lower bound of the potential employment effect for under 25, should the RMI be extended to them. Secondly, the “new entrants from unemployment” curve reaches a peak at age 25 then becomes relatively ﬂat once the backlog of workers uncovered by unemployment beneﬁts is cleared and the ﬂow into RMI stabilizes to “regular” levels. This is

¹⁰ Arguably, point (iii) ignores the intérressement top-up period discussed earlier. Indeed, a beneﬁt reduction rate t of 50% could encourage a reduction in hours rather than a complete withdrawal from the labor market for someone who becomes eligible for the RMI upon turning 25 years old. Yet, it is unlikely that a young worker will ﬁrst reduce his working time for 4 months (provided that work hours can be adjusted, which is unlikely) before leaving the labor market. In 1999, the top-up measure concerned only 8% of all RMI recipients, which corresponds to the proportion of RMI recipients holding a job in the LFS of that year. Gurgand and Margolis (2008) show that the top-up measure has, in fact, very little effect on work incentives. This is one of the reasons behind the Aubry Law passed in 1998, which extended this period to 1 year, with a rate t brought down to zero for the ﬁrst 3 months. This measure is unlikely to have taken effect when Census data were collected (March 1999) and hence to affect our results. It is analyzed by Hagnére and Trannoy (2001). González (2008) studies a similar reform of the API for single mothers, with a longer top-up period, showing that positive labor supply effects become signiﬁcant 4 years after the 1998 reform.

¹¹ Younger workers are usually distinguished by a lower employment rate than older workers in the same education group, in part due to the high universal minimum wage, which encourages the exclusion of the youngest workers (see Abowd et al., 2000). While this is true for both education groups in the mid 1980s, it no longer holds at the end of the period for HS dropouts, as the 25–30 exhibit lower employment levels than the 20–24 year olds.

¹² Naturally, movements in and out of the RMI status may be more frequent, but the interval provided in the LFS panel does not allow for a narrower time decomposition. Note also that since we follow people for 3 years at most, each individual can be classiﬁed as a new RMI recipient once at most.

¹³ We thank an anonymous referee for highlighting this important point.
understandable for new entries from unemployment. However, the lower line of Fig. 3 (black diamonds) also shows a maximum at age 25 for new entries coming from employment (a peak around 12% of all recipients at that age, then stabilization around 6% for other age levels). Yet, there is no reason for labor demand shocks to affect those aged 25 significantly more than other age groups. Thus, these results suggest that some supply-side effects are at work and materialize when the RMI becomes available at age 25. That is, the 12% reported above is likely to comprise of 6 percentage points corresponding to people affected by a recent unemployment shock (the "natural" RMI inflow observed at older ages) and 6 points corresponding to people who stop working to take up the RMI at age 25. These results are suggestive of a RMI effect but suffer from the small sample size of the LFS and the fact that RMI recipients are under-represented in the survey.

4. A regression discontinuity analysis

4.1. Census data and selection

The RD analysis that follows is based on French Census Data for years 1982 and 1999, whose coverage was universal. 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. The 1999 Census provides information 10 years after the introduction of the RMI while the 1982 Census is used as a control in what follows. We exclude all those who are still at school or in some form of education, military or retired. In the remaining sample, we can distinguish “active person holding a job” from “unemployed”, the definition of which is broader than the ILO definition (it includes job seekers and those who explicitly declared that they are not looking for work).

Our main RD analysis focuses on single men without children. This selection has been motivated before – notably because childless single individuals represent the main group of RMI claimants – but deserves more explanation. Admittedly, we cannot expect clear interpretations from results based on individuals living in a couple, since the partner may work and since the joint labor supply decision in couples is generally a more complicated problem (see the results and tentative discussion in Section 5.4). The selection of individuals without children is obviously due to the fact that a parent is eligible for the RMI regardless of age. We carefully investigate the possible selection bias implied by focusing on childless single men in the robustness checks of Section 5.4. Focusing mainly on men is justified by the fact that several factors complicate the analysis in the case of women. Firstly, a substantially larger fraction of women than men have children and, hence, are not subject to the age restriction.14 Secondly, women are more at risk of being affected by the possible selection bias engendered by choosing only childless single individuals. They are also more concerned by potential fertility responses to financial incentives. We discuss these and other possible manipulation effects in Section 5.4 (but provide nonetheless some results for single women).

Finally, we focus mainly on HS dropouts. This was already our target group in the LFS analysis, but additional explanations are useful. Young uneducated men have the lowest financial gains to work in the short term but may also have weaker attachment to the labor market, with higher job search costs and lower earnings prospects. Table A.1 (Appendix A) shows that they represent 22% of the population of young single men aged 25–30 but are over-represented among single male RMI recipients in this age range (they account for 52% of this group). The group of HS dropouts thus appears most “at risk” and is the focus of our attention in what follows. We shall nonetheless examine other education groups (Section 5.4). Note also that our main RD analysis focuses on the group aged 20–35. Including individuals under the age of 20 could lead to less robust results, as we would encounter fewer and fewer people in each age cell. This is the case even with HS dropouts, since not all of them leave the education system at age 16 (some may repeat one or more years of school). The upper bound (35 years of age) is arbitrarily chosen, but we perform sensitivity analysis on the age window as explained below. Table A.2 in the Appendix A reports sample size by age. It shows that there are between 85,000 and 105,000 observations for each year of age around 25 (all men) and between 6000 and 7000 for the selection of single male HS dropouts.

14 According to 1999 Census data, we find that among 25-years-old HS dropouts, 55.8% of all women had and lived with children, compared to 22.4% of all men. When focusing on singles in this education group, figures decrease to 30.2% for women and 0.8% for men. This reflects the fact that women are much more likely than men to be single parents and to have their children at a younger age. This is especially the case among HS dropouts. According to Davie and Mazzuc (2010), the fertility rate of all women in 1999 was around 1.8 on average but as high as 2.4 for women with no qualification. At age 25, it was 0.9 on average but 1.6 in this education group. The mean age of mothers at first birth was 27.5 but only 24.2 for those with no qualification. The mean age of first marriage was 29.8 for men and 27.7 for women.

4.2. Empirical approach

Using Census data, we exploit the age discontinuity in the RMI program. Consider the regression model:

$$Y_{ia} = \beta_0 + \beta_1 \text{TREAT}_{ia} + \delta(a) + \epsilon_{ia}$$  (1)

where \(Y_{ia}\) is an outcome variable for individual \(i\) of age \(a\). The main variable of interest is employment, but we also provide results for work hours and for particular forms of employment (notably state-subsidized contracts and fixed-term contracts). The effect of age (the forcing variable) on the outcome variable is captured by the function \(\delta(a)\), while \(\text{TREAT}_{ia}\) is a treatment dummy that takes the value 1 if the individual is aged 25 or above and zero otherwise. In this way, we can estimate the effect \(\beta_1\) of the treatment (the potential availability of the RMI) on the outcome variable. The key identification assumption of the RD approach is that \(\delta(a)\) is a continuous function. Under this assumption, the treatment effect \(\beta_1\) 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 in our case).15 The main argument for assuming that \(\delta(a)\) is a smooth function is that employment or work hours typically exhibit regular age profiles. Function \(\delta(a)\) should certainly be flexible enough to accommodate nonlinearities in the 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. Importantly, we verify hereafter that no other discontinuity embedded in fiscal or labor market policies could cause a negative employment effect at that particular age (Sections 5.1 and 5.2). Another potential issue is non-random selection in case fertility and living arrangement decisions are endogenous. In fact, as long as these selection biases are a smooth function of age, they will be captured by the function \(\delta(a)\) and the RD approach will remain valid. We present evidence supporting this view in Section 5.4.

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 the forcing variable (age), this is a “sharp” RD design. In our main analysis, we do not use age in days. Indeed, it is not clear when the potential labor supply response would occur after turning 25. Individuals who were working before their 25th birthday may not be aware that the RMI is means-tested on the income earned during the three months prior to the claim. Also, and more importantly, age cells obtained when age is measured in days may be aware that the RMI is means-tested on the income earned during the three months prior to the claim. Also, and more importantly, age cells obtained when age is measured in days may be too small for any meaningful analysis and would display a very erratic pattern. Rather, in order to reduce the amount of noise, our parametric analysis makes use of age in years and quarters. The problem is thus one where the forcing variable is discrete and all the information is summarized in the age-specific means of the variables, a situation extensively discussed in Lee and Card (2008). In this way, estimates of Eq. (1) based on individual data are identical to estimates of the age-cell version of the model:

$$Y_a = \beta_0 + \beta_1 \text{TREAT}_a + \delta(a) + \epsilon_a$$  (2)

weighted by the number of observations by age group. In the discrete case, the treatment effect is not identified non-parametrically (cf. Lee and Card, 2008). Indeed, 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. We use a variety of polynomial forms, including standard linear, quadratic, and cubic

15 We would like to include additional control variables in our regressions but the target group is already homogenous in terms of education level and marital status so there is not much left to be used. The region of residence, which could proxy local employment opportunities, is not available in the 1/4 Census.
functions, as well as linear and quadratic splines (separate regressions on both sides of the discontinuity). This approach 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 also present estimates of the linear spline model for an increasingly small window around age 25 as a further robustness check.

Two goodness-of-fit statistics have been suggested in this context. Denote \( J \) the number of age years/quarters and \( K \) the number of parameters estimated in function \( \hat{\beta}(\cdot) \). Since the outcome variable \( Y \) is a cell mean, its sampling variance \( V_0 \) can be easily computed. Under the assumption that specification (2) is correct, the only source of error in the model should be the sampling error. This assumption can be tested using:

\[
GOF_1 = \sum_{i} \left( \frac{\hat{e}_i^2}{V_0} \right),
\]

as suggested by Lemieux and Milligan (2008). Under the null hypothesis that model (2) is the true model, and assuming the normality of \( \hat{e}_i \), the \( GOF_1 \) should follow a \( \chi^2 \) distribution with \( J-K \) degrees of freedom. Lee and Card (2008) also suggest comparing the parametric models to an unrestricted model. When the forcing variable is discrete, a natural way of testing the parametric specifications relies on the standard statistic:

\[
GOF_2 = \frac{(\text{ESS}_R - \text{ESS}_{(J-K)}) / (J-K)}{\text{ESS}_{\text{Ur}} / (N-J)},
\]

where \( \text{ESS}_R \) is the estimated error sum of squares of our parametric model and \( \text{ESS}_{\text{Ur}} \) the estimated error sum of squares of a model where a full set of dummy variables for the \( J \) values of the forcing variable (age) is included. In the latter, unrestricted model, the fitted regression corresponds to the mean outcome in each cell. Under normality and heteroskedasticity of \( \hat{e}_i \), \( GOF_2 \) follows a \( F(J-K,N-J) \) distribution where \( N \) is the number of observations. This is not a definitive test — Lee and Card (2008) note that rejection of a given polynomial form \( \hat{\beta}(\cdot) \) does not necessarily imply that the corresponding estimate of the effect is inconsistent. Yet, confidence in a chosen specification increases if it cannot be rejected by this test (see the general discussion in Lee and Lemieux, 2010).

Complementary to the parametric approach, we treat age in months as continuous in order to perform nonparametric estimations. We use local linear regressions, advocated to reduce the bias inherent to nonparametric regressions at boundary points (Hahn et al., 2001).

We have experimented with different types of kernel functions including the triangular kernel, known to be optimal for estimating local linear regressions at the boundary (Fan and Gijbels, 1996), and the rectangular kernel with various bandwidths. Using a variety of bandwidths is important in order to balance precision and bias. Standard errors are obtained by bootstrapping.

4.3 Regression discontinuity: graphical results

As argued by Imbens and Lemieux (2008), the graphical representation of the discontinuity should be an integral part of any RD analysis. No evidence of that sort would cast serious doubt on the more sophisticated statistical analysis that follows. Hence, before looking at statistical results, we plot in Fig. 4 the raw employment rates by age, along with the 95% level confidence bounds, using our selection of single male HS dropouts drawn from the 1999 Census. For age in years (l.h.s. graph), results suggest that employment drops sharply at age 25, i.e., when people become eligible for the RMI. With age in quarters (r.h.s. graph), there is more noise due to smaller age cells, as expected, yet the drop in the outcome variable at the 25-year old cutoff is confirmed. Importantly, this drop is unusually large compared to other bumps in the curve away from that threshold. The steep upward trend in employment rates before the discontinuity is in line with the widely accepted theory that the employment rate is a concave function of age. This also corresponds to greater discrimination against the youngest workers and their particularly slow integration into the labor market in France. This pattern of increasing employment before 25 is also observed for other HS dropouts (cf., results for men in other demographic groups, lone mothers and single women in the following Sections). The relatively flat trend observed for the segment 25–35 is driven by the nature of the group, i.e., individuals without children. As discussed below (and in Lemieux and Milligan, 2008), these are negatively selected in terms of their labor market outcomes. This pattern, with employment increasing with age, then flattening rapidly, is found for many different groups not affected by the RMI, notably those with higher education or childless single men prior to the introduction of the RMI, as shown below.

4.4 Regression discontinuity estimates

We now turn to the statistical results presented in the upper part of Table 1. We first focus on the estimated treatment effect on the employment rates of our group of interest in 1999, i.e., single male HS dropouts aged 20–35. That is, we provide a RD estimate of coefficient \( \beta_1 \) that captures the drop at age 25 previously observed in the graphs.

![Fig. 4. Employment rate of single male HS dropouts (Census 1999).](image-url)
Table 1

RD estimates of the effect of RMI on labor supply, single male HS dropouts, 1999.

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Emp. rate</th>
<th>Weekly hours</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean value</td>
<td>0.68</td>
<td>25.5</td>
</tr>
<tr>
<td>Value at age 24</td>
<td>0.72</td>
<td>27.0</td>
</tr>
</tbody>
</table>

Age in: years Quarters Years Quarters

Polynomial specification for age:

| Linear       | −0.027    | −0.028**  | −0.081   | −0.019  |
| Quadratic    | −0.007**  | 0.067**   | 2.601**  | 2.700** |
| Cubic        |           | 0.065***  | 2.402*** | 2.514***|
| Linear spline| 0.049***   | 0.049***  | 1.874*** | 2.300***|
| Quadratic spline | 0.067*** | 0.068***  | 2.327*** | 2.405***|

Goodness of fit statistic 1 (p-value)

| Quadratic | 0.09       | 0.75       | 0.78       | 0.55    |
| Cubic     | 0.88       | 0.73       | 0.78       | 0.55    |
| Linear spline | 0.96     | 0.80       | 0.96       | 0.71    |
| Quadratic spline | 0.81    | 0.68       | 0.65       | 0.50    |

Goodness of fit statistic 2 (p-value)

| Quadratic | 0.98       | 0.80       | 0.88       | 0.61    |
| Cubic     | 0.97       | 0.77       | 0.89       | 0.60    |
| Linear spline | 1.00    | 0.84       | 1.00       | 0.75    |
| Quadratic spline | 0.92   | 0.73       | 0.77       | 0.55    |

Linear spline, with age window widths:

| ± 5 years around 25 | 0.052*** | 0.059*** | 1.953*** | 2.214*** |
| ± 4 years around 25 | 0.060*** | 0.069*** | 2.146*** | 2.504*** |
| ± 3 years around 25 | 0.065*** | 0.072**  | 2.259*** | 2.551*** |
| ± 2 years around 25 | 0.067 (−) | 0.079 (−) | 2.157 (−) | 2.804 (−) |

Local linear regressions, with bandwidth:

| 24 months (triang. kernel) | 0.080** | 3.023** |
| 12 months (rect. kernel)   | 0.071** | 2.983** |
| 24 months (rect. kernel)   | 0.088** | 3.193** |
| 48 months (rect. kernel)   | 0.078** | 2.935** |

This represents between 7 and 10% of the average working time (27 h/week), which is similar to the effect on employment probability. Hence, this suggests that basically all of the impact of the RMI on labor supply happens at the extensive margin, which is in line with our prediction in Section 2.2 (and supports previous findings about the very small impact of the intérêtissement top-up period). Goodness-of-fit measures suggest that all models fit the data very well; the two measures GOF1 and GOF2 lead to very similar results. In Table 1, the p-values are reported for all flexible models and show that we cannot reject these models at reasonable significance levels.

We present a few additional regressions in the lower panel of Table 1. First of all, in order to negate the effect of variations at the extremes of the age range 20–35, we suggest relying only on observations that are increasingly close to the discontinuity point. We do so using the linear spline model. For instance, the results for a “± 4 years” window correspond to an age range 21–29. The smallest window we can use with the linear spline model is “± 2 years” since we need at least two observations on each side to identify separate regression lines (the R2 is equal to one in this case). Results are very robust to the age window used in the estimation, with employment effects in the range [− 7.9, − 5.2] and hours effects in the range [− 2.8, − 1.9]. It is reassuring to see that the magnitude of these effects is not very different – and not actually statistically different – from those obtained with the 20–35 age window in the main regressions. We also run local linear regressions for the main group (age 20–35) using age in months. These nonparametric estimates provide an additional confidence in the robustness of the results obtained with the polynomial models. In Table 1, we simply report regressions obtained with a triangular kernel and a rectangular kernel for these different bandwidths corresponding to 1, 2 and 4 years. Estimates are slightly larger than – but not statistically different from – those obtained with the parametric linear spline model. They are much more precisely estimated due to the noise in age reported in months. A larger bandwidth decreases the variance because we include more observations and take the separate effect of months on outcomes into account (yet it should also increase the inaccuracy of the estimated effect since we move further away from the discontinuity).

4.5. Higher-educated men and comparisons with the literature

HS dropouts represent the obvious group of interest in our study. For a complete picture of the potential RMI trap, we must nonetheless check whether other education levels are affected. Fig. 5 reports employment patterns for those with a vocational qualification, a HS diploma or (any) university degree. All these groups show much higher employment levels (at age 24: 84%, 88% and 92% respectively) than HS dropouts (72%). The reasons behind such a sharp difference in employment levels may consistently explain why no RMI effect can be seen in Fig. 5 for these higher educated groups. Higher skills may limit some extent the risk of rationing due to the minimum wage or other forms of unemployment while facilitating employment via signaling effects. Educated adults may also (i) have unobserved characteristics that explain both higher educational achievement and a stronger attachment to the labor market; (ii) be characterized by higher returns to seniority, and hence higher (dynamic) incentives to stay in the labor market (see the evidence in Beffy et al., 2006). When attempting to explain why HS dropouts are, to some extent, responsive to the RMI while other education groups are not, we thus surmise that these factors are more important than financial gains to work. Indeed, those with a basic vocational diploma, in particular, show similarly small gains while their participation rate is much higher and their age-employment profile shows no response to the RMI. Other studies note that those who hold a CAP or BEP, given the professional nature of these qualifications, are in fact better integrated into the labor market (sometimes even compared to those with a HS diploma), have higher returns to experience than educated workers (Beffy et al., 2006) and have much lower probabilities of being in...
a precarious form of employment (state-subsidized or short-term contracts).

Overall, these results tend to support the (limited) evidence on the labor supply of single men and, in particular, the fact that their elasticity is usually close to zero but significant for those with low-education and, hence, low potential earnings (Aaberge et al., 2002; Meghir and Phillips, 2008). The welfare implications of these results are important, as discussed in the Introduction and in Section 2.1. Next, it is instructive to compare our results to the literature on welfare programs, notably for Canada and the US. A study very close to ours is the RD analysis of Lemieux and Milligan (2008). In fact, our results are strikingly similar to theirs for the target group. We find a 7−10% response in labor market participation associated with a 162% increase in benefit (moving from point B to point A in Fig. 1), hence an elasticity of labor market participation to benefit levels between −.06 and −.04. Lemieux and Milligan find a 6−9% response to a 175% increase in benefits, i.e., an elasticity between −.05 and −.03. Both studies point to a relatively modest behavioral effect for uneducated men. We further compare our results with the effects of welfare programs in the US, and more precisely with changes in maximum amounts – noted G in the literature and in the RMI formula in Section 2.2 – for a consistent comparison with the income effect generated by the RMI. Maybe the main issue when attempting such a comparison is the fact that the benefit reduction rate of other programs is lower than 100% (e.g., 30% for Foodstamp). Another difficulty is the lifetime eligibility conditions attached to policies like TANF or the work condition in transfers like the EITC. For these reasons, it makes sense to mainly focus on the (pre-1996) AFDC, which is the closest program to the RMI in this respect (see Moffitt, 2002). An additional difference concerns the demographic groups under investigation in other studies: there is not much we can do about it since, as previously discussed, most of the available evidence for US welfare reforms concern households with children. Comparisons are nonetheless interesting. First, Hoynes (1996) estimates the labor supply of married couples and simulates, among other things, a 20% increase in the AFDC maximum amount. She finds that the employment rate of married men (women) would decrease by 0.23% (1.21%), which yields an elasticity with respect to maximum benefit G of −.01 (−.06). These magnitudes are close enough to ours to be able to confirm that the responsiveness of uneducated single men is somewhere between that of married men and married women, and probably closer to the latter. Second, Meyer and Rosenbaum (2001) report that a 10% cut in the maximum benefit G for AFDC (holding other parameters constant) increases the employment rate of single mothers by about 1.5%, hence an elasticity of .15 which is substantially larger than for single men and consistent with larger responses in this group. Third, the negative income tax (NIT) experiment is noteworthy. It provided a generous transfer to low-income families (guaranteed levels ranged from 50 to 125% of the poverty level) with benefit reduction rates from 30 to 70%. According to Robbins (1985), its introduction decreased the labor supply of married men by 3.5% and that of married women and single mothers by 22% and 16% respectively. Our results for single men are once again between these two categories, which reinforces previous conclusions.

5. Robustness checks and additional results

In this section, we provide an extensive robustness analysis and several important additional results.

5.1. Checking alternative explanations for the discontinuity

First of all, we review all institutional features that could also be responsible for a discontinuity in employment patterns at age 25. We investigate several areas: tax-benefit policies, parents’ legal obligations to financially support their children and labor market policies. These checks are supported by additional RD results in Section 5.2 concerning particular forms of employment for youths.
5.1.1. Tax-benefit policies and parents’ legal obligations

We examine all tax-benefit policy rules for the year 1999.18 The only important issue that emerges is the possibility for parents to declare children as dependents in order to obtain (i) benefit increments or (ii) tax deductions. First, children can be treated as dependent only until age 21 in the benefit system. The only exception is the RMI itself, i.e., parents receiving the RMI obtain an increment for children aged 21–24 (an additional 20% of the maximum amount for the first child of couple, and 50% for a lone parent). Yet, this applies only if the child is a student, and hence does not concern our target group of HS dropouts. Second, tax deductions can be important. Take the case of a household composed of a couple and one child, earning EUR 25,000 per year. In 1999, the tax liability of this household, EUR 7379, is basically halved by the deduction allowed by the presence of a dependent child. Tax deductions are linked to the legal obligation of parents to financially support their children, which stops at the child’s 25th birthday. Hence children may expect a double income effect when they turn 25. Indeed, transfers received from their parents may simply decrease as this obligation stops, and this effect is accentuated by the fact that parents become poorer as they do no longer benefit from tax deductions. If leisure is a normal good, the policies cannot explain a drop in employment at age 25. On the contrary, it may actually increase the labor supply of 25 year olds so that the RMI effect we capture would be a lower bound of the actual effect. Note also that a decrease in parental transfers and the possibility of taking up the RMI at age 25 may induce changes in living arrangements. However, we find no statistical evidence of a discontinuity in cohabitation rates with parents at age 25 (the proportion of cohabitants is reported in the last column of Table A.2).19 Other changes in living arrangements, i.e., cohabitation with partners and children, receive specific attention below.

5.1.2. Labor market policies

Labor market policies targeted at young workers may affect their labor supply (by decreasing job search costs) or the labor demand if youth employment is subsidized by the state. Two main types of public interventions were in place in 1999 (see the survey by Fougré et al., 2000). First, subsidized training programs in the private sector, in the form of apprenticeship contracts or the so-called “Contrat de Qualification” (CQ, or qualifying contract), offer participants part-time work in the firm complemented by part-time education in a public training center. Labor demand could be boosted by the fact that participants are only paid at a fraction of the minimum wage (from 17 to 75%, depending on seniority), and employers are exempted from social security contributions. Secondly, subsidized public-sector jobs, paid at least the hourly minimum wage, were put forward by the government in the late 1990s, essentially the “Contrat Emploi Solidarité” (CES, or employment–solidarity contract) and the “Nouveaux Services Emplois Jeunes” (NSEJ, youth employment program in services) launched in 1997 and phased out after 2002. Importantly, these different schemes concerned youths under 26 – or even under 30 for those unemployed and without benefits or the handicapped in the case of the NSEJ – so that any break in employment figures would occur at 26 rather than 25. Another important policy concerns payroll tax subsidies for minimum wage workers in the private sector (in 1999, the main scheme was the “Contrat Initiative Emploi”), but these subsidies are defined according to wage rate levels and make no reference to age.

5.2. Precarious forms of employment

Based on the previous conclusion, we can check for other discontinuities (and notably one at age 26) in case employment rates also reflect sudden changes in labor demand when subsidized contracts terminate. For the period 1982–89, Abowd et al. (2000) find evidence that workers whose wage falls below the real minimum wage are most likely to be priced out of work if they are not covered by youth employment promotion contracts similar to those described above. That is, they estimate a sharp drop in the employment probability at age 26 and above, i.e., when apprenticeship contracts and subsidized public jobs (and the labor cost reduction that they imply) are no longer available.

According to DARES (2001), 820,000 young workers (aged 16 to 25) were employed in a state-subsidized program in 1999, including 43% in Apprenticeships and 23% in CQ, 11% in subsidized market jobs (mainly CEI) and 20% in subsidized non-market jobs (5% in CES and 15% in NSEJ). The Census data allows us to distinguish between several types of employment, in particular short-term contracts, Apprenticeship contracts (but not CQ) and subsidized public jobs (essentially CES and NSEJ). Only 14% of the Apprenticeship contracts concerned workers aged 20 or above (they represent only 0.5 – 1% of our selection at age 24–25), so we ignore them. We check the effect of the two other important programs, i.e., CES and NSEJ. The i.h.s. graph in Fig. 6 focuses on the fraction (by age) of single male HS dropouts who are employed in these subsidized public jobs. There is no evidence of a specific discontinuity at age 25 (as expected) or at age 26.20 Interestingly, the r.h.s. graph of Fig. 6 shows a significant drop in the proportion of short-term contracts at age 25.21 This is consistent with the fact that those who stop working at this age because of the RMI disincentive also have weaker attachment to the labor market and may anticipate the possibility of living on welfare (and, hence, put less effort in searching for permanent positions in their early twenties). When estimating model (2) for the outcome “short-term employment”, we find significant effects for all model specifications using age in quarters (and most specifications with age in years), with a magnitude ranging between 2.3 and 4 percentage points across the different specifications.

5.3. Falsification tests and a look at single mothers

We now conduct some falsification tests of our main results by inspecting two groups that are not affected by the RMI. The first one consists of the selection of single male HS dropouts observed in the 1982 Census, i.e., 7 years before the introduction of the RMI. The second is the group of single mothers who are, by definition, eligible for the RMI at any age. Fig. 7 (first two graphs) shows a marked contrast between these groups and our group of interest (Fig. 4). No significant discontinuity appears at age 25 for lone mothers in 1999 or for single male HS dropouts in year 1982.22 RD estimates for these two

18 The EUROMOD project provides detailed account of tax-benefit rules in the EU via a series of country reports including France. See www.iser.essex.ac.uk/research/ euromod.
19 Note also that the RMI effect is smaller when including single male HS dropouts living with their parents in our sample, yet still significant.
20 The latter result is not so surprising. Firstly, the large employment elasticities to real minimum wage variations found in Abowd et al. (2000) concerned a marginal group, i.e., those in between two minimum wage levels. Secondly, the year 1999 is characterized by one of the smallest real minimum wage growth rates of the 1980s and 1990s (+0.8%), so that any rationing that may affect those aged 26 in 1999 is certainly much more limited than in the years studied in Abowd et al. (2000).
21 In fact, the total employment drop found in our main results, around 6 percentage points, includes — 1.2 for permanent contracts, — 3 for short-term contracts, — 1 for subsidized public jobs and — 0.5 for Apprenticeship.
22 For the sake of completeness, Fig. 7 contains a third graph with lone mothers in 1982, also showing no discontinuity. Note that the RD approach cannot identify a RMI effect for single mothers, yet this group may be strongly affected by social transfers in France. Fig. 7 is suggestive: the participation rates of both single mothers and single men decrease between 1982 and 1999, affected by the depressed labor market of the early 1990s, yet participation declines much more substantially among single mothers. This may partly be due to the disincentive effects of the RMI and the API, and would be consistent high participation elasticities among women with children (see Piketty, 1998).
groups, available from the authors, show insignificant treatment effects regardless of the model specification. Hence these "control groups" tend to confirm that the effect for single men is due to the RMI (and not to other discontinuities). Note that the age-employment patterns of single men in 1982 and 1999 are similar, i.e., they show a sharp increase in their early twenties followed by a flat trend. The comparison conveys that this flattening is not due to the RMI (but to the selection of childless men, as mentioned before).

5.4. "Manipulation" effects and broader demographic groups

The question of sample selectivity, due to the fact that we consider only single men without children, is an issue if "manipulation" effects are possible. Indeed, some people may decide to (i) have children and/or (ii) live alone in order to become eligible for welfare assistance. On the first point, there is relatively weak evidence in the literature that welfare benefits actually affect fertility and single motherhood decisions (see Moffitt, 1998). It is reasonable to assume that this effect is relatively limited for single men in France, as suggested in Section 4.1, given the very small proportion of single men with children in our selection around age 25. We may however check whether men in couple households show an accelerated fertility rate below age 25 compared to the "normal" demographic trend. On the second point, manipulation effects pertaining to living arrangements with partner/children could concern adults aged 25 and above. For an unemployed couple under 25, there is no advantage to declaring one adult as single and the other one as single parent. At 25 years old, however, declaring different addresses brings in more RMI than for a couple with a child. This would imply that the change in the proportion of men living with a partner and children should be discontinuous at age 25. To check these two points, it is unfortunately difficult to find a control group that would indicate what the "normal" fertility or living arrangement patterns are. The best we could do with the available data was to perform a double comparison using year 1982 (no RMI) and educated groups, likely to be less sensitive to the RMI. Detailed results, available from the authors, show that HS dropouts under 25 have higher fertility levels than the educated group but that this is the case for both years. Results do not point to any specific fertility acceleration before 25. We also check that the proportion of HS dropout males living with a partner and children in 1999 does not change abruptly around the discontinuity point.

There is arguably a more direct and intuitive test of whether sorting or manipulation effects change the size of the selected sample in a discontinuous way on both sides of the cutoff point, potentially calling into question the exchangeability of observations on either side. It consists of examining the density of the forcing variable itself at the discontinuity point. We do not find any particular jump in the age density at 25 years old when using age in years or quarter (graphs available from the authors), which confirms the validity of the RD approach. We also implement the corresponding test as suggested by McCrary (2008) to check the continuity of age at the threshold using only the closest observations. We cannot reject the null that the density of age is the same just below and just above the discontinuity point.

Next, we examine broader demographic groups, still focusing on HS dropouts. This partly solves the selection problem but, at the same time, dilutes the employment effect found for childless single males since this group is pooled together with other household types less affected by the RMI. Fig. 8 compares the trends in employment rates for our selection (childless single males), all childless men (single and in couples) and all men. For all childless men (second graph), the employment rate is naturally higher than in our baseline selection, as married men typically tend to work more than single men, either because of a causal effect of being married or due to sorting of men into marriage. If poor labor market characteristics decrease someone’s chance of getting married, there is no particular reason for this selection to be discontinuous at age 25 (sorting would lead to a rejection of the McCrary test in that case).

We run local linear regressions of the fraction of men in each age group below and above the discontinuity point, checking whether the fraction of men predicted to be at age 25 is the same for the two regressions. We use a triangular kernel, i.e., weights on age groups in the local linear regressions linearly decline from one at the discontinuity point to zero X years/quarters away from this point. We obtain standard errors of the test by bootstrapping. For bandwidths X = 2, 3 and 5 years, the p-values are 0.57, 0.35 and 0.31 respectively. For X = 8, 12 and 20 quarters, the p-values are 0.60, 0.42 and 0.35 respectively. Note also that the McCrary test also checks for direct manipulation of the forcing variable in case people can lie about their age when claiming the RMI. This event is very unlikely since the age information is easily and systematically verified by benefit agencies. The test confirms this.

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23 While single men’s employment rates in 1982 were higher than in 1999, which is consistent with Fig. 2, we can see that the average employment rate of the 25–30 years old is around .68 with the Census and only .61 in the LFS in 1999. This discrepancy is due to the much smaller sample size of the LFS and its lack of representativity for such a specific group.

24 Note that previous studies using single (women) as a control group to measure a treatment (usually the EITC) on single mothers face the same issue of endogenous single motherhood (see the discussion in Meyer and Rosenbaum, 2001).
This graph also shows a small effect that is essentially driven, in that larger group, by the singles of our selection. That is, men in couples are not sensitive to the RMI disincentive. The third graph (“all men”) adds men in couples with children, who are not concerned by the discontinuity and whose age-employment pattern increases further in their late 20s. As a result, this graph shows no discontinuity at age 25, but simply a break in the increasing employment-age pattern caused by our selected group. Noteworthy is the fact that the age-employment profile for childless men (second graph) is similar to that for our selected group, i.e., the employment rate trends upwards as a function of age but then flattens out in the second half of their twenties. Additional results show that this pattern was very similar in 1982 for these two groups (see Fig. 7 for the childless singles). A comparison with the third graph confirms that men without children are in general negatively selected in terms of their labor market prospects and that the magnitude of the bias increases as a function of age. This also confirms that the flattening of the curve in the late twenties of our selected group should not be attributed to any effects related to the RMI.

Finally, we have argued above that a clean RD analysis on women would be complicated by the fact that around the discontinuity, women tend to have children more often than men. We have nonetheless replicated the analysis for this group (results available from the authors). Interestingly, the shape of the employment-age pattern is very similar to that of men, increasing then flattening in their late twenties. We also find a small drop at age 25 for single female HS dropouts, less significant than for men, however. This may point to different labor market behavior, and possibly greater stigma of living on welfare for women. Alternatively, this may simply confirm that the RD analysis at age 25 gives more ambiguous results for women than for men, given the smaller sample of childless single female HS dropouts. 27

6. Conclusion

This study exploits a unique feature of the French guaranteed minimum income (RMI), namely that individuals aged under 25 are not eligible for it. We find strong evidence that the transfer program reduces the employment rate of uneducated single males as they turn 25. The effect is significant but relatively modest and confined to this group.

Three broad types of conclusions can be derived from this study. Firstly, we confirm that the participation elasticities of single men are basically zero, except for the lowest educated group. The fact that higher education levels show no effect may crucially affect welfare and optimal tax analyses where labor supply responses are heterogeneous across income groups.

Secondly, the policy implications are important. This study shows that the concern for a RMI “inactivity” trap may be of a more limited scope than initially thought, at least for the group of single individuals that represent 58% of all RMI recipients (around half a million people). We cannot extrapolate our results to other age groups far apart from the threshold. Yet, for those above 25, it is likely that the disincentive effect decreases with age. Indeed, as shown in our results using the LFS, the opportunity costs of staying out of the labor market may increase as wage prospects increase. This implies that the recent EITC-type of reform that extends RMI payments to the working poor (the RSA reform) may in fact have very modest effects on the labor market participation of the core RMI recipients. More specialized schemes aimed at reconnecting these uneducated single individuals to the labor market may be more beneficial. If the policy objective is purely incentivization, this would also suggest that the essential difference of scope between the EITC and the RSA is not justified, i.e., that the “in-work” component of the RSA should be mainly targeted at single mothers as it is in the US (note that the disincentive effect of household means-tested tax credits on married women is beyond the scope than initially thought, at least for the group of single individuals that represent 58% of all RMI recipients (around half a million people). We cannot extrapolate our results to other age groups far apart from the threshold. Yet, for those above 25, it is likely that the disincentive effect decreases with age. Indeed, as shown in our results using the LFS, the opportunity costs of staying out of the labor market may increase as wage prospects increase. This implies that the recent EITC-type of reform that extends RMI payments to the working poor (the RSA reform) may in fact have very modest effects on the labor market participation of the core RMI recipients. More specialized schemes aimed at reconnecting these uneducated single individuals to the labor market may be more beneficial. If the policy objective is purely incentivization, this would also suggest that the essential difference of scope between the EITC and the RSA is not justified, i.e., that the “in-work” component of the RSA should be mainly targeted at single mothers as it is in the US (note that the disincentive effect of household means-tested tax credits on married women is beyond the scope than initially thought, at least for the group of single individuals that represent 58% of all RMI recipients (around half a million people). We cannot extrapolate our results to other age groups far apart from the threshold. Yet, for those above 25, it is likely that the disincentive effect decreases with age. Indeed, as shown in our results using the LFS, the opportunity costs of staying out of the labor market may increase as wage prospects increase. This implies that the recent EITC-type of reform that extends RMI payments to the working poor (the RSA reform) may in fact have very modest effects on the labor market participation of the core RMI recipients. More specialized schemes aimed at reconnecting these uneducated single individuals to the labor market may be more beneficial. If the policy objective is purely incentivization, this would also suggest that the essential difference of scope between the EITC and the RSA is not justified, i.e., that the “in-work” component of the RSA should be mainly targeted at single mothers as it is in the US (note that the disincentive effect of household means-tested tax credits on married women is beyond the scope than initially thought, at least for the group of single individuals that represent 58% of all RMI recipients (around half a million people). We cannot extrapolate our results to other age groups far apart from the threshold. Yet, for those above 25, it is likely that the disincentive effect decreases with age. Indeed, as shown in our results using the LFS, the opportunity costs of staying out of the labor market may increase as wage prospects increase. This implies that the recent EITC-type of reform that extends RMI payments to the working poor (the RSA reform) may in fact have very modest effects on the labor market participation of the core RMI recipients. More specialized schemes aimed at reconnecting these uneducated single individuals to the labor market may be more beneficial. If the policy objective is purely incentivization, this would also suggest that the essential difference of scope between the EITC and the RSA is not justified, i.e., that the “in-work” component of the RSA should be mainly targeted at single mothers as it is in the US (note that the disincentive effect of household means-tested tax credits on married women is beyond the
scope of this discussion, cf., Blundell et al., 2000). Indeed, lone parents face high costs of work and are most responsive to incentives. This is, of course, a pure efficiency argument, independent from other policy objectives that may apply (e.g., reducing child poverty and improving the living standards of the working poor).

Last, but not least, youth unemployment has received renewed attention recently as 16–24 year olds have been hit particularly hard by the crisis. This population faces the highest rate of unemployment in France – basically one youth out of four is unemployed – and has limited access to welfare programs, which results in a poverty rate twice as large as that of the 25–30 years-olds (almost 11% when the poverty line is half the median income). Further research should evaluate the implications of extending the RMI to these young workers. Yet, extrapolating our results on the basis of the reasoning above would suggest that HS dropouts in this age group could be more affected than those in their late twenties due to lower wage prospects. The concern expressed by Cahuc et al. (2008), that reducing poverty in this group should be done without further weakening their attachment to the labor market, is therefore genuine.

Several other aspects are worth investigating in future research. Firstly, high implicit marginal tax rates associated with social programs not only distort relative prices in favor of leisure and against labor, but also increase the attractiveness of household production and black-market labor. In particular, the possibility of receiving income from informal sector activities may further decrease the differential between financial returns to "official" work and welfare payments, and hence exacerbate the disincentive effect due to the RMI. In this case, it would be interesting to verify whether the target group characterized in the present study does indeed increase its participation in the shadow labor market at age 25. Secondly, we have simply considered the static participation effect of the RMI. The program may also affect the intertemporal allocation of labor supply.29 Yet it seems unlikely that those who anticipate leaving the labor market at age 25 work more in the preceding years. On the contrary, it is possible that the RMI encourages younger workers to linger in precarious activities while "waiting" for the RMI, as suggested by the significant decrease in the proportion of short-term contracts at age 25 found in this study.

Appendix A

### Table A.1

<table>
<thead>
<tr>
<th>Age Cell size</th>
<th>HS dropouts</th>
<th>Basic vocational qualification</th>
<th>HS diploma</th>
<th>2-year college degree</th>
<th>All men</th>
</tr>
</thead>
<tbody>
<tr>
<td>All men 1999</td>
<td>90,070 0.118</td>
<td>10,643 0.025</td>
<td>9958 0.001</td>
<td>0.927</td>
<td></td>
</tr>
<tr>
<td>All men 1999</td>
<td>89,113 0.107</td>
<td>9532 0.050</td>
<td>8411 0.001</td>
<td>0.905</td>
<td></td>
</tr>
<tr>
<td>Single men 1999</td>
<td>86,363 0.103</td>
<td>8883 0.077</td>
<td>7277 0.002</td>
<td>0.870</td>
<td></td>
</tr>
<tr>
<td>Single men 1999</td>
<td>85,160 0.107</td>
<td>9102 0.126</td>
<td>6699 0.004</td>
<td>0.838</td>
<td></td>
</tr>
<tr>
<td>Single men 1999</td>
<td>93,425 0.110</td>
<td>10,271 0.172</td>
<td>6724 0.005</td>
<td>0.798</td>
<td></td>
</tr>
<tr>
<td>Single men 1999</td>
<td>97,090 0.117</td>
<td>11,350 0.224</td>
<td>6714 0.008</td>
<td>0.762</td>
<td></td>
</tr>
</tbody>
</table>

29 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.

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Fig. 8. Employment rate of male HS dropouts per demographic group (Census 1999).
Table A.2 (continued)

<table>
<thead>
<tr>
<th>Age</th>
<th>All men 1999</th>
<th>HS dropout 1999</th>
<th>All men 1999</th>
<th>Single men 1999</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Cell size</td>
<td>% HS dropouts</td>
<td>Cell size</td>
<td>% w/ children</td>
</tr>
<tr>
<td>26</td>
<td>104,916</td>
<td>0.125</td>
<td>13,090</td>
<td>0.271</td>
</tr>
<tr>
<td>27</td>
<td>105,443</td>
<td>0.133</td>
<td>14,020</td>
<td>0.324</td>
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<tr>
<td>28</td>
<td>105,375</td>
<td>0.143</td>
<td>15,121</td>
<td>0.388</td>
</tr>
<tr>
<td>29</td>
<td>102,629</td>
<td>0.147</td>
<td>15,086</td>
<td>0.433</td>
</tr>
<tr>
<td>30</td>
<td>102,019</td>
<td>0.152</td>
<td>15,472</td>
<td>0.483</td>
</tr>
<tr>
<td>31</td>
<td>101,077</td>
<td>0.153</td>
<td>15,601</td>
<td>0.521</td>
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<tr>
<td>32</td>
<td>104,171</td>
<td>0.157</td>
<td>16,371</td>
<td>0.555</td>
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<td>33</td>
<td>106,431</td>
<td>0.163</td>
<td>17,395</td>
<td>0.572</td>
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<tr>
<td>34</td>
<td>107,908</td>
<td>0.166</td>
<td>17,879</td>
<td>0.592</td>
</tr>
<tr>
<td>35</td>
<td>108,919</td>
<td>0.167</td>
<td>18,136</td>
<td>0.611</td>
</tr>
</tbody>
</table>

Note: These descriptive statistics are based on the French Census data covering 1/4 of the population.

References


