The effect of the *Revenu de Solidarité Active* on part-time employment

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Abstract

We evaluate the effect of the *Revenu de Solidarité Active* on part-time employment using the data of the French labor force survey for the years 2003-2010. In order to avoid issues due to the endogeneity of the fact of receiving the RSA, we use the exogenous source of variation in the treatment created by the age-limit for eligibility: only persons aged more than 25 can be eligible. We use a difference-in-difference setting, comparing the persons aged 25 or more with the ones aged less than 25. We study both the evolution of the stock of part-time employment and of transitions around part-time. We find no significant effect of RSA on part-time work.

Acknowledgements

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1 Introduction

The *Revenu de Solidarité Active* (RSA) (earned income supplement) was introduced in France in June 2009. This social benefit replaces the former *Revenu Minimum d'Insertion* (RMI), with the aim to cancel its employment-discouraging effect, in particular for part-time jobs. The RMI was an allowance given to low-income households; a particularity of this scheme was that any further resource of the household was entirely deduced from the RMI amount, thus creating a 100% tax rate on the first euros earned. Many studies have questioned the efficiency of such a scheme, pointing out its employment-disincentive effects, particularly for low-income work, like part-time jobs.

Bourguignon (1997) mentions a possibility of poverty trap due to the RMI. Some authors also tried and evaluated the disincentive effects of the RMI; Laroque et Salanié (1999) have shown that there is little financial incentive to work for a minimum wage for a large share of RMI beneficiaries. Anne and L'Horty (2001) underline that this effect is even stronger if taking into account local social transfers and taxes, that are linked to receiving the RMI. Gurgand and Margolis (2001) also confirm that financial gains to work can be quite small for those people. Pisani-Ferry (2001) proposes the idea of an negative income tax for low labor incomes in order to overcome the poverty trap. This is in this spirit that the RSA was designed, with only 38% of labor income deduced from the allowance, instead of 100% with the RMI. This increased largely the incentive to work part-time, but increased the marginal tax rate when going from part-time to full-time, thus creating a fear for part-time-work trap, while displacing the discouraging large marginal tax rate from the first euros to about one minimum wage.

In this thesis, we want to study whether this increase in financial incentives to work had a positive effect on part-time employment. Two underlying questions are behind this: first, did the RSA remove the RMI's disincentive effect on part-time work ? Second, did the RSA replace the RMI's poverty trap with a part-time work trap ?

There are quite few empirical evaluations of French public policies of financial incentives to work, most of them using the method of difference-in-difference. Piketty

(1998) used this method to evaluate the effect of the extension of the *Allocation Parentale d'Education* in 1994 that modified the financial incentives to work only for women with two children, and the impact of the creation of the RMI on single parents. He finds that single men are not significantly affected by financial incentives to work, but that women are much more responsive to those incentives.

Cochard et al. (2008) evaluate the effect of the *Prime pour l'emploi* (PPE) (earned income tax credit) on employment, using a difference-in-difference scheme, with the important limitation of the construction of their treated and comparison groups, the treated being the persons with characteristics making them likely to be eligible, the control being those with characteristics very near to the first one's but not eligible. This leads to the possibility of negative classification and selection biases. The authors find no significant effect of the PPE in 2001, but because of those biases, it is only a lower bound. Stancanelli (2004) also uses differences-in-differences to measure the impact of the PPE on female employment with the data from the French labor-force survey comparing potentially eligible women (on the basis of their income) with non-eligible women, and married women with cohabiting ones. She finds a negative significant effect for married women, but non significant effects for cohabiting and single women. Bloemen and Stancanelli (2007) improve this study by simultaneously estimating the employment rate and the probability of being eligible in order to endogenize eligibility: they find no effect of the PPE on the employment of women anymore.

Foreign experience gave rise to a more abundant literature on empirical evaluation. The Earned income tax credit in the United States was mostly evaluated by difference-in-difference. Eissa and Liebman (1996) showed that it increased employment for single mothers, leaving unchanged employment of single women without children. Meyer and Rosenbaum (2001) find similar results with a structural model. The Working families tax credit in the United Kingdom was also evaluated by difference-in-difference by Blundell et *al.* (2005), who find an increase in single mothers' employment, but no impact on couples with children. Many other studies find similar results.

The RSA has been evaluated by a committee mandated by the government. They use two different strategies to identify the effect of the RSA on employment: first, they compare

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the rate of return to work of different types of households whose incentives to work have been differently affected by the RSA; second, they use a regression-in-discontinuity design around the age of 25, as only persons aged more than 25 can be eligible to the RSA. They find no clear significant effect of the RSA on employment nor on part-time employment.

In this thesis, we want to use the data from the French labor-force survey (*Enquête Emploi*) from 1990 to 2010 to study the evolution of part-time in France, particularly under the effect of the RSA. As in the study by the evaluation committee, we will use the exogenous variation in the reception of the RSA around 25 years of age to identify the effect of the RSA. We will conduct a difference-in-difference analysis both on the whole population and on different household types, in order to take into account the difference in the changes in incentives across households. Unlike the studies by the evaluation committee, we do not want to study only the stock of part-time employment and the probability of working part-time, but also transitions around part-time, in order to explain the evolution of part-time employment by decomposing it, and more particularly, to identify a potential part-time trap: an increase in part-time work would not be enough to conclude that there is such a trap: we would need one more element: the fact that the increase in part-time employment comes at least partly from full-time rather than from nonemployment. We will thus have to carefully study the transitions from full-time to part-time but also those from part-time to part-time and from part-time to full-time to see whether some persons forgo going from part-time to full-time because of the RSA.

The identification of the effect will suffer some difficulties: first, the RSA is meant to increase labor-supply, but it was implemented in a time of labor-demand crisis. As a consequence, its effects on employment may be very small. Moreover, one will have to keep in mind that people may not react fully to financial incentives, but may be sensitive also to other aspects.

This work is organized as follows: section 2 reviews RSA's legal framework as well as the literature on the RSA. Section 3 presents the data used and draws a global picture of part-time work in France. In section 4, we try and estimate the effect of the RSA on part-time employment. Section 5 concludes.

2 The Revenu de Solidarité Active: legal framework and literature review

2.1 The workings of the scheme

2.1.1 The RMI's and API's legal framework

The *Revenu de solidarité active* (RSA) was introduced in June 2009 as a substitute to the former *Revenu minimum d'insertion* (RMI) and *Allocation Parent Isolé* (API) (single-parent benefit).

The RMI was a means-tested social benefit given to households with no income or income under a given threshold. The threshold depended on the composition of the household, the threshold being higher the more children in the household, and also higher for households with heads of household living as a couple rather than single persons.

All resources were included in the income taken into account: wages, pensions, rents, unemployment benefits, family benefits, daily sickness allowances, alimonies. If the sum of those incomes was higher than the threshold, then the household could not receive the RMI.

If the household received housing benefits, owned its house or had no rent to pay nor housing loan to repay, the lump-sum amount was diminished by a housing fee (*forfait logement*), which again increased with the number of people in the household.

The RMI was a differential benefit: every household income was withdrawn from the threshold amount corresponding to the type of household, so that the marginal tax rate on labor income was equal to 100%: each first euro earned diminished the amount of RMI perceived by 1 euro and did not increase the household disposable income.

However, an unemployed person going back to employment could accumulate the RMI with all her labor income during 3 months, then, during the following nine months, only 50% of labor income was deducted from the RMI. This scheme, called *intéressement*, was supposed to avoid the RMI to create a disincentive to work.

Yet, the RMI was still blamed for decreasing incentives to work because of a lack of gains to work, particularly from working for the minimum wage, and/or part-time.

The RSA also replaced the API, which was given to single parents living in France with resources below a given threshold. This threshold also depended on the household composition.

2.1.2 The RSA's legal framework

The RSA was created as a response to those critics. The system of thresholds depending on the household composition is still the same, as are the housing fee scheme, and the various resources taken into account in the computation of the households' income. The equivalence scale for the number of people in the household is unchanged. The main change is that only 38% of labor income, against 100% for the RMI, is deduced from the benefit. For each euro earned, a household receiving the RSA will gain 62 cents, instead of nothing in the case of the RMI (except during the *intéressement* period), and so until labor income reaches a given point, at which the household goes out of the RSA eligibility.

RMI= max {lump sum amount - housing fee - household income ; 0}

RSA= max {lump sum amount - housing fee - household income + 0.62*labor income ; 0} [1]

The possibility to receive both RSA benefits and labor income has no time limit, in contrast to the RMI, where the *intéressement* period last at most 12 months. This is supposed to encourage those who benefit from RSA to get employed: the aim of the RSA design is to make work always more profitable than inactivity or unemployment. The RSA given to unemployed or inactive people is thus exactly similar in level and conditions to the former RMI or API and is called *RSA socle* (it is equal to the lump-sum amount minus the housing fee and household income), whereas the supplementary amount received by working people is called *RSA chapeau* and corresponds to 62% of labor income.

Here is an example of the amounts a household can receive according to its characteristics:

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1 In those equations, household income represents labor income and other incomes, so that 38% of labor income are deduced from the lump-sum amount for the RSA computation.

Table 1 : amounts of RSA for different types of households

Single person	Number of children	Couple
0	474,93 €	712,40 €
1	712,40 €	854,87 €
2	854,87 €	997,35 €
Per additional child	189,97 €	189,97 €
Source: service-public.fr, figures for 2012		

As for the RMI, the level of the exit point for the RSA depends on the type of household. As an example, for a single person, it is at 1.04 minimum wage, for a couple with 3 children, 2,1 minimum wages.

Both RMI and RSA are available for the whole French population, except those living abroad. Foreigners living in France can be eligible under some conditions only.

Only non-student persons have access to the RSA.

Until September 2010, people aged less than 25 were not eligible, unless being a single parent. Since September 2010, persons aged less than 25 can also be eligible if they have been working during at least 2 years on the last 3 years preceding the application for the RSA.

2.2 Expected effects: change in incentives and firms' strategic behavior

With respect to labor-market incentives, the important point of the RSA vs the RMI is that it decreases the marginal tax rates for the first euros earned (from 100% at the RMI time to 38% with the RSA). It thus increases the financial gains to work, at least for the first hours worked.

However, as a consequence, it increases the marginal tax rate at a higher labor income level, that is, at the level of the RSA exit point (around one minimum full-time wage for a single person): for instance, for a single person, it increases the marginal tax rate when going from a part-time to a full-time work. One could fear that some types of households have incentives to decrease their labor-supply, or, at least, to forgo increasing it.

The RSA should thus increase part-time work through two channels: first, by increasing incentives to participate to the labor-market, second, by increasing the marginal tax rate when going from part-time to full-time work.

It has even been said that the RSA could create a part-time trap, by displacing the threshold disincentive effect from the transition from nonemployment to part-time to the one from part-time to full-time.

Moreover, involuntary part-time could also increase, because of strategic behaviors of employers, who could propose more part-time work, knowing that workers are more likely to accept low wages, as they will be compensated by the RSA. Part-time but also involuntary part-time has increased a lot after the cut in employer social security contributions for part-time jobs in 1992, which could be an argument for this possible effect with the RSA.

2.2.1 Financial gains to work as a tool for studying changes in incentives

The more intuitive way to study the theoretical effect of the RSA on part-time is to look at the changes in financial gains to work part-time with respect to non-employment and full-time.

As RSA levels and eligibility change according to the composition of the household, the theoretical incentive effects differ according to the types of households.

One can distinguish between short-term and long-term incentive effects, according to whether one compares the RSA situation with the RMI with or without *intéressement*. Following the report of the evaluation committee (Annex 7), change in financial gains create a disincentive to go from unemployment or inactivity to work in the short-term but

less so for part-time than for full-time jobs, whereas in the long-term, returning to work induces positive gains in most cases. Here, we will consider long-term incentives, as the RSA was designed in order to change those long-term incentives, not the short-term ones.

Table 11 shows the changes in financial incentives to work part-time instead of non-employment between the RSA and the RMI situations. For persons having a full-time working partner, the change in financial gains is null; it is very high for people with a non-working partner, of for single individuals, and still important but smaller for people with a part-time working partner.

However, an analysis relying only on the changes in financial gains could suffer some shortcomings: individuals may also value leisure, so that an increase in the financial gains can create both income and substitution effects: the substitution effect increases labor-supply, but the income effect decreases it.

We rely here on the analysis by Philippe Briard et Olivia Sautory (2011) (Annex 8 of the evaluation committee report): the income effect is due to the fact that, working as many hours as in the RMI situation, an eligible active household earns more, because of the RSA-*activité* part of the scheme, which should decrease the incentive to work more. The substitution effect comes from the change in the marginal gain of one more hour worked: the marginal tax rate is not 100% anymore but 38% for the people far under the eligibility threshold, particularly for non working people ; however, this threshold becomes 100% for people who are near to the threshold. As a consequence, households benefiting from the RSA *socle* only (that is, households where none of the members is working) have an incentive to work more (for them, there is no income effect, as the RSA *socle* is perfectly equivalent to the former RMI), whereas households benefiting from the RSA *activité* have an incentive to decrease their labor supply, because of the income effect, and of the risk of getting over the eligibility threshold if working more. However, for households receiving both RSA *socle* and *activité* (that is households with working persons, but whose labor income is so low that they would have been RMI-eligible in the RMI situation), the effect is ambiguous (negative income effect and positive substitution effect). Non-eligible households that are near to the eligibility threshold may have an incentive to decrease their labor supply in order to become eligible.

Nevertheless, looking at table 11, one can see that the changes in financial gains to work part-time are very concordant with the income- and substitution-effects analysis: people with a full-time working partner are probably, if eligible to the RSA, receiving only RSA-socle, and have no increase in incentive to work part-time, which corresponds to the prediction of the income effect. Households with no labor income have a large increase in financial gains to work part-time, which corresponds to the positive substitution effect. Finally, households with one part-time working person, and one non-employed person, have a much lower increase in financial gains to work part-time, which correspond to the ambiguous effect noted by Briard and Sautory.

As a consequence, it appears that the changes in financial gains to work created by the RSA are very concordant with the predictions of substitution and income effects induced by the reform: household for whom the RSA creates a negative income effect have anyway no increase in their financial gains to work more. This is an argument for using only the changes in financial gains to work to study the incentives created by the RSA, as we do in this work.

2.2.2 Ex-ante simulations

Some ex-ante simulations have computed the expected effects of the RSA.

Denis Anne and Yannick L'Horty (2009) made a simulation across different household types on data for 10 middle-size towns in different regions of France. They computed the reservation working time, that is the minimum time that one has to work in order to increase her disposable income. In the RMI case, taking into account the *Prime Pour l'Emploi* and the *intéressement,* this was in average, 4 hours for a couple with 3 children, 18 for a couple with 2 children, 0 for a couple with 1 or no children, and 9 for a single person. With the RSA, all those figures shrink to 0. Hence, they conclude to a large effect of the RSA on the incentives to return to employment. However, they do not address the question of part-time work.

Guillaume Allègre (2011) used a micro-simulation based on a labor-supply model to estimate the effect on labor-supply of the change in the marginal tax rates at different working-times for single and in-couple women. He finds that, for low-wage women, the RSA reduces marginal tax rates when going from unemployment to part-time, but increases this rate for women with wages in the deciles 4 to 7. When going from part-time to full-time, the RSA generally increases the marginal tax rates, particularly for low wage women. He finds that 7000 single women would enter employment, whereas 18000 in-couple women would stop working. Part-time labor supply would increase by 11000 single women and decrease by 2000 in-couple women.

2.2.3 Possible practical limitations of the efficiency of the reform

However, the evaluation committee report counts a 51% take-up for the RSA in 2011, and even less in the years before, which could undermine a lot those theoretical effects. The lack of knowledge of the scheme and of its precise functioning in the population could also decrease them. Finally, the RSA is a supply-side policy, but it is implemented in a period where demand is very limited, which could make the effect of the RSA quite hard to detect. Clerc (2009) underlines that in two thirds of the cases, the RSA will not be higher than the *Prime pour l'emploi*, that is deduced from the RSA amount, so that the RSA will only have the advantage of being received instantaneously instead of coming with a one-year delay, but will not increase the household income.

2.3. First evaluations

Before its implementation in June 2009, the RSA has been experimented in some French *départements* (administrative regions), and this experimentation was evaluated.
After the implementation, an evaluation committee was mandated by the government to evaluate the effects of the RSA. It used both a qualitative and a quantitative survey. The qualitative survey was conducted in 2010-2011 in five *départements* interviewing 200 households receiving RSA *socle* benefits, and 160 households receiving RSA *chapeau.*

The quantitative survey was also conducted in 2010-2011, with 2 steps, one by phone,

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and another one, face-to-face. The phone step interviewed 15000 households selected for having low 2008 income according to housing tax and income tax declarations, and determined whether the households were eligible to the RSA and whether they actually benefited from it. The eligibility was computed according to simplified rules, so that the eligibility criterion does not perfectly match effective eligibility. In the second step, 3300 eligible (benefiting or not) households were interviewed.

2.3.1 On the probability of returning to employment

The experimentation shows in average a 9% difference between the regions where the RSA was experimented and control regions for the rate of entry in employment; however it is significant only at the 12% level, and the estimates are very different according to the regions.

The evaluation of the implemented RSA confirms, but with less strength, those results.

Oliver Bargain and Augustin Vicard, using census data, use the discontinuity around 25 years old, comparing employment rates of people aged just less than 25 and so not eligible to the RMI or RSA, and those aged just more, so eligible. They select people aged between 20 and 30, without children, with a low-level study (so that they are more likely to be eligible to the RMI and/or RSA), and single (because most RMI/RSA beneficiaries are single). They will also use non single with no children persons, for comparison reasons. As they are very similar populations, they should have identical labor-market outcomes except for the effect of the RMI/RSA. Studying the employment rate of those two groups with a regression in discontinuity design, they find that the RMI had a small disincentive effect for single people without children and with low education: the 25-year old with those characteristics are slightly less employed than their 24-year old comparison group; whereas this effect is not observable anymore in 2010 after the introduction of the RSA. One limit of this finding is that the disincentive effect of RMI was significant only in 2004-2005, but not in the period just before the introduction of the RSA. This disincentive effect is stronger for part-time than for full-time jobs. In the beginning of 2010, a discontinuity in employment rate for the 25-year old is still present for part-time, which is a piece of evidence against the

fear of a part-time trap, at least for the young, single without children and low educated people.

Elisabeth Danzin, Véronique Simonnet and Danièle Trancart (evaluation comittee report) use the difference in financial gains to work again across various household types (as the RMI and RSA levels vary according to the household composition), to study the rate of return to employment. They use data from the *Caisse Nationale des Allocations Familiales*, the administration that supervises the payment of RSA benefits, that give information about all RSA-recipient households. For some household types, in particular some with 3 children or more, they find small significant increases in the returns to work, but in other configurations, the change in employment rates are not concordant with theoretical incentives.

2.3.2 On disincentive effects and increase in part-time employment

The experimentation shows a 15% difference of very low-wages (less than 200 euros) between RSA-experimented regions and the other regions. A qualitative survey confirms that part-time work is more developed in treated regions: 11% of employed people work 9 hours or less in the treated regions, against 6% in the control regions. This is accompanied by shorter transportation time in treated regions and this development of part-time does not come with a decrease in the satisfaction about working-time nor work conditions. This could be a sign of the existence of a real disincentive effect.

However, the evaluation of the real implementation of RSA shows very small, if not nonexistent, effects of the RSA on the decrease of labor-supply.

Interviews made with working RSA-recipients show no real decrease in labor supply due to the RSA; however some recipients refused overtime hours, or to take secondary low-remunerated jobs, in order to make sure to keep their RSA-benefits. In some cases, the RSA has encouraged part-time workers not to shift to full-time work, however other reasons, like family life, the development of a new business, are estimated to be more decisive than the RSA, and the authors consider that the RSA has played no role in the final decision.

An econometric study by Philippe Briard et Olivia Sautory (evaluation committee report) compares different household groups according to their distance to the RSA exit point; they use the quantitative study presented before; the variables of interest are the employment status and the number of hours worked. The households that are the nearest to the exit point (whether they are under or above it) are supposed to be more likely to decrease their labor supply, either in order to become eligible (if they are initially above the exit point), or to make sure that they will keep being eligible (for the ones under the exit point), and are thus compared with households that are further from the exit point. They find no evidence of a decrease in labor supply for the households near to the exit point. Even making this comparison for the only households who know the RSA scheme (in order to get rid of the problem of take-up), they find no disincentive effect.

2.3.3. Employers' behavior

In the evaluation committee, Mathieu Béraud, Nicolas Castel, Anne Eydoux, Emilie Fériel, Jean-Pascal Higelé, and Mathieu Grégoire have interviewed employers in low-wage sectors in order to know whether they display strategic behaviors under the effect of the RSA. At the date of the interviews (end of 2009), the employers do not seem to know the functioning of the RSA well, and they are not aware of whom among their employees benefit from it. However one trade-union official declares some cases where employers offer part-time jobs, putting forward the RSA as an income complement.

3 Data and global picture of part-time work in France

3.1 The French labor-force survey

The advantage to work with the labor-force survey (*Enquête emploi*) rather than the data used by the previous evaluation is the representativeness of its observations.

It covers continental France households, registers all individuals living in the sampled households and aged 15 or more.

Until 2012, the LFS was a yearly survey conducted in March and each household was interviewed once a year during three consecutive years, with one third of the sample renewed each year. Since 2003, it is conducted on a quarterly basis, with each household being interviewed six times at a one-quarter interval; the sample being renewed by sixth every quarter.

In the yearly survey, 75000 households are interviewed each year, in the quarterly survey 57000 households are interviewed each quarter.

This change in the survey design in 2003 induces comparability problems between the two periods: first, the interviews of the yearly survey are all conducted face-to-face, whereas only the first and the sixth quarterly interviews are face-to-face, all the others being conducted by phone. Both because of this difference in survey mode and in periodicity, attrition may also be different in both period: attrition is usually larger for interviews conducted by phone than for those led face-to-face. Moreover, one year after the first interview, there is less attrition in the yearly survey than in the quarterly one, probably because persons may get tired of being interviewed every quarter. Moreover, some questions changed between the yearly and the quarterly survey, particularly on labor-market outcomes. For example, the part-time employment variable changed from 2 to 3 modalities in 2003. From 2003 on, the interviewees can choose between full-time, part-time and irrelevant (for self-employed persons who estimate that this question does not apply to them), whereas before, the "irrelevant" modality did not exist. This may decrease the number of people declaring full-time employment after 2002, if the people choosing "irrelevant" would have chosen "full-time" if this last modality did not exist. Filters for the part-time question are also different: they seem to be equivalent, however, as the questions are not exactly the same, it can induce some measurement differences.

For this reason, we will use only the quarterly survey for the regression analysis of the effect of the RSA. The descriptive analysis will be made on the whole period, but comparison between pre- and post-2003 data has to be made only with great precaution.

The longitudinal dimension of the data allows to observe individuals' situations on the labor market at one-year interval, and at one-quarter interval since 2003, so as to study their transitions on the labor-market.

The many data that this survey provides on the households' characteristics are very useful to compare different types of households.

The most important limit of those data for our study is that the variable for RSA-receipt suffers large under-reporting. Moreover, what this variable covers is not clear-cut: it is a variable that already existed before the second quarter of 2009, called "RMI", and that covered the reception of RMI benefits. The name of this variable is still "RMI" after 2009. Moreover, the variable for the API still exists after the introduction of the RSA, although this allowance has been suppressed when the RSA has been created: some people declare both RSA (or rather "RMI", according to the survey words) and API, some others only RSA, others only API: should the latter be classified as RSA-recipients ?

The *Enquête emploi* does not provide either all the information needed to compute eligibility: some household sources of income are missing (some social benefits, for instance).

Another problem of those panel data is attrition: from one period to the others, some households that should still be interviewed go out of the sample, mainly because they move home, or because they refuse to answer, or are not reachable anymore. The problem arises when this attrition is selective, and when the determinants of attrition are correlated with the variable of interest, here the labor-market transitions or part-time employment. This is likely to be the case, as moving home occurs more when changing job, and as people who have difficulties on the labor-market usually tend to answer less to this kind of survey.

3.2 Description of part-time work in France

3.2.1 A brief history of public policies of part-time in France

Since 1998, the definition of part-time work in France corresponds to any work-duration below the legal one, that is 35 hours, or below the branch duration, if this one is lower. Before 1998, only a duration lower by 20% than the legal one was considered as part-time. This change in definition is not a problem for our estimations, as they will be realized on 2003-2010 data only (but, for the graphs presenting series from 1990 to 2010 there will be no possible comparison between the two periods).

Since the beginning of the 1990s successive French public policies had an impact on the level of part-time work. First of all, in 1992, a decrease by 30% of employer social contributions on part-time jobs was installed for creations of undetermined duration part-time contracts, or for transformations of full-time into part-time jobs. This, combined with a reduction in social contributions on low wages in 1993, induced a large increase in the share of part-time employment in total population: from 8.2% in 1992 to 11.3% in 1998.

The reform of working-time, that decreased legal working-time to 35 hours in 1998-2000 caused a stagnation in the number of part-time workers for various reasons (Oliveira and Ulrich, 2002): new hirings were more for the benefit of full-time jobs, and, in a lesser extent, some part-time jobs were transformed in full-time ones; this was also favored by an increase in working-time flexibility for full-time jobs, versus a decrease in this flexibility for part-time jobs.

In 2000, a law by Minister Aubry gave more guarantees of job security to part-time workers, thus making part-time employment less attractive for employers, and, more importantly, suppressed the 30% reduction in social contributions on part-time jobs. From 2003 on, the reduction in social contributions on low wages is computed on hourly wages, which suppresses the advantage for part-time jobs.

The economic crisis and its repercussions on the labor-market from the second quarter of 2008 could also have an effect of part-time work, according to the OECD (2010): reductions in work-duration have been implemented in order to avoid lay-offs. However, according to the OECD, those changes have been quite modest in France. Anyway, the

deterioration of labor-market conditions could have increase demand for part-time employment at the expense of full-time jobs.

In June 2009, the introduction of the RSA, for the reasons said before, could also have participated to an increase in part-time employment.

3.2.2 Evolution of part-time work in France

The evolution of part-time work is most often studied as the evolution of part-time work in proportion of total employment. Here, however, this approach is not sufficient: as we assume that the RSA could induce an increase in part-time work, at the expense not only of full-time work, but also of non-employment, we are interested in the evolution of part-time as a share of total population. These series are displayed in figure 1 (quarterly part-time employment from 2003 to 2010) and figure 2 (yearly part-time from 1990 to 2010); nevertheless, for comparison purposes, we present also, in figures 3 and 4, the evolution of part-time as a share of total employment. In all the graphs presented here, we use the 25-64 age old population: those under 25 are not eligible to the RSA, and the rate of employment above 64 is quite low. Moreover, above 64, other allowances are available (*minimum vieillesse*, replaced after 2006 by the *allocation de solidarité aux personnes âgées*), that make the RSA irrelevant.

In the first years of our period, the evolution of the share of part-time work in total population corresponds very well to the reforms mentioned before: part-time employment increases sharply between 1992 and 1998, is more stagnant between 1998 and 2000, and decreases after 2000, as shown in figures 2 and 4.

Because of the break in the series, it is difficult to compare before- and after-2003 data. In the after-2003 period, the most striking fact is the large increase from 2009 in the level of part-time work.

Figure 1 shows a constant increase in the share of part-time in total population since 2006. However, looking at figure 3, one can see that this increase corresponds to a stagnation in the rate of part-time in total employment from 2003 to 2008, followed by an

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increase in 2009 only. Hence, one can see the increase in part-time work in total population from 2006 to 2008 as a simple effect of the increase in the employment rate, with a proportional increase of part-time employment, whereas from 2009 on, the economic crisis led unemployment to increase a lot. From 2009 on, the increase in the volume of part-time work is not due to an decrease in unemployment anymore. As said before, this increase could be linked with the changes due to the economic crisis, or to the implementation of the RSA. Our estimation will have to deal with the disentanglement of those two possibilities.

Figure 1 : Part-time work as a proportion of total population (%) 2003-2009

Source: Enquête emploi. People aged 25-64.
Data corrected for seasonal variation and smoothed by moving average.

Figure 2 : Part-time work as a proportion of total population (%) 1990-2010

Source: Enquête emploi. People aged 25-64.
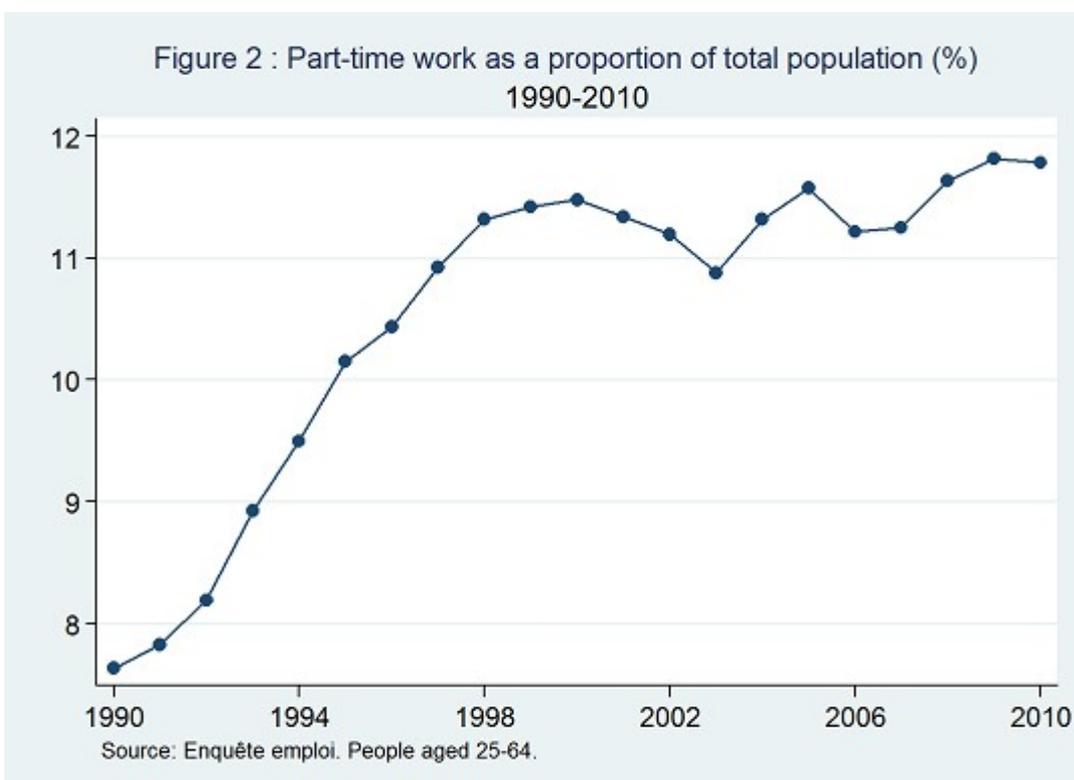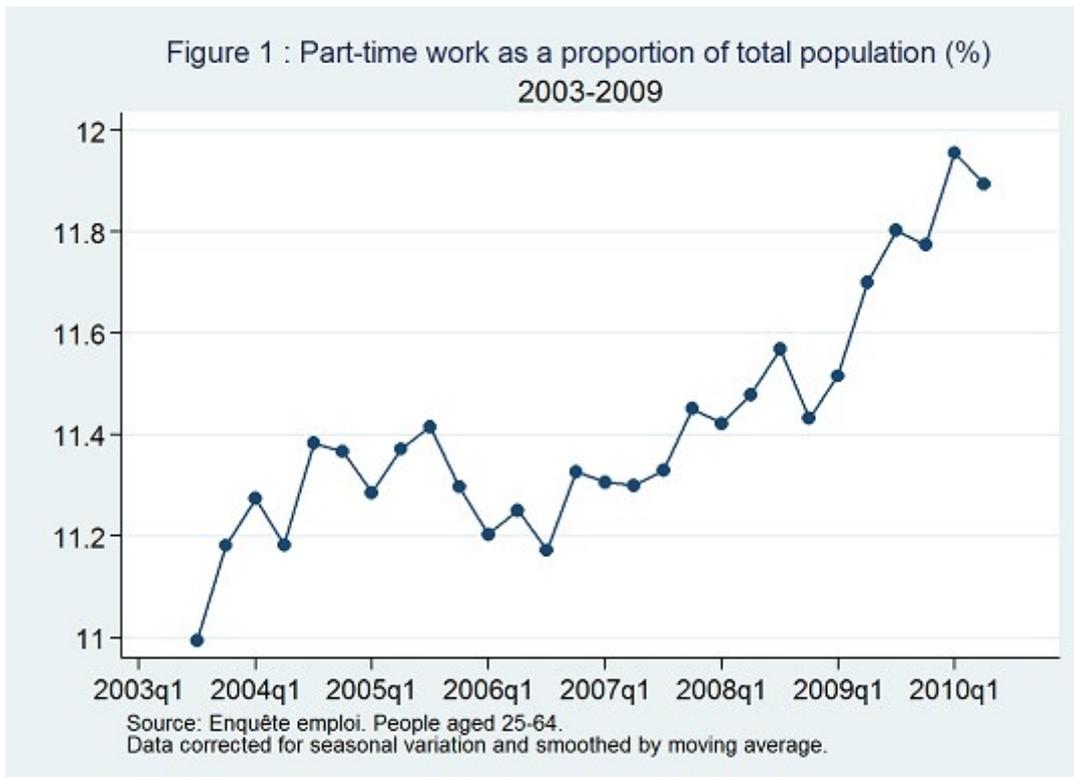
Figure 3 : Part-time work as a proportion of total employment (%) 2003-2009
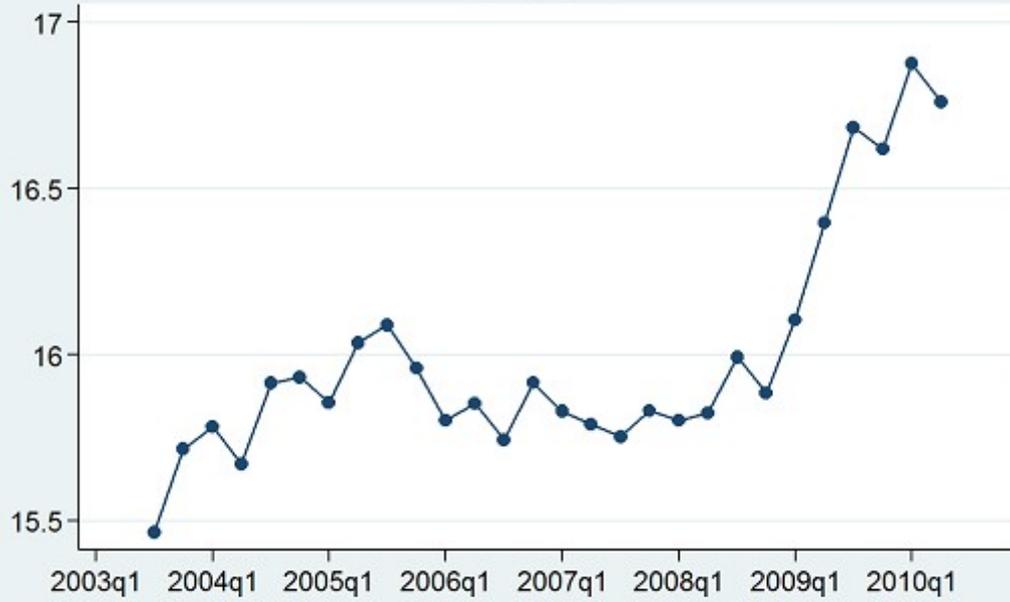
Source: Enquête emploi. People aged 25-64.
Data corrected for seasonal variation and smoothed by moving average.

Figure 4 : Part-time work as a proportion of total employment (%) 1990-2010
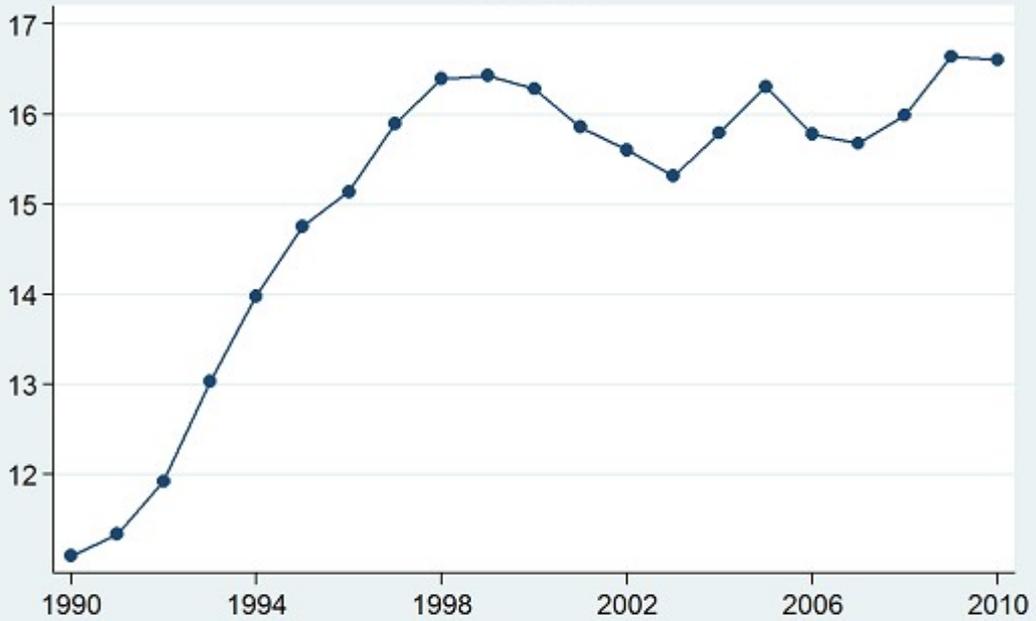
Source: Enquête emploi. People aged 25-64.

3.2.3 Transitions on the labor market around part-time

The study of transitions allow to explain the evolution of the stock of part-time work by linking it with the evolution of the flows from and toward part-time. We study both yearly and quarterly transitions: yearly transitions allow to have a large picture of the transition patterns over time, whereas quarterly transitions are useful to know the transitions in the same unit of time as the one used for the regression analysis.

Yearly transitions are observed from 1990 to 2010. For the 1990-2002 period, covered by the yearly survey, there are theoretically 3 interviews per person, but some people may go out of the sample if they move home or just stop answering or being reachable, so that the data present observations for one to three years per individual, which is equivalent to a maximum of one or two transitions per person. In order to avoid selective attrition, we use only observations for the first two interviews, so for the first transition, for which there is less attrition than between the second and the third interviews.

For the period covered by the quarterly survey (2003-2010), we use transitions from the 1st interview to the interview four quarters later, and, for the individuals for whom the 1st interview and the 4-quarter-later interview are not available, we use the transition from the 2nd interview to the interview four quarters later. We use the 4th quarter after the 1st or the 2nd interview rather than respectively the 5th or the 6th interview because some persons may not answer one quarter, and come back into the sample some time after, so that the 5th interview may take place more than one year after the 1st one.

From 1990 to 2002, the transitions are computed between two successive interviews, whereas from 2003 to 2010, they are computed between one interview and the 4th interview after (sometimes less); as a consequence, there is much more attrition for the second period than for the first one, which we will have to take into account to interpret the transition probabilities. As a consequence, the series are not comparable before and after 2003.

Quarterly transitions are computed using successive transitions from one quarter to another. We tried also using only transitions between first and second quarter in order to minimize attrition problems, but we got very similar results to the ones using all interviews.

We present here quarterly and yearly exits and entries into part-time, as well as the transitions from part-time and towards part-time.

For each of those transitions, we looked at transitions toward (or from, for transitions to part-time) unemployment, inactivity, part-time, full-time, and, for transitions from part-time, attrition, in order to avoid interpreting a change in a transition as a real change, while it could just be induced by a change in the attrition rate.

Transitions from part-time work are expressed as the probability, conditional on being in this situation at time t, of being either unemployed, inactive, part-time or full-time working, or out of the sample at time t+1. Transitions to part-time work are the probability of being either unemployed, inactive, part-time or full-time working at time t, conditionally on being working part-time at time t+1. Similarly, exit is the probability of not being in part-time at time t+1 when working part-time at time t, and entry is the probability of working part-time at time t+1 when not working part-time at time t.

We did not specifically study here whether attrition was selective, that is whether it affects more some types of transitions than others. However, it has been shown (Oliveira and Ulrich 2000) for transitions between 2000 and 2001 that attrition has no important effect on the transition probabilities. Behaghel (2003) finds no effect either for transitions in the 1990s. We have not found any study of the effect of attrition in the *Enquête emploi* on labor-market transitions after 2002, however, in order to simplify the analysis, we will make the hypothesis that attrition has no effect on transitions also for the quarterly survey.

A first decomposition of the share of part-time work in total population ca be done by looking at entries and exits from part-time as a share of total population. Quarterly exits and entries, as shown in figure 6, exhibit a very stable pattern from 2003 to 2010. Figure 5 shows clearer evolutions: the period of increase in the share of part-time, from 1991 to 1998 corresponds to an increase in both entries and exits, due to the Aubry law: more people working part-time increase also the number of people going out of part-time. Entries are nevertheless higher than exits during all the period, which corresponds to the increase in part-time over the period. This is followed by a period of decrease in part-time, due to a small decrease in entries following the end of subsidies, and to a stabilization of exits. From

2005 on, both lines are very near, which corresponds to the observed pattern of stabilization of the share of part-time work, except for the increase in part-time work between 2008 and 2009, that can be detected by the entry line going above the exit one in 2008.

Figure 10 shows that transitions to part-time are very flat, except for transitions from part-time to part-time, that increase between 1991 and 1998, explaining the increase in part-time work in this period.

Figure 11 confirms that part-time to part-time transitions led the increase in part-time in the 1991-1998 period. Very flat quarterly transition rates in figures 13 and 14 are in concordance with annual transitions between 2003 and 2010.

So, the introduction of Aubry subvention mainly had an impact on part-time-part-time transitions, with an increasing duration of the time spent in a part-time job, rather than an increase in non-employment-part-time transitions: part-time was less and less a short-term situation, but people exiting non-employment toward part-time were not more numerous.

In the case of the RSA, we may expect a different pattern, as the change in incentives is not on the demand but on the labor side, unlike at the time of the Aubry law. We could thus expect a larger increase in the transitions from non-employment to part-time.

We will try to verify this in the following section.

Figure 5 : Entries and exits from part-time as a share of total population (%) 1990-2010
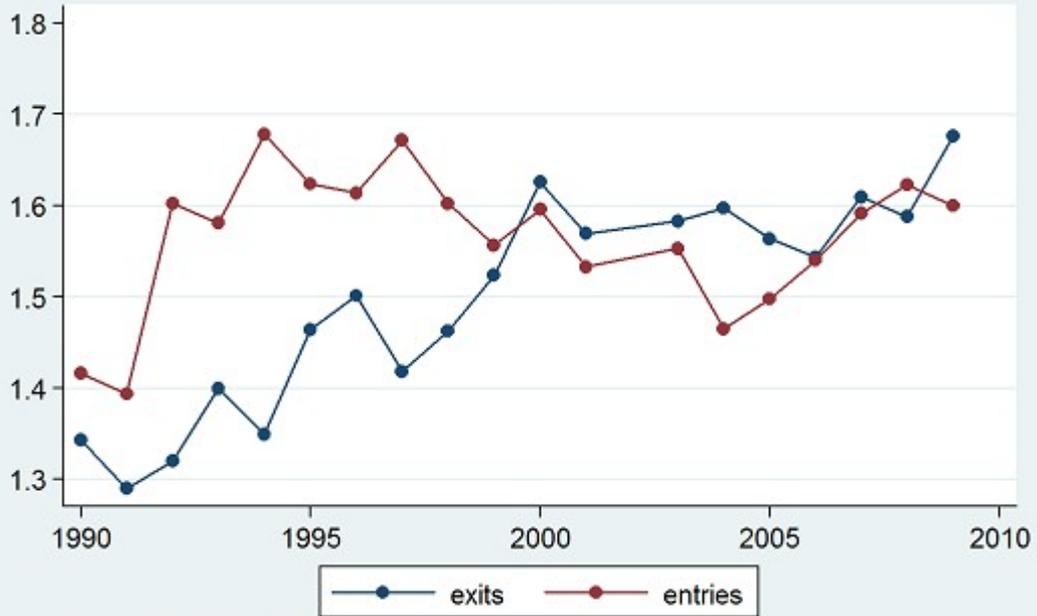
Source : Enquête emploi. People aged 15-64

Figure 6 : Entries and exits from part-time as a share of total population (%) 2003-2010
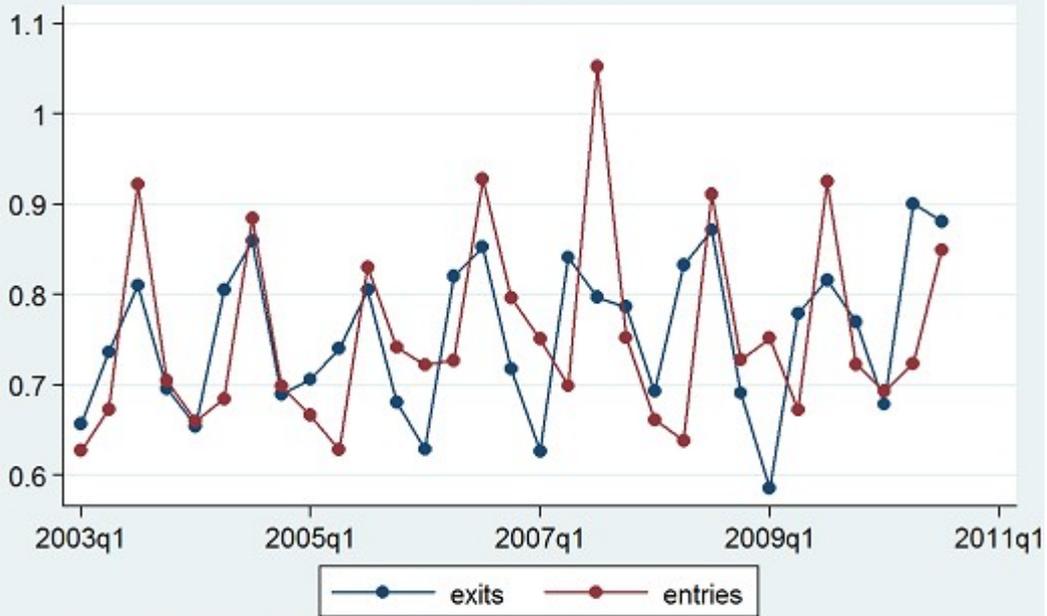
Source : Enquête emploi. People aged 15-64

Figure 7 : Transitions to part-time as a share of total population (%) 1990 - 2009

Legend:
- from full-time to part-time
- from unemp. to part-time
- from inactivity to part-time
- from part-time to part-time
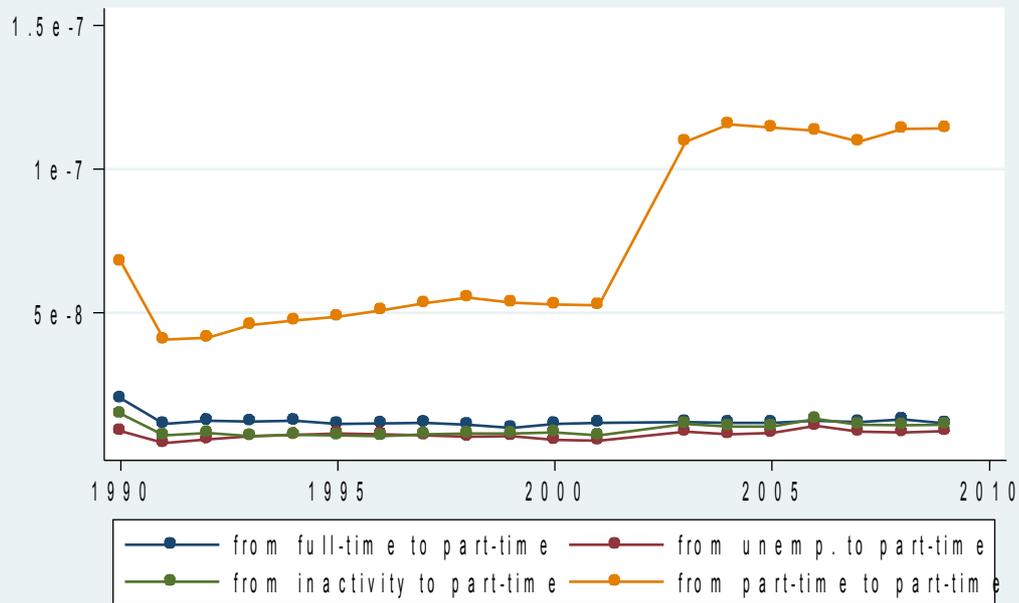
Source : Enquête emploi. People aged 15-64

Figure 8 : Transitions from part-time as a share of total population 1990 - 2009

Legend:
- from part-time to full-time
- from part-time to unemp.
- from part-time to inactivity
- from part-time to part-time
- attrition
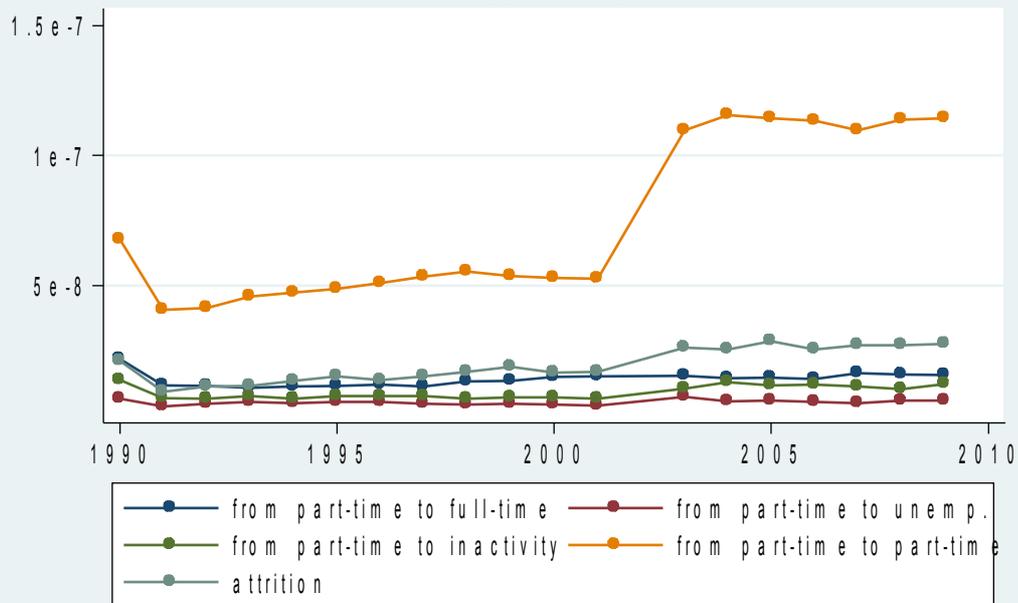
Source : Enquête emploi. People aged 15-64

Figure 9 : Transition probabilities from part-time as a share of total population (%) 2003-2010
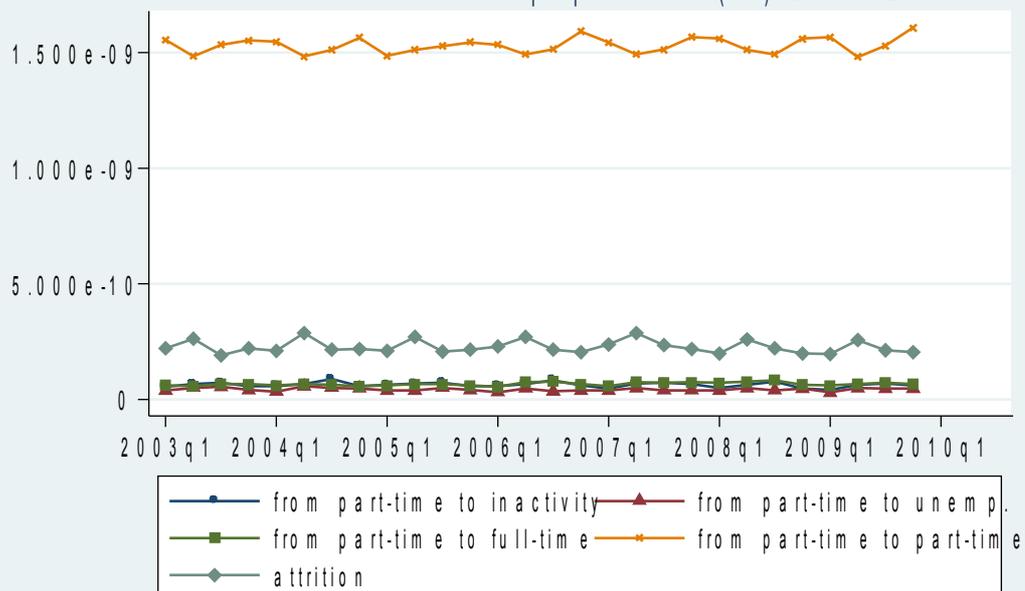
Source : Enquête emploi. People aged 15 and more.

Figure 10 : Transition probabilities to part-time as a share of total population (%) 2003-2010
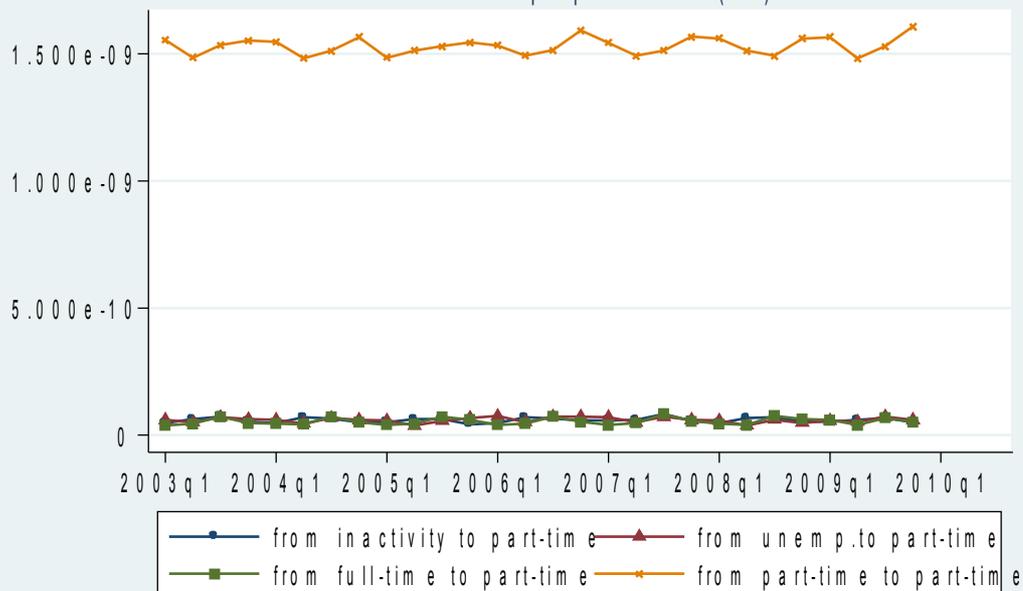
Source : Enquête emploi. People aged 15 and more.

4 The effect of RSA on part-time work

4. 1 Naive estimator and its limits

We first look at the evolution of the share of part-time work in the population receiving the RSA (we include those who declare API after the introduction of the RSA) according to our data. Using the fact that each individual is interviewed several times, we can follow the beneficiaries during at most 6 quarters, so that we have observations for some of those RSA-beneficiaries both after and before the introduction of the RSA.

Using a probit model (see box 1), we find (table 2) that the probability for those individuals to work part-time is higher after than before the reform, that the probability of being nonemployed[2] is smaller, the probability of working full-time being unchanged. Using a multinomial logit gives very exactly the same qualitative and quantitative results.

Table 2: average marginal effects for probit regression

Dependent variable	(1) part-time	(2) full-time	(3) nonemployment
2009q2	0.023*	0.009	-0.037**
	(0.05)	(0.06)	(0.05)
Constant	***	***	***
	(0.06)	(0.06)	(0.05)
N	15529	15529	15529

Those results are in line with what we expected: part-time employment seems to have increased at the expense of non-employment.

However, this estimator suffers from many shortcomings: first of all, there is a large under-reporting of the fact of receiving the RSA: only 2 % of the weighted population of the

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2 From here on, we will study inactivity and unemployment as a single nonemployment category. Indeed, in the labor-force survey, limits between inactivity and unemployment may be blurred ; moreover, the study of the transitions around part-time shows that transitions from or to unemployment and inactivity are very near in levels and evolution. Grouping thos two categories allows to have more data, and so, more precision.

Enquête emploi declares receiving the RSA, whereas, according to the evaluation committee report, about 7% of the population benefit from it. Yet, the people who self-declare are probably not a random sample of RSA-beneficiaries. If these people have a labor-market behavior, particularly concerning part-time work, that differs from the one of the total group of beneficiaries, or if they have particular individual characteristics that are important for labor-market outcomes (like age, number of children, etc.), this could induce a bias in the estimate. For example, if non-employed people declare more easily receiving RSA that employed persons, we may underestimate the decrease in non-employment and the increase in part-time employment due to RSA.

Box 1: The choice of the probit model

The outcomes we use are always alternatives that add up to one (the probabilities to be non-employed, part-time worker, full-time worker, sum up to one, as do the probabilities to go from part-time to part-time, to full-time, to non-employment and to attrition, etc.), which calls for the use of a multinomial logit.

However, the independence from irrelevant alternatives assumption that is needed for this model does not hold well, as the choice between non-employment and full-time, for example, is not independent from the possibility to work part-time.

This case where the error terms of the various regressions are correlated and where there is no cross-restriction on the coefficients of the different equations is the case of seemingly unrelated regressions, that can be estimated by running simple probit regressions equation by equation if the regressors of each equation are the same (Wooldridge (2001) p.150 and 164).

Anyway, we also ran again all the regressions presented below with multninomial logit (the results are not presented), which yielded exactly the same results, both qualitatively and quantitatively.

Second, even absent this problem of under-reporting, RSA take-up is quite small:

around 51%. And, here again, people who enter the scheme may have different characteristics from the ones of the other eligible. For instance, one can imagine that people who are very socially isolated have difficulties in both finding a job and knowing about new social benefits. Hence, there is a double selection problem: both application for RSA and declaring receiving benefits may be correlated with variables having an impact on labor-market outcomes.

Another problem may come from the fact that benefiting from RSA is not exogenous to working part-time: for example going from full-time to part-time work may induce a person to become eligible to the RSA, or choosing to work part-time may even be a strategic behavior in order to receive the RSA. This could increase the estimated coefficient for the change in part-time work due to the RSA.

But, even more important than all those endogeneity problems, labor-market has changed during the period, and the previous estimation does not disentangle the effect of the reform from more general economic changes. As the RSA was put in place in June 2009, it is hard to separate its effect from the one of the crisis: the recession has increased short-time work (OECD 2010), which could be confounded with a part-time enhancing effect of the RSA. From the beginning of 2009 on, part-time work as a share of total employment has increased steeply, making it difficult to identify an increase in part-time employment due specifically to the RSA. The positive effect of the RSA on part-time work we find may thus be overestimated, even if the decrease in the probability of non-employment may not be due to the global economic climate, as unemployment was increasing at the time of the introduction of the RSA.

All those limits make this estimator difficult to interpret, and we will prefer using the discontinuity around 25 years old to conduct a difference-in-difference analysis.

4.2 Estimation difficulties and strategy

In order to solve the endogeneity problems mentioned earlier, one could want to

instrument the fact of benefiting from RSA. This could be done with the fact of being over 25 years of age as an instrumental variable: however, this could solve the self-selection bias due to the non-random reception of the RSA among the eligible, but not the selection in the declaration of the RSA in the survey. Moreover, even considering that declaration is random, this estimator would not be a correct measure of the effect of the reform: it would yield the effect of receiving the RSA versus not receiving it, but would not give a comparative measure with respect to the RMI situation. For example, if we found a null effect of the RSA on part-time work, we would still want to know if this effect is different from a potential negative effect of the RMI on part-time work.

Because of the possibility of general changes in working-time at the time of the reform, the only way to compare pre- and post-reform situations while getting rid of the economic slowdown effect is to compare two groups that were differently affected by the RSA, before and after the reform.

Hence, one should find various population groups who were affected differently by the implementation of the RSA. This is particularly difficult because it is a universal benefit, aimed at all parts of the population.

One intuitive solution could be to compute an RSA-eligibility variable, based on household income (including labor income and social benefits) and household characteristics (number of children, single or in-couple parent), and use it as a comparison group for people receiving the RSA. We tried and did it, but the results are quite unreliable, for various reasons.

First, the construction of the eligibility variable is not perfect, as all variables needed for it are not present in the database. After constructing it, we compared it with the variable for receiving RSA: a large share of RSA-recipients were not included in our eligibility variable.

Moreover, the large under-reporting of RSA impedes a coherent comparison of recipients and eligible, as a large share of recipients would probably be classified in the eligible.

This estimation strategy could also introduce an endogeneity bias, as eligibility as well as RSA-recipient is a manipulable status: people may decrease their labor supply in order to

become eligible.

One could also restrict our estimation to people receiving the RMI before the reform, and observe whether their part-time work rate increases after the reform: this would avoid the problem of endogeneity, as RMI status is decided before the introduction of RSA, and cannot be influenced by it. However, the same problem would still present: to have a comparison group, we would have to create an eligibility variable, and, here again, RMI is largely under-reported.

The only exception to RSA universality are people aged less than 25 with no children. We can use this exogenous variation around 25 years of age and compare people over with those under 25.

A simple comparison between the stock of part-time employment (or transition probabilities) in the over-25 group before and after the introduction of RSA would be biased by the important labor-market changes occurring at the same time, in particular the decrease of labor demand. One cannot either merely compare the outcomes of both groups after the introduction of the RSA: as they are different in ages, they also probably have different levels of part-time employment. In order to get rid of time-unvarying differences between the two groups, we will use a double-difference estimation:

$$outcome = \alpha + \beta_1 * 2009q2 + \beta_2 * 25years + \beta_3 * 2009q2 * 25years + u$$

The coefficient of interest is β_3, which estimates the difference between the change in outcome before and after the introduction of the RSA (second quarter of 2009) for the 25-and-older group on the one hand and the less than 25 on the other hand. This method requires that both groups would have evolved the same way had the RSA not been implemented (even if they have different outcomes in levels, they should have similar changes in outcomes): we will see that this is a reasonable assumption.

Another necessary assumption is that the group that is non eligible has not been impacted by the RSA: in a partial equilibrium setting, as age is not manipulable, this is not a problem.

4.3 Difference-in-difference estimation

4.3.1 Whole 20-30 population

The difference-in-difference approach consists in comparing two groups: the 20-24-year old, and the 25-30-year old before and after the introduction of the RSA. The first one cannot benefit from the RSA and could not have benefited from the RMI (had the reform not taken place) either, whereas the second one can be eligible to RSA after 2009q2, and could have received RMI if the RSA had not been introduced.

Figure 11 : Part-time work as a share of total population (%) 1990-2010

Source : Enquête emploi. French non-student individuals.
Data corrected for seasonal variation and smoothed by moving average.
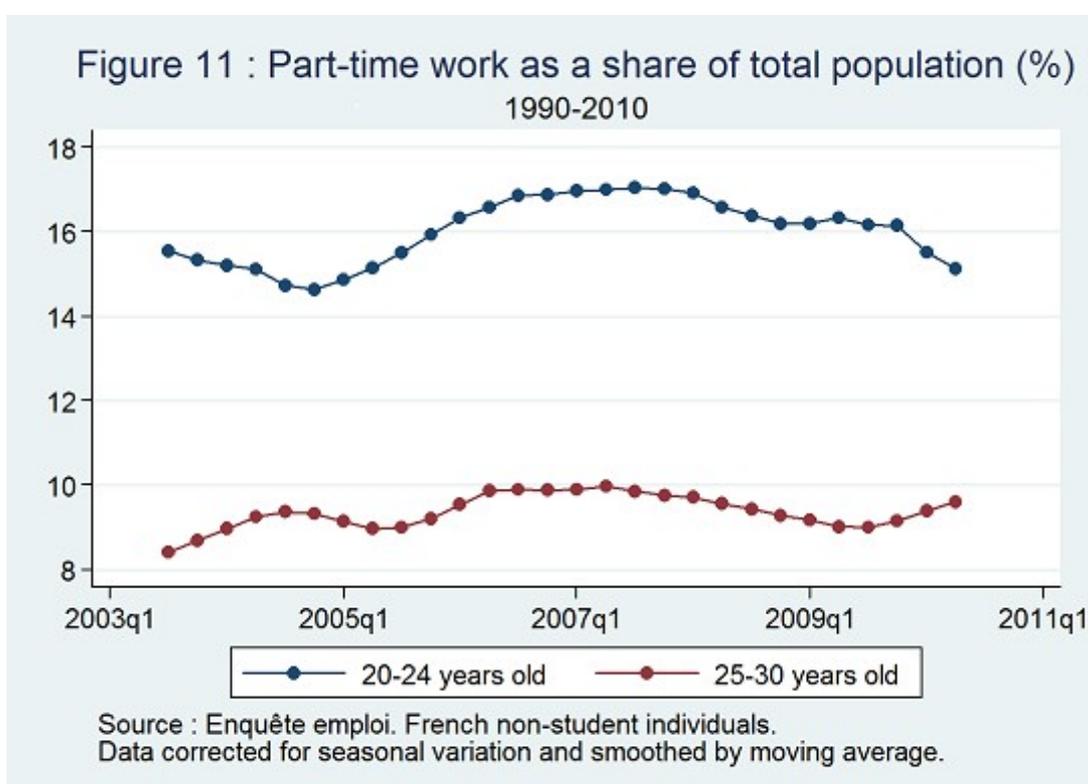
Figure 20 shows quite similar evolutions for the share of part-time in total population for the 20-24-year old and for the 25-30-year old, particularly from 2005 on, except for the period after 2009q2, where the share of part-time increases for the old part of the population, whereas it decreases for the young.

This similar evolution before the reform, and this relative increase of part-time work in

the older group after the reform allows to conduct a difference-in-difference analysis: because of the parallel evolutions of both groups in the ex-ante situation, we assume that, in the absence of the RSA, both groups would have kept having similar evolutions after the second quarter of 2009. Both groups are not similar in levels, but in evolution: measuring the change in the difference in part-time work levels for both groups after 2009q2 should yield an estimation of the effect of the RSA on part-time work.

Hence, we first regress a dummy for part-time work, equal to one if working part-time, and 0 otherwise (including non-employment), on a dummy for being after 2009q2, another dummy for being aged at least 25, and their interaction. The coefficient of interest is the one for the interaction term. The results are shown in table 3 column 1. One finds a positive non significant[3] effect of RSA on part-time. The positive sign is as expected, but the coefficient is not significantly different from zero.

We then try adding controls in the regression, and so for two reasons.

First, there could be some omitted variable bias in the first regressions: there are some variables that are correlated both with being over 25 and with working part-time: in particular, part-time work decreases with age, which could undermine the effect estimated in the first regression, as the dummy for being over 25, which we expect to have a positive effect, will also capture a negative age effect, in absence of an age variable. Moreover, the more diplomas one has, the less part-time one works, and among the 20-30 years old, the older are more educated, so that, again, in the absence of a diploma variable, the 25-years-old dummy captures the negative effect of diploma on part-time. Other variables like sex, living in cohabitation or number of children below 2 years old are also correlated with both age and part-time work.

We also have to use a variable for the quarter of the year: because the data stop at the end of 2010, we have only 6 quarters for the post-reform period, and we do not have the same proportion of each quarter in pre- and post-reform data; as the outcome may be influenced by seasonality, we need to control for quarter. Finally, dummies for year are also

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3 In this regression as in all the following ones, we use simplified standard errors: we do not take in account the characteristics of the survey, except the cluster for the individuals. This leads to an understimation of the standard error, which is not a problem, as they are already too large for the coefficient of interest to be significant.

added in order to control for potential cohort effects[4].

Second, adding controls allows to make sure that this absence of significant effect is not due to a composition effect, that is, to a change in the composition of the population, that could have offset the effect of the RSA. One could think, for example, that the RSA increased the proportion of part-time work for single people, but that, in the meantime, the proportion of single people in the population has decreased, thus diminishing the effect of the RSA on the whole population.

The results are shown in table 3, column 2, and exhibit no significant coefficient for the interaction term, that is even smaller than in the regression without controls.

We also tried running the same regressions without the years 2003 and 2004, as the evolution of part-time share in the two groups are less similar in those years. The results are very similar to those found with the whole 2003-2010 period.

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4 The quarter and year dummies are not shown in tables 3-10 in order to make the tables more readable. But they have been included in all the regressions presented in those tables.

Table 3: average marginal effects for probit regression of part-time work

	(1)	(2)
2009q2	-0.004	-0.002
	(0.03)	(0.04)
25 or more	-0.052***	-0.023***
	(0.02)	(0.03)
25*2009q2	**0.003**	**0.0004**
	(0.04)	**(0.04)**
Sexe		0.121***
		(0.02)
Age		-0.003***
		(0.00)
Single		0.021***
		(0.02)
Nb of children under 3 years old		0.010***
		(0.02)
Higher diploma		0.000
		(.)
Baccalauréat +2		-0.025***
		(0.03)
Baccalauréat		0.022***
		(0.02)
CAP/BEP		0.004
		(0.03)
BEPC		-0.000
		(0.04)
No diploma		0.002
		(0.03)
Constant	***	***
	(0.01)	(0.12)
N	242500	242490

On can decompose the global effect of the RSA on part-time work into multiple effects on different transitions : from part-time to non-employment, from part-time to full-time, from part-time to part-time, from non-employment to part-time and from full-time to part-time. In order to see if this absence of net global effect can be due to contradictory effects on transitions, we use the same difference-in-difference approach on the transitions.

The results are shown in tables 4 to 6. There is still no significant result, except for non-

employment-part-time transitions, for which the coefficient is significantly negative. However, me made the same regressions on other samples (cf infra), where it is not significant anymore. We also checked that this absence of results is not due to differential attrition: this is not the case, as the transitions toward attrition are not significantly affected by the RSA either.

Table 4: average marginal effects for probit regressions of transitions from nonemployment

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	0.008	-0.006	-0.012	0.008
	(0.09)	(0.07)	(0.06)	(0.06)
25 or more	0.001	-0.016**	0.049***	-0.029***
	(0.06)	(0.04)	(0.03)	(0.03)
25*2009q2	**-0.016****	**0.008**	**0.010**	**0.001**
	(0.08)	**(0.06)**	**(0.05)**	**(0.05)**
Sexe	0.020***	-0.062***	0.068***	-0.019***
	(0.03)	(0.02)	(0.02)	(0.02)
Age	-0.001	-0.001	0.012***	-0.010***
	(0.01)	(0.01)	(0.00)	(0.01)
Single	0.0001	-0.033***	0.016**	0.018***
	(0.03)	(0.02)	(0.02)	(0.02)
Nb of children under 3 years old	-0.015***	-0.058***	0.079***	-0.014***
	(0.03)	(0.03)	(0.02)	(0.02)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	-0.003	0.027***	-0.022*	-0.006
	(0.06)	(0.04)	(0.04)	(0.04)
Baccalauréat	-0.001	-0.023***	0.047***	-0.016*
	(0.05)	(0.04)	(0.03)	(0.03)
CAP/BEP	-0.005	-0.036***	0.071***	-0.022**
	(0.05)	(0.04)	(0.03)	(0.03)
BEPC	-0.013***	-0.055***	0.106***	-0.029**
	(0.06)	(0.05)	(0.04)	(0.04)
No diploma	-0.024***	-0.103***	0.147***	-0.025***
	(0.05)	(0.04)	(0.03)	(0.03)
Constant	***		***	**
	(0.22)	(0.16)	(0.12)	(0.13)
N	47636	47636	47636	47636

Table 5: average marginal effects for probit regression of transition from full-time

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	0.003	0.010	0.002	-0.018*
	(0.10)	(0.04)	(0.06)	(0.04)
25 or more	-0.001	0.029***	-0.008***	-0.018***
	(0.06)	(0.02)	(0.03)	(0.02)
25*2009q2	**-0.002**	**0.004**	**-0.002**	**0.004**
	(0.09)	**(0.03)**	**(0.05)**	**(0.03)**
Sexe	0.010***	-0.017***	0.014***	-0.007**
	(0.04)	(0.01)	(0.02)	(0.01)
Age	-0.000**	0.014***	-0.003***	-0.010***
	(0.01)	(0.00)	(0.01)	(0.00)
Single	0.001**	-0.032***	0.014***	0.016***
	(0.03)	(0.01)	(0.02)	(0.01)
Nb of children under 3 years old	0.008***	-0.005	0.011***	-0.020***
	(0.03)	(0.01)	(0.02)	(0.01)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	-0.002**	0.017***	-0.002	-0.013***
	(0.04)	(0.01)	(0.03)	(0.01)
Baccalauréat	0.001	0.011**	0.010***	-0.021***
	(0.04)	(0.01)	(0.03)	(0.01)
CAP/BEP	0.000	0.006	0.012***	-0.018***
	(0.05)	(0.02)	(0.03)	(0.02)
BEPC	-0.002	-0.007	0.023***	-0.016**
	(0.08)	(0.02)	(0.04)	(0.02)
No diploma	-0.000	-0.023***	0.027***	-0.005
	(0.08)	(0.02)	(0.04)	(0.02)
Constant	***	***	***	***
	(0.25)	(0.07)	(0.14)	(0.08)
N	149078.000	149078.000	149078.000	149078.000

Table 6: average marginal effects for probit regression of transition from part-time

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	0.028	-0.022	0.013	-0.021
	(0.07)	(0.12)	(0.11)	(0.08)
25 or more	0.012	-0.003	0.024***	-0.029**
	(0.05)	(0.07)	(0.06)	(0.05)
25*2009q2	**-0.010**	**0.019**	**-0.006**	**0.002**
	(0.07)	**(0.11)**	**(0.09)**	**(0.07)**
Sexe	0.050***	-0.026***	-0.009*	-0.013
	(0.03)	(0.05)	(0.04)	(0.03)
Age	0.017***	-0.001	-0.009***	-0.008***
	(0.01)	(0.01)	(0.01)	(0.01)
Single	-0.028***	-0.009**	0.016***	0.020**
	(0.03)	(0.04)	(0.04)	(0.03)
Nb of children under 3 years old	0.046***	-0.015**	-0.005	-0.030***
	(0.03)	(0.06)	(0.04)	(0.03)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	0.020	0.013**	-0.021***	-0.019
	(0.04)	(0.06)	(0.06)	(0.04)
Baccalauréat	0.027**	0.001	-0.013*	-0.016
	(0.03)	(0.05)	(0.05)	(0.04)
CAP/BEP	0.018	0.000	-0.005	-0.013
	(0.04)	(0.06)	(0.05)	(0.04)
BEPC	0.014	-0.011	0.024**	-0.032**
	(0.05)	(0.08)	(0.07)	(0.05)
No diploma	0.034**	-0.016**	0.010	-0.031**
	(0.05)	(0.07)	(0.06)	(0.05)
Constant	***	***		
	(0.19)	(0.27)	(0.24)	(0.20)
N	24352	24352	24352	24352

4.3.2 Different household types

As the incentives to work part-time created by the RSA are not the same for all household types, and some households types are even not affected at all by the reform, we look at the data for different household types, in order to see whether the lack of effect found in the first regressions could be due to the dilution of a real effect for some kinds of households only.

We separate households by marital status, and by the status (non-employed, working part-time, full-time) of the partner, if any. Individuals with a non-working partner and single individuals have the highest financial incentives to work part-time instead of not working, and even, in a large extent, instead of working full-time (Annex 7 of the evaluation committee report); the increase in financial gains for going to part-time is smaller for people with a partner working part-time, and it is null for people with a partner working full-time (see lines « increase in the financial gain to work part-time instead of not working » in table 11).

Here, one important problem is that the graphs do not show similar evolutions for younger and older groups, so that the underlying assumptions needed for difference-in-difference are not verified. Moreover, one do not observe any potential effect of the RSA on the graphs. Running the difference-in-difference estimation does not yield significant results either (we do not show those graphs and results here).

Grouping the households in two groups, one for which the incentives are large (single people, and couples with non-employed partner), and one for which they are small or nonexistent (couple with part-time and full-time working partner), in order to have more data, and so, more precision, gives a more convincing graph (figure 12), but does not yield more significant results (tables 7 to 10).

Hence, using different sub-groups confirm the absence of effect.

Figure 12 : Part-time work as a share of total population (%)
High-incentives households 1990-2010

Source : Enquête emploi. French non-student individuals.
Single persons and couple with a nonemployed partner only.
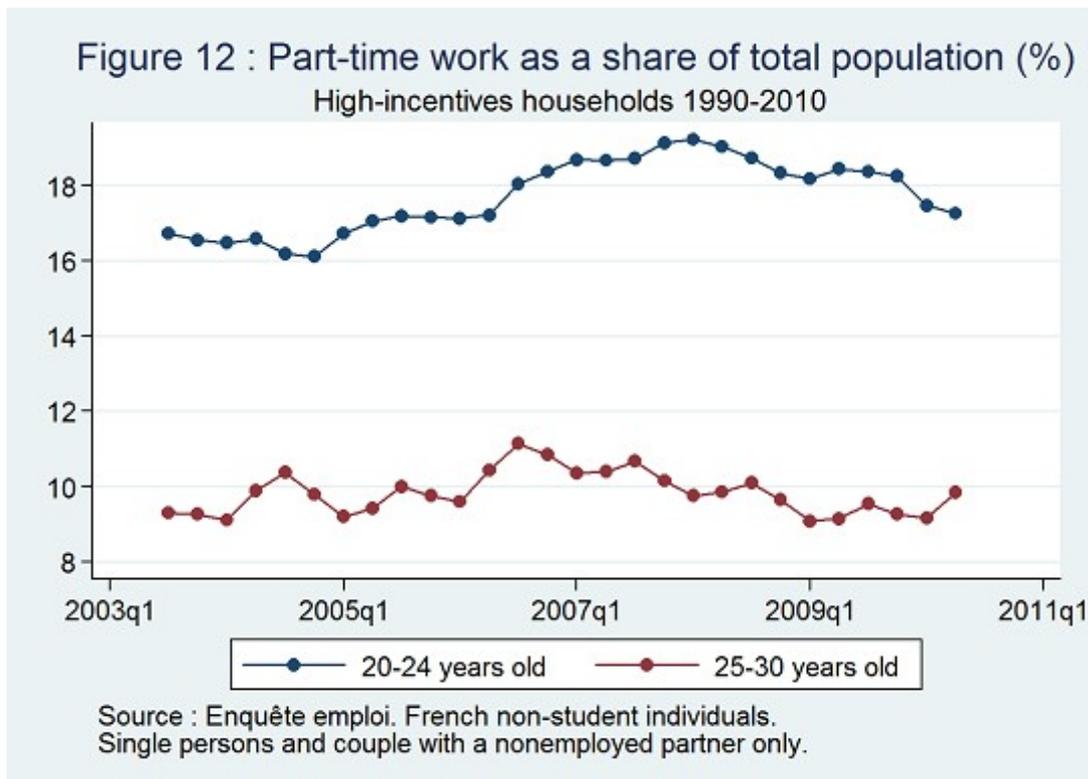
As a robustness check, we made all those estimations with a different sample, using only first-interview data. This is meant to avoid attrition problems: as labor-market status, and more particularly, the change in labor-market status may be linked to attrition (especially for people moving home), using all interviews could decrease the level of part-time estimated in the population, if people working part-time are more prone to non-response or to moving.

However, the results are very similar to those found with the whole sample, so that the absence of detected effect is not due to a problem of selective attrition.

Restricting the sample to low-diploma single and without-children people in order to make our results comparable with those of Bargain and Vicard did not yield any significant either.

Table 7: average marginal effect for probit regression of part-time work for high-incentives households

	(1)	(2)
2009q2	-0.002	0.003
	(0.04)	(0.05)
25 or more	-0.071***	-0.017***
	(0.02)	(0.04)
25*2009q2	**-0.003**	**-0.006**
	(0.05)	**(0.05)**
Sexe		0.094***
		(0.02)
Age		-0.008***
		(0.01)
Single		0.017***
		(0.03)
Nb of children under 3 years old		-0.039***
		(0.04)
Higher diploma		0.000
		(.)
Baccalauréat +2		-0.037***
		(0.04)
Baccalauréat		0.016**
		(0.03)
CAP/BEP		-0.014**
		(0.04)
BEPC		-0.009
		(0.05)
No diploma		-0.006
		(0.05)
Constant	***	***
	(0.02)	(0.17)
N	120554	120549

Table 8: average marginal effects for probit regression of transitions from part-time for high incentives households

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	0.074	-0.061*	-0.008	0.004
	(0.22)	(0.27)	(0.32)	(0.25)
25 or more	0.045	-0.014	0.031	-0.065*
	(0.14)	(0.20)	(0.17)	(0.16)
25*2009q2	**-0.052**	**0.001**	**0.020**	**0.031**
	(0.19)	(0.27)	(0.24)	(0.21)
Sexe	0.062**	-0.036***	-0.017	-0.013
	(0.08)	(0.12)	(0.09)	(0.09)
Age	0.005	0.002	-0.012**	0.006
	(0.02)	(0.04)	(0.03)	(0.03)
Single	0.045	-0.027	-0.002	-0.014
	(0.10)	(0.15)	(0.12)	(0.11)
Nb of children under 3 years old	-0.099**	0.011	0.021	0.062*
	(0.14)	(0.21)	(0.16)	(0.15)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	0.030	0.018	-0.016	-0.034
	(0.13)	(0.21)	(0.15)	(0.15)
Baccalauréat	0.046	-0.006	-0.020	-0.022
	(0.10)	(0.15)	(0.12)	(0.13)
CAP/BEP	0.019	-0.021	-0.018	0.015
	(0.12)	(0.18)	(0.14)	(0.15)
BEPC	0.050	-0.028	-0.034	0.001
	(0.15)	(0.25)	(0.18)	(0.17)
No diploma	0.080*	-0.023	-0.031	-0.028
	(0.12)	(0.17)	(0.15)	(0.15)
Constant				*
	(0.61)	(0.84)	(0.68)	(0.76)
N	2211	2211	2211	2211

Table 9: average marginal effects for probit regression of transitions from full-time for high incentives households

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	0.001	0.014	0.009	-0.037
	(0.38)	(0.11)	(0.16)	(0.12)
25 or more	0.005*	0.030*	-0.004	-0.028*
	(0.17)	(0.06)	(0.08)	(0.07)
25*2009q2	**-0.008**	**0.001**	**0.002**	**0.009**
	(0.32)	(0.09)	(0.15)	(0.09)
Sexe	0.006***	-0.028***	0.015***	0.005
	(0.09)	(0.03)	(0.05)	(0.04)
Age	-0.002***	0.011***	-0.006***	-0.004
	(0.03)	(0.01)	(0.01)	(0.01)
Single	-0.001	-0.006	0.010	-0.003
	(0.13)	(0.04)	(0.07)	(0.05)
Nb of children under 3 years old	-0.002	-0.000	0.005	-0.006
	(0.18)	(0.05)	(0.08)	(0.06)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	-0.004	0.001	-0.004	0.006
	(0.16)	(0.05)	(0.08)	(0.06)
Baccalauréat	0.001	-0.001	0.019***	-0.018
	(0.13)	(0.05)	(0.07)	(0.05)
CAP/BEP	0.002	-0.010	0.016**	-0.005
	(0.14)	(0.05)	(0.07)	(0.06)
BEPC	-0.000	0.012	0.019*	-0.031
	(0.19)	(0.08)	(0.10)	(0.09)
No diploma	0.001	-0.049***	0.048***	-0.004
	(0.19)	(0.06)	(0.09)	(0.07)
Constant			**	**
	(0.78)	(0.24)	(0.34)	(0.26)
N	13514.000	13514.000	13514.000	13514.000

Table 10: average marginal effects for probit regression of transitions from nonemployment for high incentives households

	(1) To part-time	(2) To full-time	(3) To nonemployment	(4) Attrition
2009q2	-0.035	-0.024	0.066	-0.003
	(0.26)	(0.21)	(0.15)	(0.15)
25 or more	0.022	-0.051**	0.060*	-0.026
	(0.17)	(0.11)	(0.09)	(0.10)
25*2009q2	**0.002**	**0.030**	**-0.033**	**0.001**
	(0.22)	**(0.18)**	**(0.13)**	**(0.13)**
Sexe	0.027***	-0.069***	0.045***	-0.004
	(0.09)	(0.06)	(0.05)	(0.05)
Age	-0.007***	0.002	0.008*	-0.003
	(0.03)	(0.02)	(0.01)	(0.02)
Single	0.002	-0.043***	0.048**	-0.006
	(0.10)	(0.08)	(0.06)	(0.06)
Nb of children under 3 years old	-0.020**	-0.040***	0.064***	-0.007
	(0.11)	(0.08)	(0.06)	(0.06)
Higher diploma	0	0	0	0
	(.)	(.)	(.)	(.)
Baccalauréat +2	0.001	0.086***	-0.103***	-0.006
	(0.15)	(0.12)	(0.10)	(0.12)
Baccalauréat	0.007	-0.007	-0.027	0.029
	(0.13)	(0.11)	(0.08)	(0.09)
CAP/BEP	-0.007	-0.011	0.040	-0.023
	(0.13)	(0.10)	(0.08)	(0.09)
BEPC	-0.014	-0.034	0.050	0.007
	(0.15)	(0.13)	(0.09)	(0.10)
No diploma	-0.023*	-0.098***	0.121***	-0.010
	(0.16)	(0.11)	(0.08)	(0.09)
Constant				*
	(0.70)	(0.47)	(0.35)	(0.39)
N	5116	5116	5116	5116

4.4 Power calculation: is the sample too small to detect the effect ?

As we find no empirical evidence of any effect of the RSA on part-time employment,

we want to know whether this is due to the effective absence of any effect, or to the lack of data, which could impede the detection of a small, but real, effect.

In order to do so, we compute the power of the test of the coefficient found in the regressions: knowing the expected effect, we can compute the power, that is one minus the probability of having a real effect when none is detected. If the power is high, it means that the absence of detection of any effect reflects with a high probability the absence of a real effect. On the reverse, a small power indicates that, even if there is a real effect, it would have only a small probability of being detected, meaning that the sample is too small to detect the effect or, depending on the point of view, that the effect is to small to be detected by this sample.

The power κ depends on the real size of the theoretical effect TE, on the level of significance α, and on the standard error of the estimate $\sigma_{\hat{\beta}}$:

$$TE = (t_{\alpha/2} + t_{1-\kappa}\sigma_{\hat{\beta}})$$, with t being the t-statistic of the Gaussian distribution.

with: $$\sigma_{\hat{\beta}}^2 = \frac{\sigma^2}{(1-\rho)\sum(x-\bar{x})^2}$$

and $\sum(x-\bar{x})^2 = \bar{x}(1-\bar{x})N$, which yields the following formula for the computation of the power:

$$t_{(1-\kappa)} = \frac{TE}{\sqrt{\left(\frac{\sigma^2}{(1-\rho)\bar{x}(1-\bar{x})N}\right)}} - t_{(\alpha/2)}$$

σ^2 is the variance of the baseline output variable, here the variable for part-time work; ρ is the coefficient of correlation between the interest variable x (here, it is 2009q2*25years) and the other right-hand side variables; N is the number of observations, and $\bar{x}$ is the proportion of the population with x equal to 1.

The formula is simple, and quite intuitive: the higher the theoretical effect and the number of observations, the larger the power; the higher the variation in the baseline output (that is the output in the case where there would have been no reform), the smaller the power: the more volatile the output before the reform, the hardest it is to detect a change after the reform; the higher the significance level, the higher the power; the more balanced

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the sample is between treated and untreated individuals, the higher the power.

Nevertheless, the implementation of the formula presents some difficulties, in particular for the computation of the theoretical effect.

4.4.1 Computation of the theoretical effect

The effect we are studying when looking at the regressions' results is the difference in the probability of working part-time when aged 25 to 30 (and thus, potentially RSA-eligible) versus 20 to 24 (not eligible).

The theoretical effect depends both on the change in the financial incentives to work induced by the introduction of the RSA and on the elasticity of labor-supply to those incentives.

The reform increases the incentives to work part-time but also the incentives to work full-time, though less (see table in Appendix). The theoretical effect of the RSA on part-time thus depends of both changes in incentives to part and full-time. In order to simplify the problem, we will consider only the transitions between non-employment and part-time work, thus omitting the changes in the transitions from full-time to part-time, and from non-employment to full-time. This assumption is justified by the fact that the changes in incentives to work full-time are much less important than those of the incentives to work part-time. However, this is still quite a strong assumption, which we will have to take into account.

Particularly, excluding the effect on transitions from full-time to part-time decreases our theoretical effect, as we omit a potential source of increase in part-time. Hence, the theoretical effect that we compute here should be seen as an lower bound of the real theoretical effect.

Thus, we will say that the theoretical percentage change in the proportion of people working part-time is equal to the percentage change in financial incentives to work part-time times the elasticity of labor supply.

4.4.1.1 The change in financial gains to work part-time

As table 11 shows, the substitution of the RMI by the RSA globally increases the incentive to work part-time, but the level of this change in incentive depends on the type of households (it changes across cohabitation status, number of children, and labor-market status of the partner). Hence, we will compute the expected effect for each subgroup and add them to obtain the total expected effect of RSA on part-time work. The calculation of the theoretical effect for each subgroup is done using data from the pre-RSA period on French non-student people aged 25-30. We use data from the three quarters before the reform as a baseline, because we need to know the level of part-time absent the reform in order to compute σ^2 : we thus make the hypothesis that, in absence of the reform, the proportion of part-time work in each subgroup would have kept similar to the one in the three quarters before the reform.

The computation of the change in gains to work part-time is made the following way: we first compute the respective gains of working part-time instead of not working before the introduction of RSA and after. This is equal to the difference between the RMI or RSA *socle* and the labor income earned (plus the RSA *activité* in the after-situation).

$$financial\ gain_{before} = labor\ income - RMI$$

$$financial\ gain_{after} = labor\ income - RSA\ socle + RSA\ activité$$

We then compute the difference between the gain from working part-time instead of not working before and after the introduction of the RSA[5]. This difference is then divided by the gain from working part-time instead of not working at the time of the RMI. This yields the percentage change in the financial gains to work part-time. This change is computed for each household type.

$$(financial\ gain_{after} - financial\ gain_{before}) / financial\ gain_{before}$$

The effects for the different subgroups are added using weights for each subgroup. The weights are proportional to the share of the subgroup in the total population in 2009q2 and to the proportion of people receiving the RSA in the subgroup.

As we have no direct information on the number of people receiving the RSA in each

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5 We used for this computation the figures given in the evaluation committee report, and shown in Appendix.

subgroup, we use the own-built eligibility variable, making the assumption that the take-up is the same in all subgroups. Using the own-built eligibility variable amounts to taking into account the fact that the different household types have in average different incomes, and thus, a different probability of benefiting from the RSA.

Table 11 shows the increase in the financial gain to work part-time and the corresponding theoretical increase in part-time work, as well as the weights used for each household type.

Table 11: Theoretical effects of RSA on part-time work

	single with no child	single with 1 child	single with 2 children	single with 3 children
%age of part-time work before RSA (3 quarter average)	9,49	7,36	7,52	14,41
increase in the financial gain to work part-time instead of not working	79,00%	400,00%	308,00%	62,00%
Increase of part-time work in %age for a .5 elasticity	39,50%	200,00%	154,00%	31,00%
Increase in the %age of part-time work	3,75	14,72	11,58	4,47
Population after RSA	1380353	140780	38051	16282
Proportion of RSA eligible in %	54,35	57,54	60,56	67,29
Weight	750221,8555	81004,812	23043,6856	10956,1578

	couple with partner working part-time no child	couple with partner working part-time with 1 child	couple with partner working part-time with 2 children	couple with partner working part-time with 3 chlidren
%age of part-time work before RSA (3 quarter average)	6,49	0	0	0
increase in the financial gain to work part-time instead of not working	11,00%	41,00%	45,00%	7,00%
Increase of part-time work in %age for a .5 elasticity	5,50%	20,50%	22,50%	3,50%
Increase in the %age of part-time work	0,36	0	0	0
Population after RSA	222926	1637	74	75
Proportion of RSA eligible	25.70	19,88	18.60	22.67
Weight	57291,982	294,66	13,764	17,0025

	couple with non-employed partner no child	couple with non-employed partner with 1 child	couple with non-employed partner with 2 children	couple with non-employed partner with 3 children
%age of part-time work before RSA (3 quarter average)	5,2	0	10,28	0
increase in the financial gain to work part-time instead of not working	315,00%	291,00%	267,00%	244,00%
Increase of part-time work in %age for a .5 elasticity	157,50%	145,50%	133,50%	122,00%
Increase in the %age of part-time work	8,19	0	13,72	0
Population after RSA	480681	5545	1706	867
Proportion of RSA eligible	68.80	50.88	46.02	50,49
Weight	330708,528	2821,296	785,1012	437,7483

	couple with partner working full-time no child	couple with partner working full-time with 1 child	couple with partner working full-time with 2 children	couple with partner working full-time with 3 children
%age of part-time work before RSA (3 quarter average)	11,26	1,26	0	0
increase in the financial gain to work part-time instead of not working	0	0	0	0
Increase of part-time work in %age for a .5 elasticity	0	0	0	0
Increase in the %age of part-time work	0	0	0	0
Population after RSA	2101405	10167	1696	1365
Proportion of RSA eligible	16.75	14,11	13.95	18,52
Weight	351985,3375	1434,5637	236,592	252,798

4.4.1.2 Choice of the elasticity of labor-supply

As said in the introduction, there is little empirical literature on the effect of financial incentives on employment. Piketty (1998) finds an elasticity of 0.6-1 for women.

Considering than women's labor supply is larger than men's, we choose an elasticity of 0.5, which is already quite large.

According to the evaluation committee report, around 7% of the population receive the RSA, so we have to multiply the effect we find by 7% to find the effect for the global population. For an elasticity of 0.5, we find an effect of 0.29% (4.16 x 0,07).

4.4.2 Computation of the variance of the estimate

4.4.2.1 Computation of the variance of the part-time work variable

We need the baseline variance of the output, that is the variance of part-time work variable that we would have had the RSA not been introduced. Making the assumption that the variance of part-time work did not change over the period 2003-2010 except for the effect of the RSA, we use the variance of part-time work on the period 2003-2009q2, that is the period preceding the implementation of the reform.

Looking at the data, we find that the variance of the part-time work variable for a sample reduced to French non-student people aged 20 to 30 before 2009q2 is 0.10.

4.4.2.2 Computation of the variance of the treatment variable

The coefficient of correlation between the explanatory interaction term 2009q2*25years and the other explanatory variables is computed by regressing 2009q2*25years on the other explanatory variables: ρ is the R^2 of the regression. We find ρ equal to 0.75.

We also need to know $\bar{x}(1-\bar{x})$, which is equal to the proportion of people in our sample with 2009q2*25years=1 times the proportion of people with 2009q2*25years=0. Here, we have $\bar{x}$ equal to 0.065.

The number of observations N is 34945.

4.4.3 The power is very small

Applying the formula yields, for a level of significance α=10%, a negative power: even

if the real effect was as high as the theoretical effect, the sample is too small to detect this effect. However, this theoretical effect is a lower bound, which nuances this result. Applying the formula for a power of 80%, we find that 6 million observations are needed in order to detect the effect. Even if we widened the sample from the 20-30 to the whole population, we would have only about 2 million observations.

However, as the value chosen for the elasticity of labor supply is quite uncertain, one can try and compute the minimum detectable effect for an acceptable power, let us say 80%, and deduce the elasticity of labor-supply it corresponds to.

Applying the same formula for a power of 80%, we find that the minimum detectable effect is equal to 0.0108, which is 3.7 times the effect with an elasticity of 0.5. This corresponds to an elasticity of around 1.86, which is impossible. But here, we still have to take into account that transitions from the extensive margin between part-time and full-time has been omitted in the computation of the theoretical effect, so that an elasticity of 1.86 is only an upper bound for the elasticity that would be really needed to detect the effect.

Hence, it could be that the theoretical effect as we computed it is too small to be detected by the sample we use. If this is the case, we need more hindsight and post-RSA data in order to detect the effect of the RSA on the labor-market with the French labor-force survey.

5 Conclusion

We have seen that part-time employment has been influenced by previous public policies, particularly between 1992 and 1998, where part-time has risen, mainly through an increase in the duration spent in part-time jobs.

We have found no evidence for a part-time trap, nor for any effect of the RSA on the increase in part-time nor in full-time employment. This could have many explanations: first, such a supply-oriented policy may have limited effects in times of scare labor demand.

Moreover, even if the change in the financial gains to work instead of not working are important, they may remain without effect if they are under the reservation wage. Elasticities for young people are also quite small, which could explain the absence of effect. One last explication might be the lack of data, in which case a possible effect could be detected in a few years, when data on a longer period are available.

This work could be extended by looking at the numbers of hours one is ready to work, instead of the real labor-market outcomes, as this information is available in the French labor-force survey: this could be a better way of measuring labor-supply, in order to get rid of the limitations due to the absence of strong labor demand in the period.

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Appendix

Change in the financial gains from working after the introduction of the RSA :

SITUATION FAMILIALE DU MENAGE		REPRISE D'UN EMPLOI A ...			
		Mi-temps		Temps plein	
		rSa majoré / API	*rSa* / RMI	*rSa* majoré / API	*rSa* / RMI
Célibataire inactif	Sans enfant	-	144	-	0
	1 enfant	261	264	204	110
	2 enfants	258	261	248	95
	3 enfants	255	258	122	0
Couple inactif puis monoactif	Sans enfant	-	260	-	106
	1 enfant	-	257	-	215
	2 enfants	-	254	-	226
	3 enfants	-	251	-	87
Couple monoactif (tps plein) puis biactif	Sans enfant	-	0	-	0
	1 enfant	-	0	-	0
	2 enfants	-	0	-	0
	3 enfants	-	0	-	0
Couple monoactif (tps partiel) puis biactif	Sans enfant	-	57	-	0
	1 enfant	-	166	-	0
	2 enfants	-	177	-	0
	3 enfants	-	38	-	0

(1) Variations des gains financiers entre la situation où le dispositif RMI-API est remplacé par le *rSa* à partir de 2010 - hypothèse (b) - et celle où le dispositif RMI-API est stationnaire - hypothèse (a). Les gains sont calculés comme les variations du revenu entre 2009 et 2011. Calculs en euros constants.

Lecture : Un couple inactif puis monoactif à mi-temps, avec un enfant, gagne 260 euros par mois avec la mise en place du *rSa* par rapport à la situation où le dispositif RMI est stationnaire.

Source: Report of the evaluation committee.