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Labor Market Flows after the redesign of the Portuguese Labor Force Survey

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ISEG/UTL, Fevereiro de 2014

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Viva Voce Exam in: February 07, 2014.

Abstract

In the first quarter of 2011, the Portuguese Labor Force Survey underwent a major redesign. The redesign substantially changed the methodology of collecting data and concepts of employment, unemployment and inactivity. Since then, the labor market flows became substantially high compared with historical standards. This study documents the definitional changes and impacts of the redesign on labor market flows data. In addition, it provides a systematic study about the Portuguese labor market flows for the period between 1999 and 2012. LFS microdata are used to compute labor market flows in the usual three-state set-up, the flows disaggregated by micro-characteristics of the respondents and the flows in a four state set-up, with employment disaggregated into permanent and temporary, and inactivity disaggregated into those who *want a job* and those who *do not want a job*. Then, we make use of regression discontinuity methods to investigate the redesign effect in the flows series, controlling their cyclical and seasonal movements. The results suggest that the jump in the flows series is mostly explained by the redesign's effect, despite of the countercyclical pattern found at some flows. Furthermore, the redesign effect seems to concentrate in specific groups, namely on the employed with a fixed-term contract, on the inactives who *want a job* and on the older and less educated respondents. On a second stage, we apply a set of cross-sectional and longitudinal methods to model the effect of the redesign in the probability to move out of employment. The new methodology appears to have increased the probability to record transitions out of employment. This appears to be more a result of a redesign effect caused by the major methodological changes than of differences between samples that could positively influence the likelihood to observe a higher level of transitions out of employment after the redesign.

Keywords: labor market dynamics, labor market flows, labor force survey.

JEL codes: E24, E32, J60.

Os Fluxos do Mercado de Trabalho após as alterações ao Inquérito ao Emprego

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Provas concluídas em: 7 de Fevereiro de 2014.

Resumo

No primeiro trimestre de 2011, o Inquérito ao Emprego Português foi amplamente redesenhado, alterando-se substancialmente a metodologia de recolha de dados, bem como os conceitos estatísticos de emprego, desemprego e inactividade. Desde então, os fluxos entre estados do mercado de trabalho tornaram-se substancialmente elevados, considerando o seu padrão histórico. O presente estudo descreve as alterações conceptuais que advieram do redesenho do inquérito e pretende analisar as suas implicações nas séries dos fluxos. De forma complementar, é feito um estudo sistemático sobre os fluxos entre estados do mercado de trabalho em Portugal durante o período de 1999 a 2012. A partir dos microdados do Inquérito ao Emprego são calculados os fluxos agregados entre os três estados habituais do mercado de trabalho, estes são posteriormente desagregados por características micro dos inquiridos, e por último calculam-se os fluxos num modelo a quatro estados, onde o emprego é desagregado entre temporário e permanente, e a inactividade é desagregada entre inactivos que *querem um emprego* e inactivos que *não querem um emprego*. Em seguida, usamos métodos de regressão descontínua para investigar os efeitos das alterações ao inquérito nas séries dos fluxos, controlando as suas flutuações cíclicas e sazonais. Os resultados sugerem que o aumento brusco dos fluxos é fundamentalmente devido ao redesenho do Inquérito ao Emprego, não obstante do comportamento contracíclico exibido por algumas séries. Adicionalmente, existe evidência que os fluxos dos empregados com contractos de trabalho a termo, dos inactivos que *querem um emprego* e dos indivíduos de idade superior e menor escolaridade, foram particularmente atingidos pelas alterações metodológicas. Num fase posterior, são usados métodos seccionais e longitudinais para quantificar o efeito do redesenho na probabilidade de transitar do emprego para o não-emprego. Os resultados sugerem que sob o novo inquérito a probabilidade de se registarem transições do emprego para o não-emprego é superior, comparativamente com o anterior método. Contudo, tal parece resultar mais das consideráveis alterações metodológicas, do que de alterações na composição da amostra do novo inquérito que favoreçam a observação de transições do emprego para o não-emprego.

Palavras-chave: dinâmica do mercado de trabalho, fluxos do mercado de trabalho, inquérito ao emprego.

Códigos JEL: E24, E32, J60.

Acknowledgments

I would like to express my gratitude to Álvaro A. Novo and Mário Centeno for their guidance throughout this study. I am also grateful to Lucena Vieira for the very useful tips she gave me about the computation of worker flows. This study benefited from financial support of Banco de Portugal, which is also gratefully acknowledged.

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1 Introduction

The labor market flows are crucial to our understanding of labor market dynamics. They drive movements in aggregated indicators such as the employment, unemployment and inactivity rates, their size is frequently taken as a proxy of the labor market flexibility and they lie at the heart of state-of-art of search and matching models of unemployment (Mortensen and Pissarides, 1994).

In the first quarter of 2011, the Portuguese Labor Force Survey (LFS) was widely redesigned, which caused major changes either in the definitions of the labor market states or in the methodology used to collect data. Since then, the labor market flows became substantially high compared with historical standards. The aim of this paper is to provide a systematic study of the definitional changes and impacts of the redesign on the labor market flows. In addition, it establishes a number of key facts about the Portuguese labor market flows for the period between 1999 and 2012.

To address this issues, we follow two main empirical strategies. In the first, regression discontinuity methods are used to quantify the redesign's effect on the aggregated flow series, controlling their seasonal and cyclical properties. On a second stage, this exercise is repeated for labor market flows disaggregated by age and education levels of the respondents. In order to consider the two-tier structure of the Portuguese labor market and the differential behavior within the pool of inactives, we first disaggregate employment between fixed-term and open-ended contracts, and then inactivity between those who *want a job* and those who *do not want a job*.

Overall, the redesign is found to explain a large part of the jump of the flows. In particular, the discontinuity in flows between employment and unemployment seems to be mostly explained by the effect of the redesign in the older respondents with a fixed-term contract. Similarly, the discontinuity in flows between employment and inactivity appears to be due to the effect of the redesign in flows of the older and less educated respondents moving between a fixed-term contract and inactivity. In addition, evidence suggests that the jump in flows between unemployment and inactivity is determined by the redesign impact in flows between the unemployed and the inactives who *want a job* (the smallest group within the pool of inactives).

Regarding the business cycle characteristics of the flows, one finds that flows from employment into unemployment are countercyclical, as well as flows between inactivity and unemployment, and flows from permanent employment into temporary employment. In contrast, the flows from the inactives who *want a job* into unemployment appear to be procyclical. On the other hand, we find that the reaction of the flows series to recessions is not significantly different between recessionary quarters before and after the redesign. Hence, it is essentially the redesign that determines the jump of the flows, even when they exhibit a significant countercyclical behavior.

In the second approach, we apply cross-sectional and longitudinal methods to model the probability to exit employment over the samples *before* and *after* the redesign. Overall, the redesign appears to have increased the probability to record transitions out of employment. Moreover, the out-of-sample transitions computed over the sample *before* using the coefficients fitted in the sample *after*, suggest that the methodological changes may lie behind the higher level of employment exits *after*, despite the compositional differences between samples.

The dissertation is organized as follows. Section 2 contains preliminary concepts and conventions and provides an overview of the theoretical models and previous evidence on the cyclical properties of labor market flows. Section 3 describes the definitional changes brought by the redesign. Section 4 quantifies the impact of the redesign in the aggregated flows, the flows disaggregated by micro-characteristics and in the four-state set-up, with employment and inactivity split as mentioned above. Section 5 models the redesign's effect in the probability to exit employment and computes the average transitions *before* with the coefficients fitted *after*. Section 6 concludes.

2 Preliminary Concepts

2.1 Labor market flows

We begin by presenting the fundamental equations that describe the evolution of the stock of employed E, the stock of unemployed U and the stock of inactive or out of the labor force I, which sum the population W (i.e., $W = E + U + I$). Adding the first two pools it gives us the labor force L (i.e., $L = E + U$). The unemployment rate is given by $u = \frac{U}{L}$. Total employment evolves according to the following difference equation:

$$E_t = E_{t-1} + N_t^{UE} + N_t^{IE} - N_t^{EU} - N_t^{EI}, \tag{2.1}$$

where N_t^{XY} is the labor market flow from state X into state Y, i.e., the number of people entering Y from X between $t-1$ and t, where $X, Y \in \{E, U, I\}$. Alternatively eq. (2.1) may be written as a function of the hazard rates λ_t^{XY} (i.e., the probability to move from one state to another):

$$E_t = E_{t-1} - (\lambda_t^{EI} + \lambda_t^{EU})E_{t-1} + \lambda_t^{IE} I_{t-1} + \lambda_t^{UE} U_{t-1}. \tag{2.2}$$

A similar decomposition can be performed for unemployment and inactivity:

$$U_t = U_{t-1} + N_t^{EU} + N_t^{IU} - N_t^{UE} - N_t^{UI}, \tag{2.3}$$

$$I_t = I_{t-1} + N_t^{UI} + N_t^{EI} - N_t^{IU} - N_t^{IE}. \tag{2.4}$$

Alternatively, focusing on the hazard rates:

$$U_t = U_{t-1} - (\lambda_t^{UE} + \lambda_t^{UI})U_{t-1} + \lambda_t^{EU} E_{t-1} + \lambda_{t-1}^{IU} I_{t-1}, \tag{2.5}$$

$$I_t = I_{t-1} - (\lambda_t^{IE} + \lambda_t^{IU})I_{t-1} + \lambda_t^{EI} E_{t-1} + \lambda_t^{UI} U_{t-1}. \tag{2.6}$$

Before proceeding, it is useful to outline our notation and clarify the precise meaning of some concepts of which we will make use. Research on labor market flows considers two varieties of flows: gross flows of workers and flows of jobs. Job flows measure whether a new position has been created or destroyed by a firm, rather than changes in the labor status of a worker. The gross worker flows measure transitions in labor market status of workers. Throughout this paper we will focus only on gross worker flows, to which we indistinctively refer as labor market flows. We use the symbol "$\rightarrow$" to denote a flow from one labor market state to another (for example, the flows from employment into unemployment are abbreviated as E$\rightarrow$U), we also use the symbol "$\leftrightarrow$" to denote flows between two labor market states (e.g. "E$\leftrightarrow$U" will denote E$\rightarrow$U and U$\rightarrow$E flows).

Labor market flows have been studied through two main approaches: the analysis of worker flows or the analysis of the hazard rates. Some authors like Blanchard and Diamond (1990), Burda and Wyplosz (1994) or Davis et al. (2006) focus on worker flows, while more recent studies of Fujita and Ramey (2009) or Shimer (2012) focus on hazard rates. The two perspectives could be considered complementary and numerous authors explore both in the analysis of labor market. We will also explore both, but more emphasis will be given to the flows approach.

2.2 Theoretical perspectives

Research by Blanchard and Diamond (1990) suggests a model of *primary* and *secondary* workers to justify why flows between employment and unemployment were markedly different from those between employment and inactivity. In their model, primary workers have strong attachment to the labor force, brief spells of unemployment and only separate from jobs involuntarily. In contrast, secondary workers have much weaker labor force attachment and are likely to spend significant time both in unemployment and inactivity. Firms perceive these workers differently and, as consequence, prefer to hire primary workers when available, and they are mostly available during recessions when masses of them suffer involuntary separations. Secondary workers are hired only in booms, when primary workers are not available. Since secondary workers are often inactive this means that flows from inactivity to employment are procyclical (that is, they go up in upturns and down in downturns).

Later, Blanchard and Diamond (1992) argue that we might expect movements from employment to unemployment (or inactivity) to be countercyclical, however flows from unemployment to employment are not necessarily procyclical, as we might intuitively think. In fact, their proposed matching function implies that, *ceteris paribus*, a larger stock of unemployed may lead to more hires, such that flows from unemployment to employment may actually increase during a recession, still the associate hazard rate necessarily goes down.

Pissarides (2000) also suggests that we might expect flows from inactivity into both employment and unemployment to be procyclical, particularly as labor market tightness rises and as the employment rate increases.

We have been highlighting the predictions of theoretical models for the cyclicality of flows between the different labor market states, however when discussing the cyclical properties of worker flows one also needs to take some account what Bleakley et al. (1999) called *movements at other frequencies*, i.e., other relevant factors to explain worker flow fluctuations than business cycle phases. According to the authors, these movements at other frequencies occur at a macro level (*higher frequencies*) and at a micro level (*lower frequencies*). At the higher frequencies are essentially movements related with seasonal factors (e.g., the movements in and out of the labor force related to the academic calendar) while at the lower frequencies are movements related with changes in characteristics of the population (age structure, participation of women, education and others).

To summarize, we expect the flows out of employment to be countercyclical, flows from inactivity into both employment and unemployment to be procyclical, and no clear pattern for flows from unemployment to inactivity. The hazard rate from unemployment to employment should be procyclical, while the actual flow may be countercyclical. Finally, seasonal factors and changes in demographics also play a major role in the fluctuations of worker flows data.

2.3 Previous empirical evidence

There is a number of empirical studies that focus on individual European countries, examples are Bell and Smith (2002) or more recently Gomes (2012) for United Kingdom, Schmidt (1999) for Germany, Hairault et al. (2012) for France, Balakrishnan (2001) or Silva and Vázquez-Grenno (2012) for Spain and Blanchard and Portugal (2001) or Centeno and Novo (2013) for Portugal. Burda and Wyplosz (1994) built a series of stylized facts for France, Germany, Spain and UK.

The findings of the cited studies are broadly consistent with the existence of a common pattern for the cyclical properties of labor market flows across countries. Overall, flows from employment to unemployment are found to be countercyclical, as the associated hazard rate. The reverse flow is also found to be countercyclical, while its hazard rate is procyclical. Flows between inactivity and employment appear to be procyclical, while flows between inactivity and unemployment are countercyclical. Contradictory evidence was found by Balakrishnan (2001) for Spain and by Gomes (2012) for UK, where unemployment outflows tend to be pro rather than countercyclical. In the case of the UK, no clear pattern is found for flows from employment to inactivity (while for the reverse flow a procyclical pattern emerge, consistently with the cross-country pattern).

Blanchard and Diamond (1990) and Bleakley et al. (1999) reached a number of common findings about the cyclicality and other properties of gross worker flows in the US, even though the two studies used different sample periods.[1] Flows between unemployment and employment are found to be countercyclical, whereas flows between inactivity and employment are found to be procyclical and no clear pattern is found for flows between inactivity and unemployment. Blanchard and Diamond explain this through their model of primary and secondary workers discussed previously. Bleakley et al. (1999) go further and find that the volatility of employment outflows is significantly larger than employment inflows and conclude from this that it is essentially job destruction who drives the business cycle properties of worker flows in the US labor market.

More recently, Kahn and McEntarfer (2013) use administrative data to compute labor market flows for US and conclude that the evolution of employment over the business cycle is essentially driven by the hiring decisions of firms, namely the high-quality ones. Also using administrative sources, Centeno and Novo (2013) reached a similar conclusion for Portugal: hirings have a larger contribution to the business cycle than separations, which is primarily explained by the behavior of the largest firms that reduce their workforce more strongly in recessions and lead hirings in upturns. We shall have more to say on this findings later in this paper.

As a whole, the findings on the pattern of American labor market flows are similar to those found in the European countries. However, we should bear in mind some issues regarding their comparability. First, some studies use data from household surveys, while others use data from administrative sources, and therefore they are not directly comparable. Second, they are based on data from different frequencies (in the US data is collected at a monthly basis, whereas in most of the studies for European countries data comes from quarterly surveys) which rises the so-called "multiple transitions" problem. A simple example illustrates this issue: if a worker moves from inactivity to employment via unemployment in a short period of time, it will probably be registered as an inactivity to employment transition in a quarterly survey, while a monthly survey would pick up the initial I→U followed by the U→E transition. For example, Blanchard and Portugal (2001) comparing the US to Portugal, conclude that on an annual basis the two economies have similar worker flows, but at a quarterly frequency Portugal has flows only of one-quarter of those in the US. However, the authors do not correct for time aggregation bias in the extrapolation of flows at the different frequencies for which the surveys are carried, which might generate serious biases and misleading conclusions.

To summarize, the studies outlined above provide evidence that flows from employment into unemployment exhibit a countercyclical pattern, whereas the reverse flows have an heterogeneous behavior between economies, being countercyclical in some and procyclical in others. In the flows between inactivity and employment a procyclical pattern is widely found, while in the flows between inactivity and unemployment the cited studies point for a countercyclical behavior in Europe and identify no clear pattern in US.

[1] Blanchard and Diamond (1990) consider Current Population Survey (CPS) data from 1968 to 1986, while Bleakley et al. (1999) use CPS data from 1976 to 1999.

3 The Portuguese Labor Force Survey

3.1 The LFS redesign

The LFS is a major source of information about the Portuguese labor market. In addition to providing quarterly estimates of the employment, unemployment and inactivity, economists and policymakers use data from the LFS to examine broad societal and cyclical changes in economic activity. It includes around 40 000 households quarterly that are selected to represent the population in the nation and in each region.[2] The probability sample of housing units is drawn using a multistage stratification procedure, where the metropolitan areas within each region are included and the remaining areas of a region are sampled on a probability basis built upon the population CENSUS, with the probability of selection being proportionate to the population of the area. Households are interviewed for six consecutive quarters, such that each quarter $1/6$ of the sample is rotated out and $5/6$ of the sample is retained, allowing us to observe the labor force status in the quarter $t-1$ and t for $5/6$ of the workers, and therefore compute labor market flows or transition rates. We had access to the micro data for the 1998-2012 period.

In the first quarter of 2011, the Portuguese Statistical Office (*Instituto Nacional de Estatística,* henceforth INE) revised the questionnaire and switched to computer-assisted telephone interviewing collection procedure (CATI). In this section we will describe the main changes.

3.1.1 General changes

In the revised LFS the first interview in each of the six consecutive interview quarters is conducted through a personal visit, in the subsequent quarters the interviews are conducted over the phone, this method is known by computer-assisted telephone interviewing (CATI). The respondents however have the option to answer all the six interviews through a personal interview (CAPI) as in the old procedure. The rotation pattern established prior to the redesign was maintained, as well as the multistage sample scheme.

3.1.2 Changes in labor market states definition

Hereafter we will focus only on the path of individuals with 15 years old or older, since those who are less than 15 are immediately classified as inactive in both surveys.

In the previous LFS, the distinction between employed and non-employed was done by asking the following mutually exclusive questions: “Last week did you have a paid work, either occasional or for just one hour?”, “Last week did you have a non-paid work for a relative or for self-supply?” and “Regardless of not having done a paid or non-paid work, do you have any job or business from which you have been absent last week?”. Those who answered affirmatively to one of the previous questions were classified as employed, while a negative answer implied to be classified as non-employed.

Among the non-employed, the distinction between unemployed and inactive was also straightforward. Respondents were first asked if they *actively* search for a job, even for a part time one or for self-employment. Those who said *yes* and were available to start working immediately, or at least in the next two weeks, were classified as unemployed. Those who said *no* were then asked if they expected to be called for a job in the next three months and were available to start working immediately or at least in the next two weeks (it sounds redundant, but that is how it was). If they answered affirmatively to both questions, they were classified as unemployed, too. Consequently, the non-employed individuals classified as inactive were those who were neither *actively* searching

[2] More precisely the “NUTS”, the regions for statistical purposes.

for a job nor expected to be called for a job in the next three months and/or were not available to start working immediately or at least in the next two weeks.

The classification algorithm in the old method was rather simple when compared to the one of the revised LFS. Let us begin by considering the following cases:

1. Worked last week and received compensation.

2. Worked last week but received no compensation.

3. Did not work last week and did not have any job or business from which (s)he has been absent last week.

4. Did not work last week but has a job or business from which (s)he has been absent last week.

In cases 1 and 3 respondents are directly classified as employed and non-employed, respectively. Whereas in case 2 respondents were then asked a set of questions in order to determine what kind of work they had. If the non-paid work was due to an internship or agriculture and fisheries on own account and for self-supply, but whose impact on the family budget is irrelevant, then individual is re-allocated to cases 3 or 4. In the remaining situations of case 2 the respondents are classified as employed.[3]

In case 4 the respondent's classification depends on the reasons for the absence. If maternity/paternity leave, vacations or health reasons are invoked, then the respondent is classified as employed. If the respondent has a new job that (s)he hasn't started yet, (s)he is classified as non-employed. If other reasons are reported, the respondent is then asked how long the absence is expected to last.[4] In that situation we have to consider the cases:

5. The absence is expected to last three or less than three months.

6. The absence is expected to last for more than three months or for an unknown period.

Individuals in case 5 are classified as employed, while for those in case 6 further refinement is done. If the respondents receive no compensation or less than an half of their wage, they are then classified as non-employed, whereas those whose absence is that of case 6 and are receiving an half or more of their wage are classified as employed.

Before proceeding, it is useful to point out how the probability to be classified as non-employed increased with the redesign, even for individuals in the same circumstances in the fourth quarter of 2010 and in the first quarter of 2011. For example, an individual in cases 2 or 4 would have been immediately classified as employed before the redesign, while in the revised survey the same individual has serious chances to be classified as non-employed.

Having disentangled the rules underlying the classification of individuals as employed or non-employed, now we attempt to distinguish those that are classified as unemployed or as inactive from the pool of non-employed. For this distinction, to declare availability to start working in a given period is crucial, thus respondents are asked: "If you had found a job last week, would you be available to start working last week or in the next fifteen days?". Those who say *no* are directly classified as inactive, whilst the classification of the others depends on specific job search questions.

[3] Such as working for a relative from whom the individual depends on and working in agriculture/fisheries with commercial purposes or for self-supply with impact on the family budget.

[4] Such as lay-off, seasonal work, leave without pay, strikes or other labor conflicts, academic and professional training and others.

Table 3.1: Job search diligences by type

Active job search diligences
• Contacts with private job agencies
• Contacts directly with employers
• Contacts with own social conections or unions
• Publishes, replies or searches for job announcements
• Has done job interviews or recruitment tests
• Searches for land plots, facilities or equipments
Passive job search diligences
• Sought licences or financial resources
• Is waiting the results of a job application
• Is waiting to be contacted by the Employment Office
• Is waiting the results of a public tender

Again it is useful to take into account the following scenarios:

7. Have been looking for work last week or three weeks before and:
   (a) Contacted an Employment Office (*Centro de Emprego*) for the purpose of:
      i. Enroll as unemployed for the first time, get informed about a specific job offer received from the Employment Office or other possible job offers.
      ii. Renew the enrollment, apply to professional trainning (and others).
   (b) Have done active job search diligences.
   (c) Have done passive job search diligences.
8. Have not been looking for work last week or three weeks before.

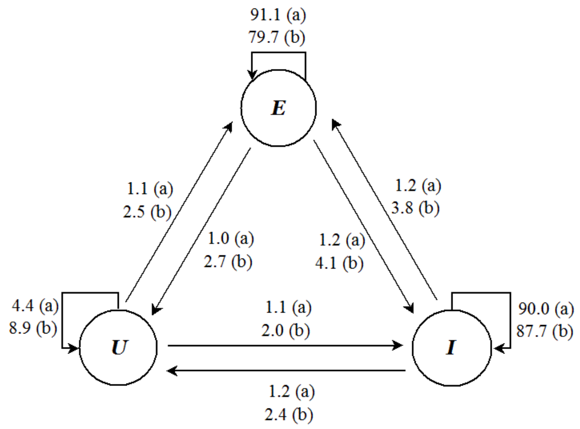
In the revised LFS, it is not enough to declare to be *actively* engaged in job search, in fact, for those who state to have been looking for work (case 7) are just considered unemployed those who have done, at least, the diligences of cases 7(a)i and 7b (Table 3.1 summarizes what it is meant by *active* and *passive* diligences). In turn, individuals in case 7 whose contact with Employment Office was due to the purposes of case 7(a)ii or whose job search diligences were those of case 7c, are then classified as inactive. In general, those of case 8 do not meet the criteria to be classified as unemployed, however, there is one exception: when the respondents have already found a job where they will start working in the next three months.

To summarize, the LFS redesign brought major definitional changes in the concepts of employment, unemployment and inactivity. In the revised LFS it is not sufficient to report a paid or non-paid work, or an absence from work to be classified as employed. Now, the specific nature of the non-paid work, the reasons for the absence and the individuals expectations of how long the absence will last, play a major role in their classification as employed or non-employed.

In a similar way, the distinction between unemployed and inactive is not merely based on the respondent personal assessment of his active job search. The concept of unemployed incorporates the specific job search diligences made by the respondent. If those were considered *active*, the respondent is classified as unemployed, otherwise (s)he joins the pool of inactive. It is not the case that those diligences were not asked before the redesign (they were, even with a rather similar wording), but now they are determinant to the respondent's classification.[5]

[5] In fact, before the redesign all the response categories correspond to the current active diligences (with the exception of "Sought licenses or financial resources"), the passive diligences were added to the revised LFS.

Figure 3.1: Average quarterly worker flows, 1999-2012



Source: Author's calculations based on Portuguese LFS.
Note: The worker flows are expressed as a percentage of the labor force.
The averages are computed over the samples: (a) 1999:1-2010:4 and (b) 2011:1-2012:4.

3.1.3 Other changes

There were other changes either in the wording or in the structure of the questionnaire that could be of interest. One of them has to do with the question where the employed respondents were asked about the type of contract they have. Before the redesign, there were five response categories: (1) open-ended (permanent) contract; (2) fixed-term (temporary) contract; (3) self-employed (*recibos verdes*); (4) seasonal work without formal labor contract; and (5) odd jobs (*biscates*). In the new survey the last two categories were dropped out, while the first three were kept unchanged.

Another change occurred in the question where the majority of unemployed provide their reasons for unemployment: the revised LFS no longer considers voluntary quits as a response category. In the literature on labor market flows, it is frequently subject of research (Anderson and Meyer, 1994; Bleakley et al., 1999; Gomes, 2012) the sensitivity of three classes during business cycle fluctuations: those who are voluntarily unemployed (voluntary quits) and those who are involuntarily unemployed (involuntary quits) and a broader category usually named "others" (that is a catch-all category for a variety of different reasons). However, the interest lies predominantly in the differential experience of the first two, which with the referred methodological change introduces serious limitations on data to perform this analysis for the Portuguese labor market.

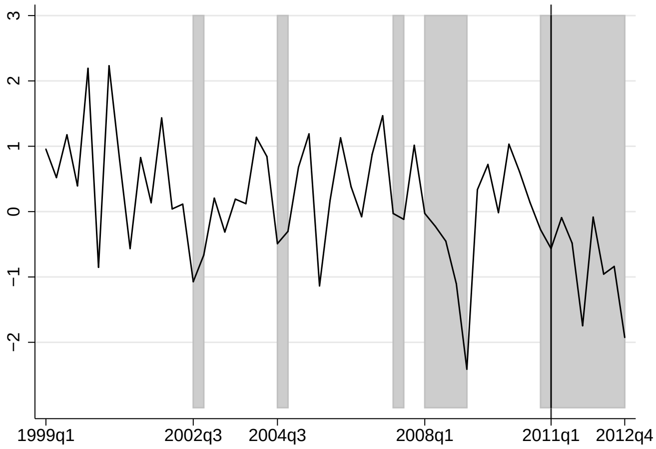
3.2 Consequences of the redesign

Poterba and Summers (1986) investigated the impacts of misclassification errors for the indicators derived from the CPS. They found that ambiguities in the survey questions, recording errors or simple mistakes on the part of the respondents cause the measured flows to dramatically overstate actual movements in the labor market and, to a lesser degree, lead to biases in the surveyed characteristics of the respondents.

Polivka and Miller (1998) assessed the impact of the CPS redesign for various aggregated measures derived from it. They found that the redesign had no significant effect on major statistics, such as the unemployment rate, but it greatly affected some disaggregated measures, as worker flows and the surveyed characteristics of the respondents.[6]

[6]The CPS redesign occurred in January 1994, the process had some similarities with the redesign of the Portuguese LFS, namely the adoption of CATI and the introduction of a time threshold for work absence in the classification as employed. The authors estimated adjustment factors using two parallel surveys: one prior to the

Figure 3.2: Quarterly GDP growth rate



Source: OECD.
Note: Growth rate compared to previous quarter, seasonally adjusted.
Shadings indicate recessions. The vertical line signals the revised survey.

We can face the LFS redesign as a kind of "deterministic misclassification", since there are deliberated changes in the labor market states' definition that might cause the same individual in the same circumstances to be classified differently. For example, let's consider the case of a respondent who reports to be absent from work or to be in a non-paid work (cases 2 and 4). Before the redesign, he would be directly classified as employed, while in the revised LFS he could be considered non-employed. In a very similar way, a non-employed individual who states to be *actively* searching for work could be classified as inactive if the reported job search diligences do not match the required ones to be classified as unemployed, whereas before the redesign the same individual would be considered unemployed. Thus transitions in and out of employment, unemployment and inactivity might show-up in the revised LFS, while before the redesign none would actually occur.

Figure 3.1 illustrates the average quarterly worker flows as a percentage of the labor force over two subsamples: (a) the period *before* the redesign and (b) the period *after* the redesign. One can observe that the labor market flows suffered a dramatic change after the redesign. In almost all cases more than doubles the averages taken before the redesign. However, as Figure 3.2 displays, a deep recession started almost at the same time as the LFS redesign. So, the question of whether these numbers are solely explained by methodological changes emerges. For example, it seems reasonable to ask what mostly explains the huge increase in the employment outflows: the redesign or their countercyclical behavior?

As outlined in sections 2.2 and 2.3, aside from business cycle fluctuations and seasonal movements (the *higher frequencies* in the words of Bleakley et al., 1999), the characteristics of the population (the *lower frequencies*) also influence the likelihood to obtain certain labor market outcomes. Therefore, several factors must be taken into account in our attempt to explain the changes in the worker flows since the redesign.

We will focus now on the *lower* frequencies and compare the surveyed population characteristics before and after the redesign, in particular between the fourth quarter of 2010 and the fourth quarter of 2012. For that we compute the means of the variables frequently used in the literature to explain individual's labor market mobility.

redesign using the new collection procedures and another after the redesign using the old methodology.

Figure 3.3 gives us a picture of the changes, but more rigorous information can be found in Table C.1.[7] Taking as reference the usual 5 percent level, Table C.1 reveals that the new methodology might have significantly decreased the proportion of individuals: (a) married or living as married, (b) in the age cohort of 25 to 34 years old and (c) living in Alentejo or Azores. However, the most noticeable change occurs in the educational levels: the proportion of individuals with none or little education (Educ 1 and Educ2) significantly decreased, 6 and 9.7 percentage points, respectively. On the other hand, the proportion of individuals with nine years of education (Educ3), an high school degree (Educ4) or a college degree (Educ5) significantly increased by 0.9 p.p., 1.8 p.p. and 3 p.p., respectively.

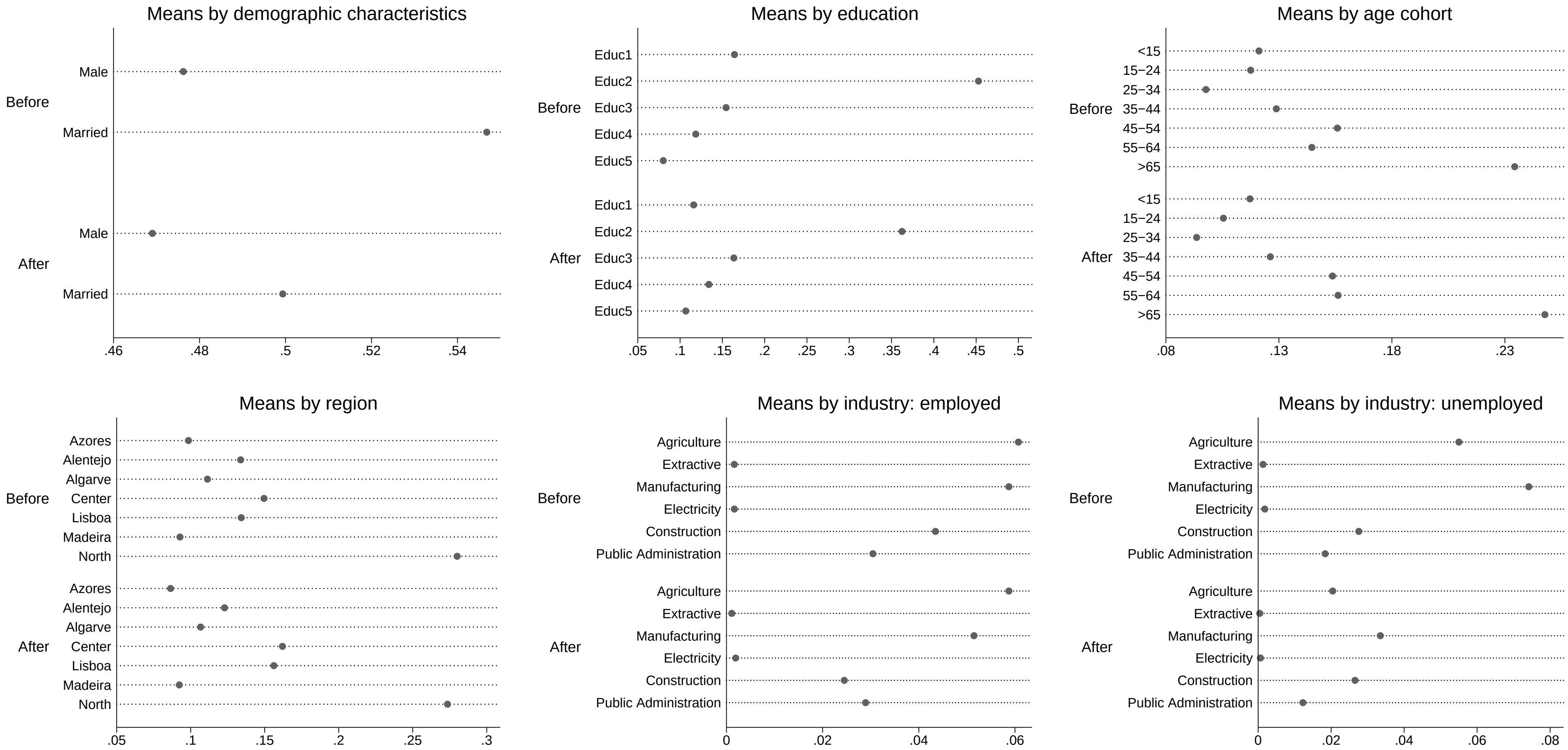
In addition to variations in the surveyed population characteristics, it might be also useful to investigate whether the respondents' occupations by industry significantly changed with the redesign. As the occupation is not a generic population characteristic, we have to distinguish the employed respondents from the non-employed ones, that's what is done in Figure 3.3 and again in Tables C.2 and C.3. The proportion of respondents working in Services significantly increased by 3.2 p.p., while it is observed a significant decline in the proportion of those employed in Agriculture, Manufacturing and Construction. If there was no redesign effect, one might attribute this decline to the business cycle effects (as the recession became more severe between the two sample periods) and thus expect a coherent raise in the proportion of non-employed previously working in Agriculture, Manufacturing or Construction. Still, the proportion of non-employed by previous occupation significantly declines in all industries, with the exceptions of Construction (whose decline is not statistically significant) and Services, which increase by 6.8 p.p. (Table C.3). These results suggest that the new method might have significantly lowered the respondents in Agriculture and Manufacturing and raised the respondents in Services, in particular for the non-employed ones.

In short, the average labor market flows over the sample of the new method are substantially higher than those computed under the previous survey. Three factors compete to explain this structural break: (1) the redesign itself, (2) business cycle fluctuations and (3) possible changes in the characteristics of the population. Significant differences are found in the latter, however, these might also be due to the fact that they are derived under different methodologies, which is in line with previous studies focused on the impacts of misclassification and survey redesign.

In the following sections, we will propose a strategy to test the redesign effect on the worker flows series, controlling for the business cycle. We will also try to gauge how changes in micro characteristics might affect individual transitions, controlling for possible redesign effects on the surveyed characteristics by interacting them with a variable that indicates the survey data source.

[7]The nature of survey data should be incorporated in estimation procedures whenever that is possible. The statistical package `Stata` through it's module `svyset`, is one of the most complete and widely used packages for survey data analysis. Nevertheless, a number of routine procedures are not available with `svyset`, among these are the usual dot graphs of proportions like those of Figure 3.3. This is why the values in the graphs of Figure 3.3 may differ from those reported in Tables C.1 through C.3 of Appendix C. See Appendix B for details on estimation of proportions with survey data.

Figure 3.3: Means of population characteristics



Source: Author's calculations based on LFS.
Note: Here *Before* indicates 2010q4 and *After* indicates 2012q4.
Means computed without sampling weights.
The "Services" were dropped for scaling purposes.

4 Worker Flows in the Portuguese Labor Market

4.1 Constructing worker flows

Numerous issues regarding the estimation of worker flows have prompted researchers to suggest a variety of ways to estimate them. Abowd and Zellner (1985) discuss at length the issues surrounding the estimation of worker flows on the basis of survey data and propose a set of ex-post adjustments to eliminate the sources of spurious transitions. A comprehensive discussion of survey's methodology and the effects of non-response bias on the flows' estimation can also be found in Clark and Tate (2000).

We will discuss here the issues regarding the estimation of the flows in the context of the Portuguese LFS, namely the methods adopted to match individuals in consecutive quarters, to adjust and disaggregate the flows series.

There are two bases for computing flows, either a change in status today relative to last quarter (backward matching) or change next quarter relative to today (forward matching). In the first case, quarters 2, 4 and 6 are matched with their counterparts looking backward (i.e., 1, 3 and 5), while in the second quarters 1, 3 and 5 are matched with their counterparts forward. It is worth to note that these forward or backward matching methods are not mere conventions. In fact, the resultant flows will be different according to the adopted method, because the sampling weights vary with the quarters. We use the backward matching approach in this study, and thus the flows are the population-weighted sums of all workers who change labor market status in quarter t relatively to quarter $t-1$.

In constructing worker flows, we track the individuals' labor market states over the six quarters they are in the panel, these are in turn determined by the algorithms presented in section 3.1.2. Before the redesign, there was no individual's unique identifier, so we match individuals based on a core group of variables.[8] The new method introduced a variable that identifies each individual, making the matching procedure simpler. Still, further adjustments are done. At each pair of adjacent quarters, we verify if a given transition, matched with the referred identifiers, is consistent with two observable characteristics: age and sex. The inconsistent observations are removed from the sample, which in practice makes the fraction of the matched respondents lower than the $5/6$ referred in section 3.

After constructing our matched sample, we cross-tabulate the labor force status in each quarter, which gives us a matrix describing the worker flows for that quarter. Repeating this process for each pair of adjacent quarters in the sample generates the series of aggregated worker flows. Nevertheless, there are two main obstacles to construct a continuous set of worker flows from 1998 to 2012: (i) between the fourth quarter of 1998 and the first of 1999 the households identifiers were scrambled by the INE; and (ii) between the fourth quarter of 2010 and the first of 2011 there was the mentioned change in the individuals' identifiers. As a consequence of this two issues, we were unable to follow individuals and estimate the correspondent flows. In the latter case, we adopted an imputation procedure in order to keep the seasonal pattern of the flows and the expected structural break due to the redesign, while in the former we trimmed the first four quarters of the sample.[9]

A host of difficult data constructions issues surrounds the use of the survey and we touch on a few key issues here. Still, we believe that our efforts reflect a good compromise given the available data.

[8] Accommodation ID (Item cua); location ID (Item seccao); household identifier inside the accommodation (Item num_familia); individual identifier inside the household (Item num_individuo)

[9] Based on a four quarter moving-average. See Heeringa et al. (2010) pp. 345-359 for a complete overview on imputation models for survey data.

Table 4.1: Seasonality of labor market flows since Spring 1999.

	E→I	I→E	E→U	U→E	I→U	U→I
Winter	1.693	1.630	1.290	1.329	1.296	1.155
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Spring	1.523	1.581	1.062	1.396	1.190	1.202
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Summer	1.594	1.568	1.185	1.275	1.424	1.207
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Autumn	1.798	1.539	1.413	1.337	1.423	1.301
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	55	55	55	55	55	55
R^2	0.699	0.702	0.795	0.869	0.889	0.907
F-Statistic	29.649	30.064	49.531	84.685	101.726	124.562

Source: Author's calculations based on LFS.
Note: p-values in parentheses.

4.2 Aggregated worker flows

The aim of this section is to analyze the basic facts of the Portuguese labor market flows from 1999 to 2012. We will look at some labor market descriptive findings and then the flows discontinuity and their cyclical properties will be assessed within a regression discontinuity framework.

Figure 4.1 shows the labor market stocks, flows and hazard rates; the shadings indicate recessions while the vertical line signals the beginning of the new survey. It becomes clear how the redesign affected differently labor market stocks and flows: in all the flows series one observes a dramatic shift in the flows derived under the revised LFS, while, for instance, there is not such a break in the sharply upward trend of the unemployment rate. "Eyeballing" Figure 4.1 one can also observe the following features: (a) There is not a clear trend in both employment and inactivity outflows, contrarily to unemployment outflows, which exhibit an upward trend; (b) The flows series contain periodic spikes related to seasonal movements, as well as fluctuations related to the economic cycle, with employment separations (and somewhat inactivity outflows) peaking during recessions; (c) The picture of worker flows and hazard rates is very similar for employment and inactivity outflows. This is not the case of the unemployment outflows hazard rate, where one can see a sharply downward trend. Although, there is a quite logical reason for this: as the pool of unemployed enlarges due to much less hires (Centeno and Novo, 2013) the ratio between the unemployment outflows and the stock of unemployed shrinks, decreasing the probability to move out of unemployment.

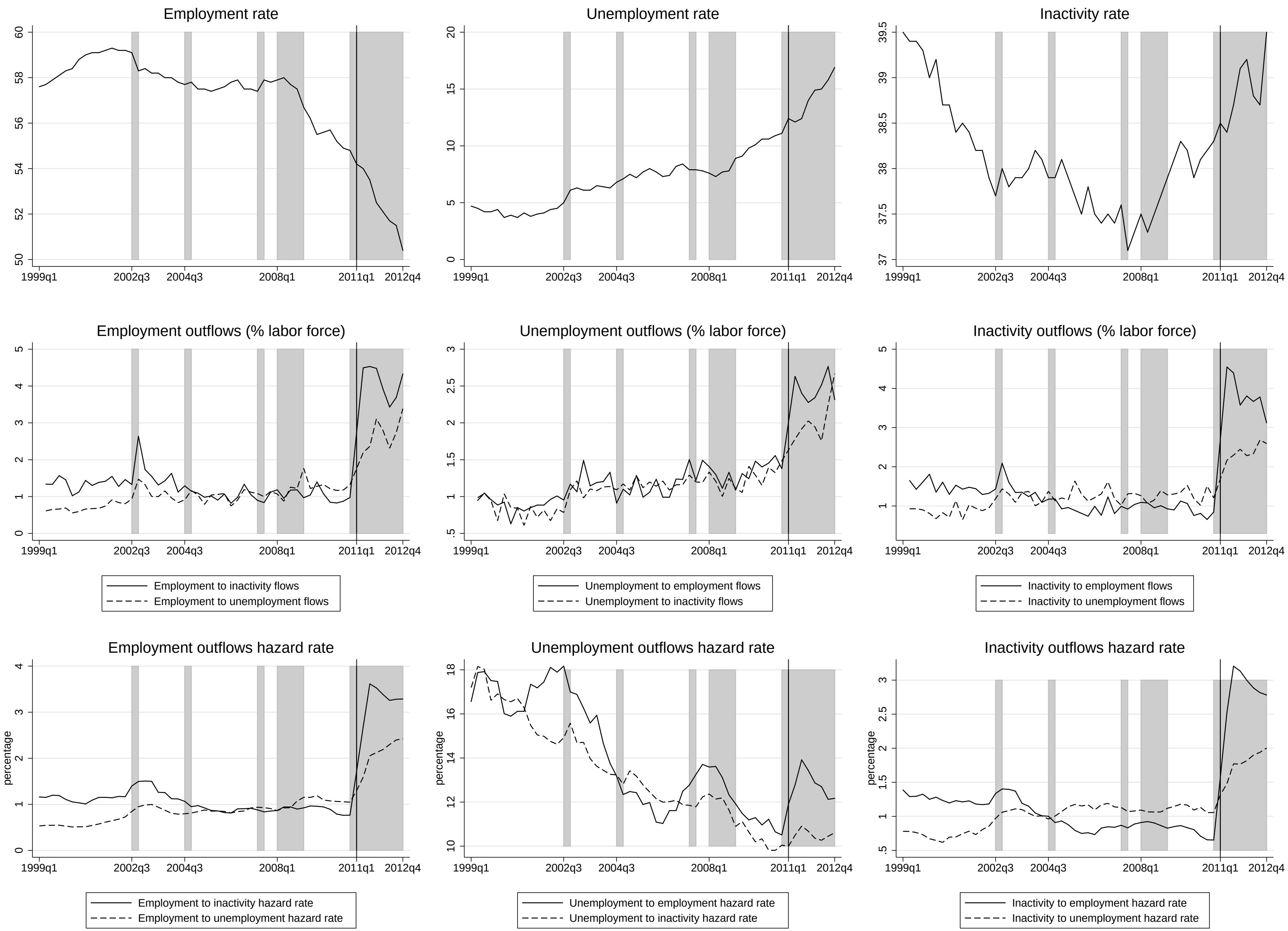
4.2.1 Seasonality

Examination of Figure 4.1 made us suspect that worker flows are strongly influenced by seasonal factors. In order to confirm this, we regress each of the flows series on a set of seasonal dummies.

Table 4.1 shows that all coefficients are statistically significant at any usual level. As shown by the value of the R^2, seasonality is most important in U↔I flows, followed by E↔U and E↔I. This motivated us to seasonally adjust the flow series using Census Bureau X-12 (CB X-12). We used this procedure because it is a standard and readily available package for seasonal adjustment and also because previous studies, such as those of Blanchard and Diamond (1990) and Bell and Smith (2002) chose its predecessor CB X-11 for seasonally adjust worker flow data.[10]

[10]There are no reasons to believe that the redesign change the seasonal pattern of the flows. Still, in order to investigate this, we repeated this exercise using only the first subsample and the results didn't change.

Figure 4.1: Labor market stocks, gross flows and hazard rates



Source: Author's calculations based on LFS. The stock series are the INE official rates.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

Table 4.2: Average conditional transition probabilities

	Unconditional probabilities		Conditional on: E_{t-2}		U_{t-2}		I_{t-2}	
	Before	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*
$E \to U$	0.843	2.251	0.697	1.775	6.817	16.641	3.090	2.641
$E \to I$	1.028	3.425	0.855	2.139	2.613	4.452	8.165	30.846
$U \to E$	14.056	12.893	22.552	22.839	11.112	9.488	12.200	9.210
$U \to I$	13.316	10.648	9.103	6.428	10.240	8.360	25.466	23.095
$I \to E$	1.007	2.983	11.792	23.813	8.198	6.414	0.696	1.724
$I \to U$	0.997	1.868	7.139	5.753	23.493	24.000	0.573	1.108

Source: Author's calculations based on LFS.

4.2.2 State dependence

State dependence in labor market transitions has been a subject of research for a long time. For example, Ruhm (1991) found that displaced workers suffered persistent effects ("scars") that made them face higher unemployment rates for at least four years. More recently, Shimer (2012) discussed the effect of state dependence in the job-finding rate.

The presence of state dependence in worker flow data would reflect on different conditional transition probabilities, so we test its presence in our data by computing the average conditional probabilities based on three period flows:

$$\lambda^{XY}_{t|E_{t-2}} = \frac{N^{XY}_t}{X_{t-1}}|E_{t-2},\ \lambda^{XY}_{t|U_{t-2}} = \frac{N^{XY}_t}{X_{t-1}}|U_{t-2},\ \lambda^{XY}_{t|I_{t-2}} = \frac{N^{XY}_t}{X_{t-1}}|I_{t-2}.$$

Table 4.2 shows substantial differences in the conditional probabilities. The probability of separation from employment is considerably high for those who were non-employed two quarters earlier. In case of a E→U transition, those who had been unemployed in $t-2$ face a higher probability to return to unemployment. In the same way, those who make a E→I transition and experienced inactivity in $t-2$ have higher probabilities to return to inactivity.

There is also evidence that the job-finding rate is strongly influenced by the individuals' path. Those who had been unemployed last quarter and passed through employment two quarters earlier, have about two-times more chances to find a job than those who had been unemployed or inactive in $t-2$. In the case of I→E transitions, those who were already inactive two quarters ago have a job-finding rate roughly sixteen times lower (thirteen, after the redesign) than those who were employed in $t-2$. In the same way, a I→U transition is much more likely for individuals who have been in the labor force two quarters earlier, particularly for those who had been unemployed. By contrast, transitions from unemployment into inactivity are much more likely to occur among individuals who experienced inactivity in $t-2$.

All in all, state dependence greatly accounts for the probability to find a job or return to the labor force. The redesign did not change the overall pattern, however, it appears to have increased the returning probabilities of those who came from inactivity, since these more than doubled under the revised survey. The probabilities to move from I to E also more than doubled for individuals who were employed in $t-2$. On the other hand, there are almost no differences in the probabilities of those transitioning from I to U who where in U two quarters earlier.

Gomes (2012) made use of the British LFS to compute this probabilities for the 1996-2010 period. When comparing our results to those obtained for the UK, it appears that passing through unemployment or inactivity leaves more "scars" in the probability to find a job of the Portuguese workers. Indeed, even considering the differences caused by the redesign, the job-finding rate is substantially higher for those who had been previously unemployed or inactive in the UK: 20.0 percent vs. 11.112 percent (9.488 percent, after the redesign) for those who had been unemployed in $t-2$, and 24.1 percent vs. 12.200 percent (9.210 percent) for those who had been inactive in $t-2$.

These results have an important consequence for policy: they stress the need to activate non-employed individuals (for example, through professional training that prevent human capital depreciation or through incentives that encourage firms to hire) to mitigate the negative self-reinforcing effects of passing through non-employment, particularly in the context of Portugal's weak labor market.

We acknowledge that this approach does not control for micro characteristics of the individuals, however, notably studies of job loss provide individual-level evidence of this scarring thesis (Heckman, 1981; Hall, 1995).

4.2.3 Regression discontinuity approach

Regression discontinuity (RD) methods (Imbens and Lemieux, 2008; Angrist and Pischke, 2009) have been used in causal and treatment effects literature to model jumps (discontinuities) in the observed outcomes due to the assignment to some treatment. The particularity of RD methods is the assignment mechanism be a deterministic and discontinuous function of an observable covariate x_i (the forcing or treatment-determining variable) such that there is no value of x_i at which treatment and control observations overlap. Before proceeding, we shall formalize the RD model.

Let $W_i \in \{0, 1\}$ denote the assignment mechanism to a particular treatment such that

$$W_i = \begin{cases} 1 & \text{if } x_i \geq c \\ 0 & \text{if } x_i < c \end{cases} \tag{4.1}$$

where c is a known threshold, thus if we observe x_i we then know W_i (deterministic). Furthermore, there is no value of x_i at which we observe both treatment and control observations (discontinuous). Let also $Y_i(0)$ and $Y_i(1)$ denote the pair of potential outcomes. Thus, the observed outcome Y_i can be written as

$$Y_i = (1 - W_i) \cdot Y_i(0) + W_i \cdot Y_i(1) = \begin{cases} Y_i(0) & \text{if } W_i = 0, \\ Y_i(1) & \text{if } W_i = 1. \end{cases} \tag{4.2}$$

Assuming that $Y_i(0)$ and $Y_i(1)$ can be described by a linear model and letting ρ be the causal effect of interest, we get $\mathbb{E}[Y_i(0) \mid x_i] = \alpha + \beta x_i$ and $Y_i(1) = Y_i(0) + \rho$. So, if the forcing variable is below the threshold ($x_i < c$) then $W_i = 0$ and we get $\mathbb{E}[Y_i(0) \mid x_i] = \alpha + \beta x_i$. Conversely, if $x_i \geq c$ then $W_i = 1$ and we get $\mathbb{E}[Y_i(1) \mid x_i] = (\alpha + \rho) + \beta x_i$. Considering the assignment mechanism and making use of eq. (4.2) leads to the regression

$$Y_i = \alpha + \beta x_i + \rho W_i + \epsilon_i. \tag{4.3}$$

However, the trend relationship between Y_i and x_i might be linear, as in eq. (4.3) or non-linear. Let us suppose a flexible functional form for $\mathbb{E}[Y_i(0) \mid x_i]$ such that $\mathbb{E}[Y_i(0) \mid x_i] = \alpha + f(x_i)$, where $f(x_i)$ is some smooth function of a p^{th}-order polynomial. Then RD estimates can be constructed by fitting for some p

$$Y_i = \alpha + \underbrace{\beta_1 x_i + \beta_2 x_i^2 + \cdots + \beta_p x_i^p}_{f(x_i)=\sum_{j=1}^{p} \beta_j x^j} + \rho W_i + \epsilon_i. \tag{4.4}$$

Still, it might be inappropriate to impose the same $f(x_i)$ for both $\mathbb{E}[Y_i(0) \mid x_i]$ and $\mathbb{E}[Y_i(1) \mid x_i]$ as eq. (4.4) does. By modeling both expectations in order to allow distinct polynomial trend functions of the same p^{th}-order, we get

$$\mathbb{E}[Y_i(0) \mid x_i] = \alpha + \sum_{j=1}^{p} \beta_{0j}\tilde{x}_i^j, \tag{4.5}$$

$$\mathbb{E}[Y_i(1) \mid x_i] = (\alpha + \rho) + \sum_{j=1}^{p} \beta_{1j}\tilde{x}_i^j. \tag{4.6}$$

Again, making use of eq. (4.2), this leads to the regression

$$Y_i = \alpha + \sum_{j=1}^{p} \beta_{0j}\tilde{x}_i^j + \rho W_i + \sum_{j=1}^{p} \beta_j^*(W_i \times \tilde{x}_i^j) + \epsilon_i, \tag{4.7}$$

where $\beta_j^* = \beta_{1j} - \beta_{0j}$ with $j = 1, ..., p$ and ϵ_i is a conventional error term. Centering x_i at c such that $\tilde{x}_i \equiv x_i - c$ is just a normalization that ensures ρ in eq. (4.7) has the same interpretation as in equations (4.3) and (4.4). The interaction terms on W_i allow for different trend functions, therefore, if we set $\beta_j^* = 0, \forall j$ we get eq. (4.4).

In addition to the forcing variable, there might be other covariates that explain the jump in the observed outcomes. According to Imbens and Lemieux (2008), the presence of these covariates in the specification can eliminate sample biases and improve the precision, particularly if they are expected to be correlated with Y_i. Let the vector of these covariates be denoted by $\boldsymbol{z}_i$. Including $\boldsymbol{z}_i$ in eq. (4.7) leads to the more general model

$$Y_i = \alpha + \sum_{j=1}^{p} \beta_{0j}\tilde{x}_i^j + \rho W_i + \sum_{j=1}^{p} \beta_j^*(W_i \times \tilde{x}_i^j) + \delta'\boldsymbol{z}_i + \epsilon_i, \tag{4.8}$$

where the parameters are defined analogously to those in eq. (4.7).

In this study we make use of the RD idea to model the discontinuity in the labor market flows. Analogously, there is an assignment to a treatment (the interviews under the revised survey) whose mechanism is a deterministic function of the covariate x_i (the time) at a given known threshold c (the first quarter of 2011) resulting in the pair of outcomes $Y_i(0)$ and $Y_i(1)$ (the flows before and after the redesign). Additionally, there is no value of x_i at which we observe both treatment and control observations.

4.2.4 Results

We estimate equations (4.7) and (4.8) for each seasonally adjusted labor force flow Y_i. Where: $\tilde{x}_i$ is the time centered at the first quarter of 2011; W_i is a Design dummy that assumes value 1 since the first quarter of 2011 and 0 in the prior quarters; $\boldsymbol{z}_i$ is a scalar variable that contains the GDP cyclical component, obtained using a Hodrick-Prescott filter with standard smoothing parameter of 1600 on the previously logged and seasonally adjusted GDP series; and ϵ_i is a conventional error term. Both equations were estimated by least squares.

Examination of the data suggests a non-linear relationship between and Y_i and x_i, so we modeled $f(x_i)$ through a linear and a polynomial trend function of order 2. High polynomial orders were fitted, but the main conclusions remained unchanged and it was felt that using two specifications for $f(x_i)$ would help our analysis seem more objective, as we would not have to be engaged in fitting different models for each polynomial order.

The coefficient of the "Design dummy", $\hat{\rho}$, will give us the estimated effect of the redesign in each of the flows series. So, we might infer the presence of a design effect if $\hat{\rho}$ turns out to be statistically significant.

Table 4.3: RD estimates: Employment, Unemployment and Inactivity outflows

	E outflows		U outflows		I outflows	
	E→U	E→I	U→E	U→I	I→E	I→U
Design Effects						
Design dummy	0.901	3.867	1.175	0.539	3.654	0.996
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Business Cycle Effects						
GDP cyclical component	-5.342	-1.894	-0.227	-3.177	0.111	-5.349
	(0.007)	(0.550)	(0.892)	(0.046)	(0.965)	(0.001)
Observations	55	55	55	55	55	55
Adjust. R^2	0.941	0.945	0.931	0.896	0.961	0.929
F-Statistic	144.308	154.856	122.625	78.541	223.373	117.906
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. This table summarizes the estimates of Tables C.7 through C.9 of Appendix C. The forcing variable and its polynomials are omitted from the table.

As discussed, the literature on worker flows defines the cyclicality of flows as their correlation with the level of economic activity. Thus, when a worker flow goes up in a downturn (or down in an upturn) it is said to be countercyclical and one might expect the estimated coefficient of the cyclical component to come negative, $\hat{\delta} < 0$. Conversely, when a flow goes down in a downturn (or up in an upturn) it is said to be procyclical and one might expect $\hat{\delta} > 0$.

The estimates of eq. (4.7) for both polynomial orders indicate significant design effects in all the flows series (Tables C.4 through C.6 of Appendix C). Controlling for business cycle effects by estimating eq. (4.8), the "Design dummy" also comes strongly significant in all the flows (Tables C.7 through C.9 of Appendix C). Moreover, the E→U and U↔I flows reveal a significant countercyclical pattern. Regardless of minor changes in the point estimates, the results do not depend on the choice of the polynomial order, so we will focus on the estimates of eq. (4.8) for $p = 2$ (Table 4.3).

From 1999 to 2010, an average of 1.139 percent of the labor force moved from U into E every quarter, this values changed to an average of 2.464 percent in the 2011-2012 period (Figure 3.1, section 3). This drastic change is solely explained by the redesign, which has an estimated effect of 1.175 p.p. increase in the U→E flows.

In a similar way, before the redesign an average of 1.178 percent of the labor force moved from I to E every quarter, while an average of 1.222 percent did the reverse movement. After the redesign, this averages changed to 3.841 and 4.124 percent, respectively. Again, only design effects are found significant, having an impact of 3.867 p.p. increase in E→I and 3.654 p.p. in the reverse flow.

Both design and cyclical effects are statistically significant in E→U flows. But which effect primarily explains the high values of employment separations since 2011: design or business cycle? The first quarter of 2009 was the fifth recessionary quarter since 2008, with a GDP growth rate of -2.411 percent. Then, 1.768 percent of the labor force moved from E to U. In turn, the fourth quarter of 2012 had GDP growth of -1.925 percent, however, the E→U flows were 3.385 percent of the labor force, almost the double than in 2009.

Ceteris paribus, in the first quarter of 2009 the business cycle had an impact of 0.118 p.p. increase in E→U flows, while this effect was of 0.157 p.p. in fourth quarter of 2012. Taking the difference of the cyclical effects, one observes that in 2012 the business cycle effect was only 0.039 p.p. higher relatively to 2009. Repeating this exercise for all quarters since 2011 this difference comes marginally negative, revealing that the business cycle had an even lower effect in the E→U flows of all quarters since the redesign, relatively to the first of 2009 (except for the fourth of 2012).

Hence, the business cycle phase explains a little part of the jump in E→U flows and it is essentially the redesign that lies behind their discontinuity. In fact, if the countercyclicality of E→U flows had been the main reason for their structural break, one should expect to observe a similar jump in the aftermath of the 2008 economic and financial crisis.

Research by Centeno and Novo (2013) gives some strength to this point. The authors estimate labor market flows using social security data and find no structural break in employment outflows. Furthermore, they find that during recessions the reaction of the Portuguese labor market is mostly driven by the reduction of hirings, which confirms the view that the jump in employment outflows is mainly due to the redesign. It is worth noting that labor market flows computed through household surveys or through administrative sources are not directly comparable. Nevertheless, there is no reason to believe that the data source affects the cyclical properties of the labor market flows.

Regarding the flows between U and I, the redesign had an estimated effect of 0.996 p.p. increase in the I→U and 0.539 p.p. in the reverse flow. In addition, the cyclical coefficient comes strongly significant, confirming that recessions are periods in which flows between unemployment and inactivity significantly increase, which in line to the findings for other countries (section 2.3). By doing an analogous exercise to the one made for E→U, we reach similar conclusions: the redesign explains much of the jump of U↔I flows, despite their significant countercyclical behavior.

4.3 Disaggregated worker flows

In this section, we estimate labor market flows disaggregated by education and age levels of the survey respondents. The impetus is twofold. First, we want to uncover if there are differential design and cyclical effects within the categories of the same flow. Second, little is known about the flows disaggregated by micro-characteristics for the Portuguese labor market.

The procedure for constructing the disaggregated flows was rather similar to the one discussed in section 4.1, but in addition to the matching procedure we have to add breakdowns for age and education. The matched sample is then cross-tabulated according to the chosen breakdown and gross flows are computed for the consecutive quarters. Figure 4.2 and Figure 4.4 display the estimated flows, while Figure 4.3 and Figure 4.5 display the respective hazard rates. The levels of education are defined as: less than 4 years of schooling (Educ1); between 4 and 8 years of schooling (Educ2); between 9 and 11 years of schooling (Educ3); exactly 12 years of schooling (Educ4); and more than 12 years of schooling (Educ5). The ages are grouped into five cohorts representing the working-age population, for example "15-24" indicates the respondents who are between 15 and 24 years old.

After constructing the disaggregated flows, we estimated eq. (4.8) with $p = 2$ for each of the six flows adjusted with CB X-12 and split by each of the five categories of both age and education levels. The right-hand side variables of eq. (4.8) are defined as in the aggregated case.

4.3.1 Flows by education

The results suggest the presence of strong design effects in E↔I flows by all levels of education (Tables C.10 and C.12). However, the design effects seem to be stronger in individuals with no or little education, which can be seen by comparing the average quarterly flows (Table 4.4) to the magnitude of the "Design dummy". For example, in the case of the E→I flows, the redesign had an effect of 0.404 p.p. increase for those with Educ1 (with the average quarterly flows changing from 0.184 to 0.721 percent of the labor force) and 1.166 p.p. for Educ2 (with averages changing from 0.635 to 2.216 percent of the labor force). The second column charts of Figure 4.2 are very illustrative of how the E↔I flows by Educ1 and Educ2 increase more steeply than the remain flows.

Table 4.4: Average gross flows by education (% labor force)

	Education 1		Education 2		Education 3		Education 4		Education 5	
	Before	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*
$E \to U$	0.036	0.072	0.471	0.940	0.226	0.715	0.161	0.567	0.104	0.406
$E \to I$	0.184	0.721	0.635	2.216	0.189	0.527	0.140	0.387	0.073	0.273
$U \to E$	0.036	0.061	0.458	0.734	0.263	0.590	0.203	0.628	0.178	0.450
$U \to I$	0.058	0.093	0.571	0.756	0.230	0.563	0.149	0.429	0.074	0.207
$I \to U$	0.050	0.084	0.512	0.785	0.258	0.670	0.207	0.550	0.131	0.312
$I \to E$	0.116	0.650	0.501	2.091	0.256	0.478	0.195	0.358	0.110	0.264

Source: Author's calculations based on LFS.

Table C.12 shows significant design effects in the U→E flows across education levels. It is worth highlighting the low strength of the redesign's effect in individuals with no education. The redesign had an effect of 0.050 p.p. increase in flows from U to E of individuals with Educ1, while the average quarterly flows changed from 0.036 to 0.061 percent of the labor force. The third chart in the first row of Figure 4.2 is quite enlightening on this low sensitivity to the redesign of U→E flows by Educ1.

According to the results of Table C.10, the E→U flows of those with Educ1 were not affected by the redesign, while significant design effects were found in the flows for the remain levels of education (Educ2 at the 10 percent level). The cyclical coefficient of E→U flows appears strongly significant for Educ2 (and Educ3 at the 10 percent level), which suggests that the countercyclical behavior of E→U flows is mainly driven by separations of individuals with less education. Figure 4.2 depicts this: individuals with less education seem to be more prone to cyclical fluctuations than individuals with higher levels of education, with the E→U flows of individuals with less education peaking during recessions. A similar result was obtained by Gomes (2012), who disaggregated the transition probabilities by education levels for the UK and observed that individuals with higher education face less cyclical fluctuations than individuals with lower education.

The redesign seems to have significantly affected the U→I flows of those with Educ3 and Educ4 (Table C.11). For example, the average quarterly flows of those with Educ2 changed from 0.571 percent to 0.756 percent of the labor force, whilst for those with Educ4 these averages changed from 0.149 percent to 0.429 percent, with the redesign having a significant effect of 0.128 p.p. increase in the U→I flows by Educ4. In the reverse movement, we find significant design effects in the flows of those with Educ2, Educ3 and Educ5 (Table C.12). Furthermore, the flows from I to U of those with Educ5 seem to not respond to business cycle fluctuations, contrary to the remain categories where cyclical coefficient is found strongly significant.

Analogously to the aggregated level, if we compare the business cycle effects in the first quarter of 2009 with each quarter since 2011 for the flows where the "Design dummy" and cyclical coefficient are found simultaneously significant, we found no differences in the response of the series to the business cycle that could justify its structural break. Hence, the redesign seems to explain a large part of the jump of the disaggregated flows, too. Indeed, if nothing substantially different in the reaction of the series to recessions *before* and *after* the redesign is found at the aggregated level, it seems unreasonable that this differences could emerge at the disaggregated level.

Table 4.5: Average gross flows by age cohort (% labor force)

	15-24 years		25-34 years		35-44 years		45-54 years		55-64 years	
	Before	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*
$E \to U$	0.234	0.348	0.349	0.905	0.220	0.640	0.137	0.569	0.058	0.230
$E \to I$	0.232	0.304	0.193	0.304	0.146	0.335	0.173	0.534	0.263	1.044
$U \to E$	0.355	0.438	0.402	0.859	0.234	0.532	0.114	0.446	0.032	0.180
$U \to I$	0.236	0.492	0.276	0.417	0.196	0.369	0.193	0.397	0.173	0.342
$I \to U$	0.389	0.807	0.286	0.462	0.193	0.370	0.163	0.401	0.124	0.316
$I \to E$	0.454	0.402	0.227	0.249	0.147	0.264	0.128	0.447	0.118	0.835

Source: Author's calculations based on LFS.

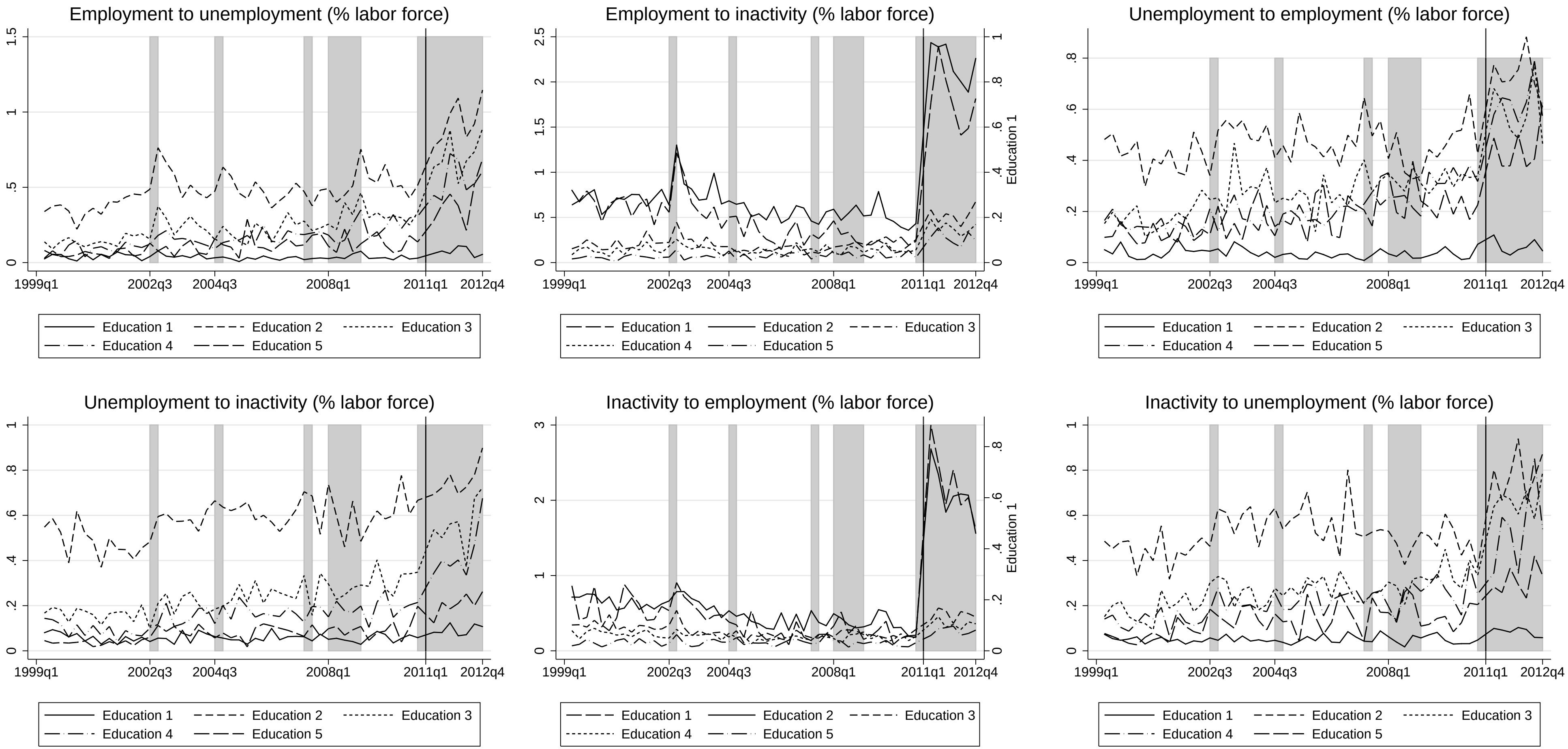
4.3.2 Flows by age cohort

The results suggest that the new methodology did not affect the I→E flows of those who are "15-24" and "25-34" years old (Tables C.13 and C.15). In turn, the redesign seems to have significantly affected the I→E flows of the older cohorts and, more interesting, the strength of the design effect increases with the age of the respondents. This is easily seen by comparing the average quarterly flows before the redesign (Table 4.5) to the magnitude of the design coefficient for each age cohorts: "35-44" (0.147 percent vs. 0.122 p.p. increase due to the redesign); "45-54" (0.128 percent vs. 0.281 p.p.); and "55-64" (0.118 percent vs. 0.433 p.p.). In the case of E→I flows the design coefficient is strongly significant for all age cohorts, but again, the older cohorts seem to be particularly affected by the methodological change. For example, before the redesign, the average quarterly flows were 0.232 percent for the age cohort of "15-24" and 0.263 percent for "55-64". In the former case, the redesign had an effect of 0.235 p.p., while in the latter the effect was of 0.563 p.p., with the average flows after the redesign becoming 0.304 percent and 1.044 percent, respectively. Figure 4.4 (third column) is quite enlightening of what has been said: the jump in the E↔I flows of the "45-54" and "55-64" clearly stands out among the remain flows. All in all, it seems that the flows between E and I of the older and less educated were particularly affected by the redesign.

A similar pattern emerges in the case of E→U flows. As Table C.13 shows, the "Design dummy" appears strongly significant for the older cohorts (with the average flows becoming around four times higher *after* for both "45-54" and "55-64"). For the "35-44" cohort the design effect is significant only at the 10 percent level, while for the cohorts below the 35 years old no redesign effects are found significant. The cyclical coefficient is found significant for the age cohorts of "35-44" and "25-34", which indicates that during recessions the increase in E→U flows is mostly due to employment separations of the youngest cohorts. Regarding the flows from U to E, the "Design dummy" comes strongly significant for all age cohorts, except for the "15-24" cohort (Table C.14).

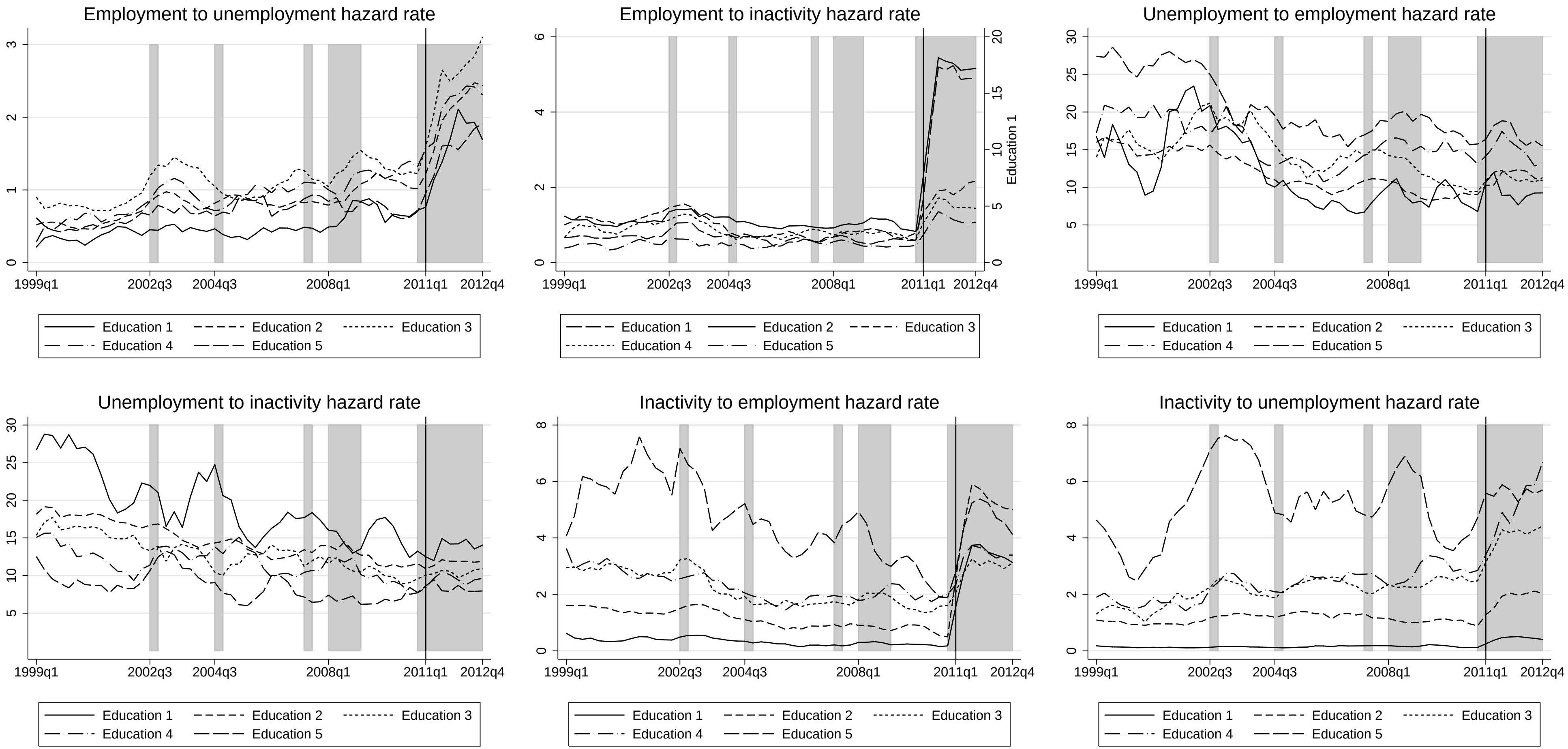
The redesign significantly affected the U→I flows of those who are "15-24" and "35-44" (Tables C.14 and C.15). In the former case, its effect was 0.110 p.p. (with the average flows more than doubling *after*: 0.236 vs. 0.492 percent), while in the latter the effect was 0.097 p.p. (0.196 vs. 0.369 percent). Furthermore, a significant countercyclical behavior is found for the same cohorts, which suggests that during recessions the U→I flows might go up mostly due to the movements of those who are "15-24" and "35-44". For the reverse flow, one finds evidence of redesign effects in all age cohorts except for the "35-44". The countercyclicality of flows from I to U appears to be driven by movements of those who are between 15 and 44 years old, the cohorts for which $\hat{\delta}$ is significant.

Figure 4.2: Labor market flows by education



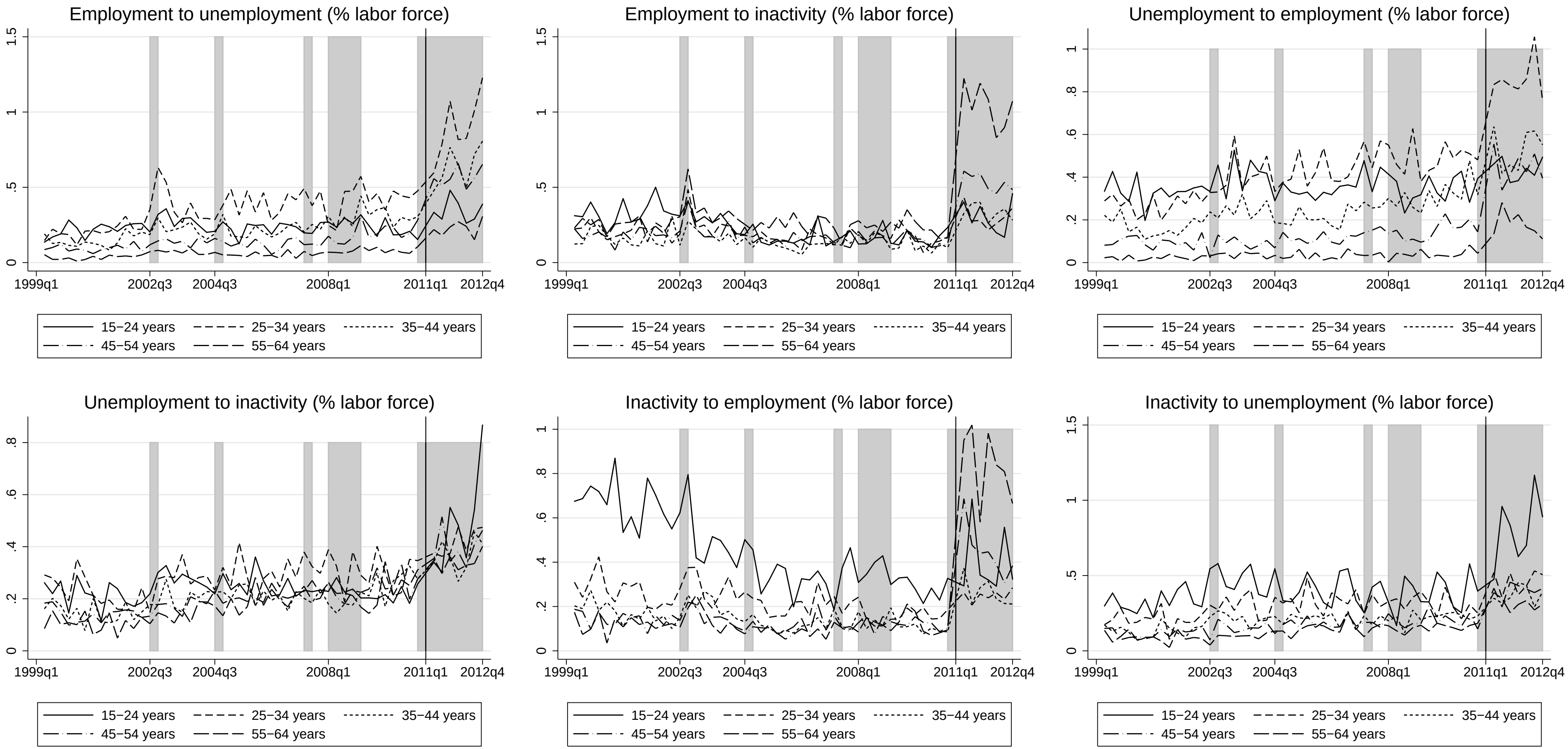
Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

Figure 4.3: Hazard rates by education



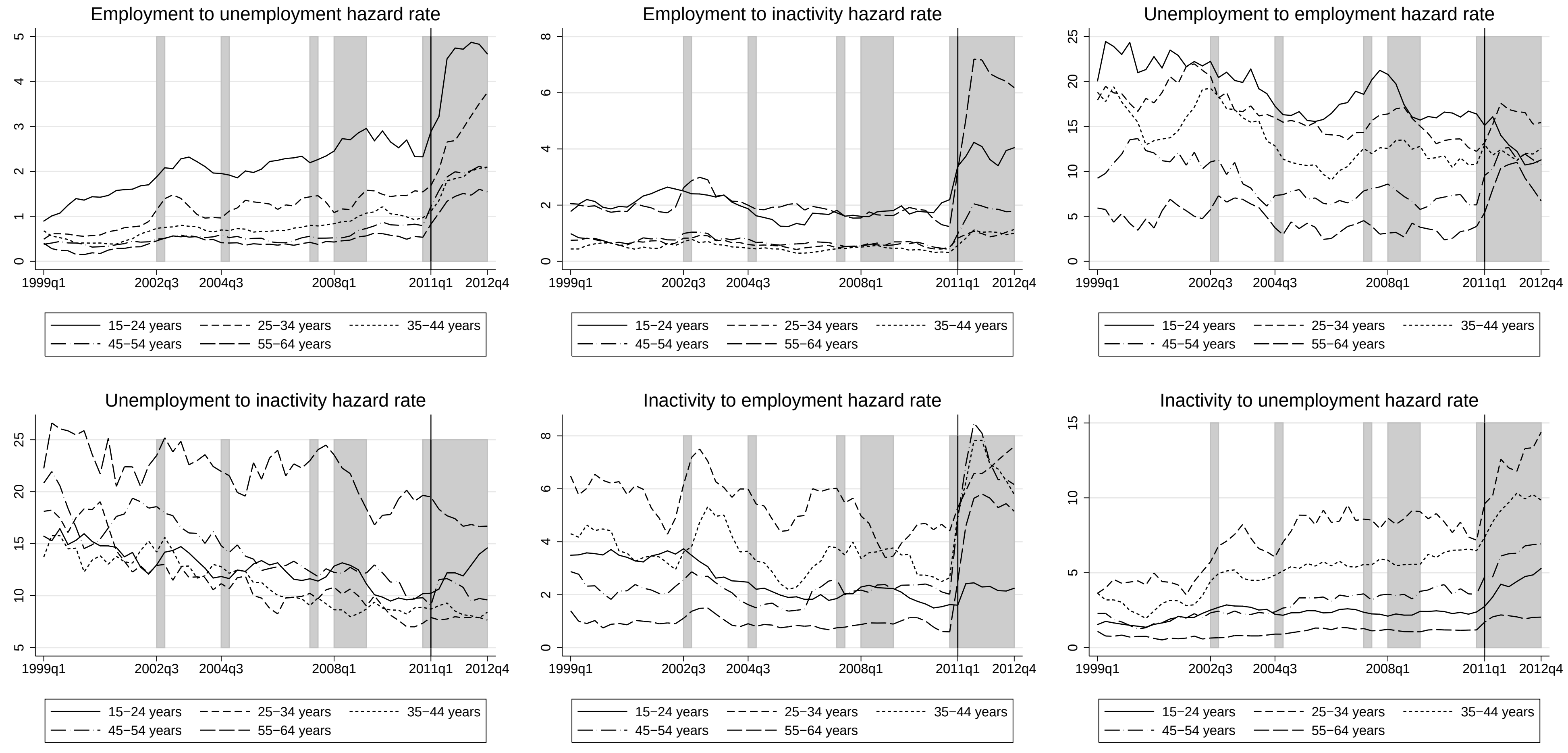
Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

Figure 4.4: Labor market flows by age cohort



Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

Figure 4.5: Hazard rates by age cohort



Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

4.4 A four-state model of labor market flows

In the previous sections, we disaggregate labor market flows by the micro-characteristics of those who make the transitions in the context of the usual three labor market states. Now, we attempt to study the dynamics of the Portuguese labor market and the impacts of the LFS redesign in a four-state set-up, firstly by disaggregating employment into permanent and temporary, and then by disaggregating inactivity between individuals who *want a job* and those who *do not want a job*. As we shall see, the analysis under this four-state model is particularly suited to the contractual structure of the Portuguese labor market and to understand the differential behavior of transitions between the pool of inactivity.

4.4.1 Disaggregating employment

A two-tier labor market is characterized by an employment protection gap between two types of contracts: fixed-term (or temporary) and open-ended (or permanent) contracts. The latter frequently have higher dismissal costs which are absent in the former. As in other European countries, Portugal implemented a labor market reform in 2004 that gave greater flexibility to the use of fixed-term contracts and maintained a high level of employment protection for permanent employees.[11] As noted by Centeno and Novo (2012b), this duality "splits the labor market and spreads unevenly the composition of job and worker flows among the two types of contracts". This gave us the impetus to explicitly consider the two-tier structure of the Portuguese labor market. The equations of section 2 can be extended to accommodate this structure. Let E^f denotes temporary and E^p permanent employment, this four-state model evolves according to the following difference equations (as a function of the hazard rates):

$$E_t^p = E_{t-1}^p - (\lambda_t^{E^pU} + \lambda_t^{E^pE^f} + \lambda_t^{E^pI})E_{t-1}^p + \lambda_t^{UE^p}U_{t-1} + \lambda_t^{E^fE^p}E_{t-1}^f + \lambda_t^{IE^p}I_{t-1}, \tag{4.9}$$

$$E_t^f = E_{t-1}^f - (\lambda_t^{E^fU} + \lambda_t^{E^fE^p} + \lambda_t^{E^fI})E_{t-1}^f + \lambda_t^{UE^f}U_{t-1} + \lambda_t^{E^pE^f}E_{t-1}^p + \lambda_t^{IE^f}I_{t-1}, \tag{4.10}$$

$$U_t = U_{t-1} - (\lambda_t^{UE^p} + \lambda_t^{UE^f} + \lambda_t^{UI})U_{t-1} + \lambda_t^{E^pU}E_{t-1}^p + \lambda_t^{E^fU}E_{t-1}^f + \lambda_t^{IU}I_{t-1}, \tag{4.11}$$

$$I_t = I_{t-1} - (\lambda_t^{IE^p} + \lambda_t^{IE^f} + \lambda_t^{IU})I_{t-1} + \lambda_t^{E^pI}E_{t-1}^p + \lambda_t^{E^fI}E_{t-1}^f + \lambda_t^{UI}U_{t-1}, \tag{4.12}$$

where λ_t^X is the hazard rate defined as in section 2 and $X, Y \in \left\{E^p, E^f, U, I\right\}$.

The construction of the flows is broadly similar to the procedure described in section 4.1. However, prior to the matching process, we have to re-classify the respondents considering the disaggregation of employment. Figure 4.6 plots the estimated flows and Table 4.6 reports the average hazard rates between the four groups. In what follows we will report the values after the redesign in parentheses.

Examination of Table 4.6 reveals that the unemployed are roughly five (seven) times more likely to find a temporary job than a permanent one. Relatively to those with open-ended contracts, the temporary workers are about three (four) times more likely to move into unemployment and two (seven) times more likely to join the pool of inactive. These numbers suggest that in dual labor markets firms seem to use fixed-term contracts as the main mechanism for hiring and firing workers (a similar result to the one obtained in Silva and Vázquez-Grenno (2012) for Spain). However, substantial differences emerge in the average transition probabilities after the redesign, which might be a symptom of significant design effects in the worker flows series.

[11] See Centeno and Novo (2012a) and Centeno and Novo (2012b) for an overview on this reform and its main consequences.

Table 4.6: Transition matrix, disaggregating employment (% per quarter).

From:		Inactivity		Unemployment		Employment (temporary)		Employment (permanent)	
To:		Before	After	Before	After	Before	After	Before	After
Inactivity	Before	77.234		13.316		1.509		0.688	
	After		68.174		10.648		7.212		1.066
Unemployment	Before	0.997		50.419		1.334		0.500	
	After		1.868		46.144		4.153		1.069
Employment (temporary)	Before	0.791		11.841		74.126		0.731	
	After		2.672		11.306		52.888		3.842
Employment (permanent)	Before	0.216		2.215		1.630		76.811	
	After		0.311		1.587		6.872		66.565

Source: Author's calculations based on LFS.

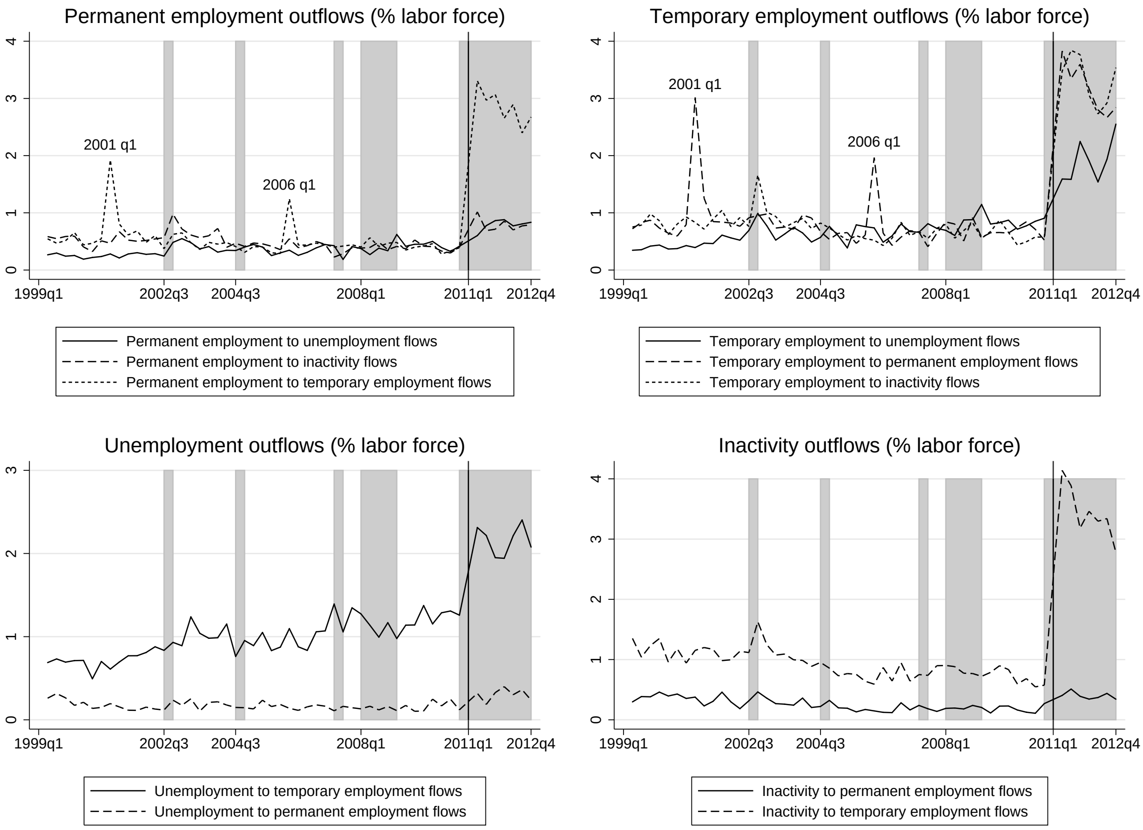
To investigate the design effects we use the RD approach again. The steps are rather similar to the previous sections: we seasonally adjust the flows with CB X-12 and then estimate eq. (4.8) with $p = 2$ for each of the twelve series. The right-hand side variables of eq. (4.8) are defined as usual. Tables 4.7 and 4.8 show the results obtained. As we are only disaggregating employment, the estimates for U$\leftrightarrow$I flows are equal to those of section 4.2.4.

The results give some strength to the previous findings. Again, even disaggregating employment by type of contract, strong design effects are found in E$\leftrightarrow$I flows. In particular, the redesign had an effect of 1.818 p.p. increase in flows from E^f to I and 1.801 p.p. in the reverse movement. Regarding the E^p $\leftrightarrow$ I flows, the redesign's effect was 0.396 p.p. increase in E^p $\rightarrow$ I and 0.155 p.p. increase in I $\rightarrow$ E^p. The magnitude of the design coefficients suggests a higher effect in flows between E^f and I, relatively to flows between E^p and I. For example, the average of E^p $\rightarrow$I flows increases by 65 percent after the redesign (0.479 percent vs. 0.791 percent), while the average of E^f $\rightarrow$I flows comes more than four times higher after the redesign (0.743 percent vs. 3.333 percent). Figure 4.6 depicts this: the jump of both E^f $\rightarrow$I and I $\rightarrow$ E^f flows comes markedly sharp compared to the one of E^p $\rightarrow$I and I $\rightarrow$ E^p. Hence, a large part of the discontinuity in flows between E and I seems to be due to the effect of the redesign in flows between E^f and I.

The picture of flows between E^p and U, and of flows between E^f and U is also quite revealing. In section 4.2.4, both the "Design dummy" and the cyclical coefficient were found significant in E$\rightarrow$U flows, while only the "Design dummy" was found significant in U$\rightarrow$E flows. The disaggregation of employment allows us to observe that redesign did not affect the flows between permanent employment and unemployment, since the "Design dummy" for both E^p $\rightarrow$ U and U $\rightarrow$ E^p comes non-significant. Furthermore, a significant countercyclical pattern is found in E^p $\rightarrow$ U flows. Figure 4.6 give an intuitive view of this: the E^p $\rightarrow$ U and U $\rightarrow$ E^p series exhibit no jump, apart from a smooth increase in flows from E^p to U, certainly related to their cyclical properties.

On the other hand, the flows between E^f and U appear to be strongly affected by the redesign. The "Design dummy" had an estimate of 0.480 p.p. increase in E^f $\rightarrow$ U flows and 0.629 p.p. increase in the U $\rightarrow$ E^f flows. In addition, the cyclical coefficient of E^f $\rightarrow$ U reveals a countercyclical pattern significant at the 10 percent level. By comparing the cyclical effect in the first quarter of 2009 with all quarters since 2011, we reach similar conclusions to those drawn in section 4.2.4: insignificant differences in the cyclical effects between quarters lead to the redesign as the main source of the discontinuity in E^f $\rightarrow$ U flows.

Figure 4.6: Labor market flows: disaggregating employment



Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

In short, by disaggregating employment, we find that it is essentially the redesign's effect in flows between temporary employment and unemployment that explain the discontinuity in flows between E and U, despite the countercyclical pattern of both $E^p \rightarrow U$ and $E^f \rightarrow U$ flows.

Every quarter, 1.630 percent of the employed with a fixed-term contract change to a permanent one, while only 0.731 percent do the reverse movement. After the redesign these values changed to 6.872 percent and 3.842 percent, respectively. Significant design effects uncover this differences: the redesign is estimated to account for 2.626 p.p. increase in $E^p \rightarrow E^f$ and 2.720 p.p. in $E^f \rightarrow E^p$. The flows from E^p to E^f also reveal a countercyclical pattern significant at the 10 percent level. This suggests that in two-tier labor markets recessions might be periods where permanent employment is converted into temporary. Although, by comparing again the business cycle effects between quarters, the discontinuity in the $E^p \rightarrow E^f$ series is clearly determined by the redesign.

However, a countercyclical behavior in flows from E^p to E^f seems contradictory with the changes in the average percentage of employment by type of contract after the redesign: the percentage of permanent employment increased from 58.8 to 61.8 percent and thus, the percentage of temporary employment decreased from 41.2 to 38.2 percent. More than structural changes in the contractual system of the Portuguese labor market, this differences seem to reflect the definitional changes brought by the redesign. Indeed, previous episodes of definitional change in the measurement of employment by type of contract originated peaks in the $E^p \leftrightarrow E^f$ flows, namely at the first quarters of 2001 and 2006, as it is displayed in E^p and E^f outflow charts of Figure 4.6.[12]

[12] The response category "collective labor agreement" was dropped and aggregated in the open-ended contracts in 2001, while in 2006 was introduced the response category "contract with a temporary-work agency".

Table 4.7: RD estimates: Employment outflows, disaggregating employment

	Permanent Employment			Temporary Employment		
	$E^p \rightarrow U$	$E^p \rightarrow I$	$E^p \rightarrow E^f$	$E^f \rightarrow U$	$E^f \rightarrow I$	$E^f \rightarrow E^p$
Design Effects						
Design dummy	0.056	0.396	2.626	0.480	1.818	2.720
	(0.385)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Business Cycle Effects						
GDP cyclical component	-2.706	-2.084	5.276	-2.763	-0.105	5.760
	(0.002)	(0.146)	(0.095)	(0.069)	(0.974)	(0.203)
Polynomials						
$\widetilde{\text{Time}}$	0.002	-0.008	0.010	0.006	-0.011	0.014
	(0.546)	(0.110)	(0.364)	(0.257)	(0.373)	(0.394)
$\widetilde{\text{Time}}^2$	-0.000	-0.000	0.000	-0.000	-0.000	0.000
	(0.470)	(0.476)	(0.170)	(0.428)	(0.801)	(0.201)
Interactions						
Design$\times\widetilde{\text{Time}}$	0.176	0.039	-0.202	0.188	0.570	-0.154
	(0.000)	(0.556)	(0.250)	(0.009)	(0.000)	(0.541)
Design$\times\widetilde{\text{Time}}^2$	-0.021	-0.004	0.025	-0.014	-0.069	0.007
	(0.000)	(0.628)	(0.376)	(0.159)	(0.002)	(0.871)
Adjust. R^2	0.851	0.513	0.911	0.929	0.912	0.833
F-Statistic	52.203	10.463	91.283	118.331	93.833	45.192
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000

Table 4.8: RD estimates: Unemployment and Inactivity outflows, disaggregating employment

	Unemployment outflows			Inactivity outflows		
	$U \rightarrow E^p$	$U \rightarrow E^f$	$U \rightarrow I$	$I \rightarrow E^p$	$I \rightarrow E^f$	$I \rightarrow U$
Design Effects						
Design dummy	0.036	0.629	0.539	0.155	1.801	0.996
	(0.441)	(0.000)	(0.000)	(0.007)	(0.000)	(0.000)
Business Cycle Effects						
GDP cyclical component	-1.277	0.889	-3.177	0.116	-0.457	-5.349
	(0.036)	(0.540)	(0.046)	(0.868)	(0.872)	(0.001)
Polynomials						
$\widetilde{\text{Time}}$	0.004	0.009	0.010	0.001	-0.008	-0.009
	(0.067)	(0.089)	(0.085)	(0.571)	(0.459)	(0.123)
$\widetilde{\text{Time}}^2$	0.000	-0.000	-0.000	0.000	0.000	-0.000
	(0.020)	(0.418)	(0.707)	(0.006)	(0.598)	(0.001)
Interactions						
Design$\times\widetilde{\text{Time}}$	0.049	0.088	-0.085	0.051	0.643	0.054
	(0.078)	(0.191)	(0.241)	(0.117)	(0.000)	(0.454)
Design$\times\widetilde{\text{Time}}^2$	-0.007	-0.007	0.021	-0.008	-0.083	-0.003
	(0.080)	(0.440)	(0.036)	(0.085)	(0.000)	(0.752)
Observations	55	55	55	55	55	55
Adjust. R^2	0.539	0.931	0.896	0.695	0.931	0.929
F-Statistic	11.512	122.394	78.541	21.504	122.035	117.906
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. E^p: Permanent employment. E^f: Temporary employment.
The tilde denotes the forcing variable centered at the discontinuity point.

Table 4.9: Transition matrix, disaggregating inactivity (% per quarter).

From:		Employment		Unemployment		Inactive (want)		Inactive (out)	
To:		Before	After	Before	After	Before	After	Before	After
Employment	Before	76.806		14.056		10.626		1.200	
	After		66.322		12.893		8.255		4.391
Unemployment	Before	0.843		50.419		18.017		0.939	
	After		2.251		46.144		16.997		1.583
Inactive (want)	Before	0.184		5.135		28.979		0.513	
	After		0.621		6.128		21.029		3.793
Inactive (out)	Before	0.829		7.719		18.927		77.143	
	After		2.233		3.422		23.529		63.672

Source: Author's calculations based on LFS.

4.4.2 Disaggregating inactivity

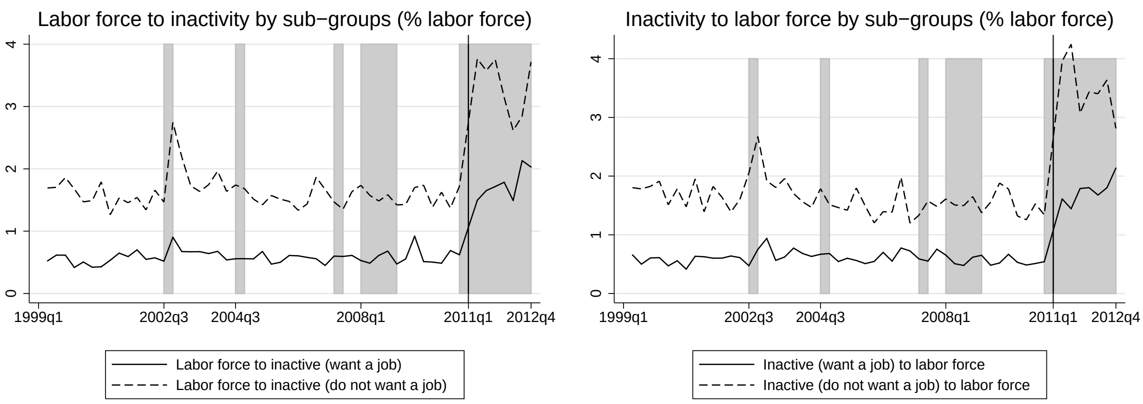
The magnitude of the flows in and out of inactivity have led many researchers to ask if unemployment and inactivity are behaviorally distinct labor force states. Flinn and Heckman (1983) analyzed the conditional and unconditional transition probabilities of the two states and rejected this hypothesis for the US. Joyce et al. (2003), for the UK, found that many subgroups of the inactives have the same transition probability to employment as the unemployed. Blanchard and Diamond (1990) also found useful to disaggregate the inactives into two sub-groups: those who *want a job* (I^w) and those who *do not want a job* (I^o). They argued that this distinction makes sense because the inactives who *want a job* can be considered marginally close to the labor force, and thus they are more likely to go into the labor force.

We did the same exercise for the Portuguese labor market and disaggregated inactivity into the referred sub-groups. This seems particularly relevant if we take into account that the new methodology made unemployment a more "demanding" labor market state, because the same individual in the same circumstances could be classified as unemployed prior to the redesign and inactive under the new method, as discussed in sections 3.1.2 and 3.2. Indeed, the average proportion of inactives that *want a job* substantially increased after the redesign (2.627 percent vs. 11.150 percent). In order to compute the worker flows by subgroups of inactivity, we re-classified the inactives according to their availability to work, before the matching process discussed in section 4.1. Figure 4.7 plots the estimated flows and Table 4.9 reports the average hazard rates between the four groups.

As Table 4.9 displays, the inactives who *want a job* are almost nine (two) times more likely to find a job and nineteen (ten) times more likely to join the pool of unemployed than the inactives who *do not want a job*. In turn, the unemployed are around one third (one half) more likely to find a job than the inactives who *want a job*. Furthermore, every quarter, 5.135 (6.128) percent of the unemployed move into inactivity but still *want a job*, while 18.927 (23.529) percent of the inactives who *want a job* abandon their intentions by the following quarter. These results show substantial differences in the worker flows (expressed as hazard rates) after the redesign, as usual we will make use of the RD approach to investigate the redesign's effect in the flows series.

The flows were seasonally adjusted using CB X-12 and eq. (4.8) was fitted for each of the twelve flows series, with $p = 2$. Tables 4.10 and 4.11 show the results. Since only inactivity is being disaggregated, the E↔U flows are the same as in the aggregated case of section 4.2.4.

Figure 4.7: Labor market flows: disaggregating inactivity



Source: Author's calculations based on LFS.
Note: Shadings indicate recessions. The vertical line signals the revised survey.

The results suggest that the redesign greatly affected the flows between I^w and U. It is found an estimate of 0.280 p.p. increase in the flows from U to I^w (the average flows changed from 0.370 percent to 1.078 percent) and 0.365 p.p. increase in the flows from I^w to U (0.373 vs. 1.179 percent). Additionally, the magnitude and the statistical significance of the cyclical coefficient reveal a strong countercyclical pattern for the $\mathrm{U} \leftrightarrow \mathrm{I}^w$ flows, suggesting that recessions are periods in which flows between the unemployed and the marginally attached significantly increase. Still, there is nothing markedly different in the reaction to cyclical fluctuations of $\mathrm{U} \leftrightarrow \mathrm{I}^w$ flows after the redesign. For example, the difference of the cyclical effects in flows from U to I^w between the first quarter of 2011 and the first of 2009 was -0.070 p.p., using the fourth quarter of 2012 as reference this value changes to 0.017 p.p. Again, insignificant differences are found in the cyclical effects between recessionary quarters and thus, the redesign seems to explain a large part of the $\mathrm{U} \leftrightarrow \mathrm{I}^w$ discontinuity.

The methodological changes seem to not have had any effect in the flows between unemployment and the inactives who *do not want a job.* Indeed, the "Design dummy" appears widely non-significant in both $\mathrm{U} \rightarrow \mathrm{I}^o$ and $\mathrm{I}^o \rightarrow \mathrm{U}$ flows, although it should be noted the statistical significance of the cyclical coefficient in flows from I^o to U. The disaggregation of inactivity is quite enlightening relatively to what was found at the three-state set-up: it allows us to conclude that the effects of methodological change in flows between U and I concentrate in flows between unemployment and the inactives who *want a job.*

The estimates for both $\mathrm{E} \leftrightarrow \mathrm{I}^o$ and $\mathrm{E} \leftrightarrow \mathrm{I}^w$ reveal strongly significant design effects, coherently to the previous findings that extensively attributed the discontinuity of E$\leftrightarrow$I flows to the redesign. However, the cyclical coefficient of the $\mathrm{I}^w \rightarrow \mathrm{E}$ flows comes positive and statistically significant at the 10 percent level, which reveals that in downturns the job finding rate of the marginally attached goes down as well. Compared to the unemployed, the recessions seem to be particularly damaging for the job finding rate of the marginally attached, since the cyclical coefficient of U$\rightarrow$E flows indicates a procyclical pattern as well, but it comes non-significant.

Finally, the discontinuity in $\mathrm{I}^w \leftrightarrow \mathrm{I}^o$ flows is fully explained by methodological change, which is probably a result of the major definitional changes that broaden the spectrum of inactivity.

These findings give a deeper insight to what is displayed in Figure 4.7, where flows between inactivity and the labor force appear to be somewhat sensitive to the business cycle. The countercyclicality of inactivity inflows appears to be driven by flows of individuals from unemployment into a limbo between inactivity and the labor force, in turn the countercyclical pattern of inactivity outflows seems to be due to flows from both groups of inactives into unemployment.

Table 4.10: RD estimates: Employment and Unemployment outflows, disaggregating inactivity

	Unemployment outflows			Employment outflows		
	$U \to I^{w}$	$U \to I^{o}$	$U \to E$	$E \to I^{w}$	$E \to I^{o}$	$E \to U$
Design Effects						
Design dummy	0.280	0.020	1.175	0.342	1.516	0.901
	(0.000)	(0.841)	(0.000)	(0.000)	(0.000)	(0.000)
Business Cycle Effects						
GDP cyclical component	-2.273	-1.286	-0.227	0.664	-2.713	-5.342
	(0.011)	(0.308)	(0.892)	(0.488)	(0.385)	(0.007)
Polynomials						
$\widetilde{\text{Time}}$	0.006	0.004	0.013	-0.006	-0.014	0.008
	(0.047)	(0.436)	(0.034)	(0.097)	(0.228)	(0.228)
$\widetilde{\text{Time}}^2$	0.000	-0.000	0.000	-0.000	-0.000	-0.000
	(0.400)	(0.325)	(0.874)	(0.436)	(0.693)	(0.370)
Interactions						
$\text{Design} \times \widetilde{\text{Time}}$	0.035	-0.095	-0.125	0.135	0.316	0.145
	(0.383)	(0.104)	(0.112)	(0.003)	(0.032)	(0.106)
$\text{Design} \times \widetilde{\text{Time}}^2$	0.007	0.011	0.015	-0.014	-0.043	-0.007
	(0.208)	(0.192)	(0.165)	(0.023)	(0.035)	(0.585)
Observations	55	55	55	55	55	55
Adjust. R^2	0.931	0.478	0.931	0.818	0.837	0.941
F-Statistic	123.378	9.238	122.625	41.317	47.218	144.308
Prob $>$ F	0.000	0.000	0.000	0.000	0.000	0.000

Table 4.11: RD estimates: Inactivity outflows, disaggregating inactivity

	Inactive (want)			Inactive (out)		
	$I^{w} \to E$	$I^{w} \to U$	$I^{w} \to I^{o}$	$I^{o} \to E$	$I^{o} \to U$	$I^{o} \to I^{w}$
Design Effects						
Design dummy	0.190	0.365	0.503	1.323	0.138	1.372
	(0.000)	(0.000)	(0.000)	(0.000)	(0.144)	(0.000)
Business Cycle Effects						
GDP cyclical component	1.106	-2.057	0.708	-1.524	-3.695	1.750
	(0.067)	(0.022)	(0.574)	(0.564)	(0.003)	(0.206)
Polynomials						
$\widetilde{\text{Time}}$	-0.001	-0.005	0.004	-0.005	-0.004	0.009
	(0.720)	(0.119)	(0.435)	(0.609)	(0.382)	(0.065)
$\widetilde{\text{Time}}^2$	0.000	-0.000	0.000	0.000	-0.000	0.000
	(0.070)	(0.012)	(0.199)	(0.339)	(0.007)	(0.086)
Interactions						
$\text{Design} \times \widetilde{\text{Time}}$	0.079	0.159	0.012	0.454	0.003	0.133
	(0.005)	(0.000)	(0.862)	(0.000)	(0.954)	(0.088)
$\text{Design} \times \widetilde{\text{Time}}^2$	-0.006	-0.011	0.016	-0.062	-0.002	-0.010
	(0.122)	(0.054)	(0.161)	(0.001)	(0.843)	(0.414)
Adjust. R^2	0.857	0.937	0.873	0.883	0.649	0.967
F-Statistic	54.799	134.375	61.679	68.608	17.660	256.110
Prob $>$ F	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. I^w: Inactives who *want a job*. I^o: Inactives who *do not want a job*. The tilde denotes the forcing variable centered at the discontinuity point.

5 Outs of Employment

So far, we have been assessing the redesign's impact in labor market flows at a macroeconomic level. In this section, we will frame our analysis at a microeconomic level. Using cross-sectional and longitudinal models, the effects of the redesign and the micro determinants of labor market mobility are estimated and compared using the previous and the revised LFS.

5.1 Cross-sectional approach

This approach involves the construction of two cross-sectional datasets from the LFS: the first drawn from the fourth quarter of 2010 (before the redesign) and the second from the fourth of 2012 (the eighth quarter after the redesign, where a certain accommodation to the new method is expected). Between cross-sections, the proportion of transitions out of employment changed from 2.6 percent (before) to 10.3 percent (after) (Table 5.5). This shift in the proportion of exits might be a symptom of a relationship between the probability to record exits and the redesign.

We start by considering the following model specification:

$$Y_i = \alpha + \boldsymbol{\beta}'\boldsymbol{x}_i + \rho Design_i + \sum_{j=1}^{k} \delta_j (Design_i \times x_{ij}) + \epsilon_i, \tag{5.1}$$

where Y_i assumes value 1 if individual i exits employment and 0 otherwise; $Design_i$ indicates the survey data source, assuming value 1 after the LFS redesign and 0 before; $\boldsymbol{x}_i$ is a vector that includes a set of covariates, namely: age cohorts; gender indicator; marital status indicator; type of contract indicator; education dummies; sector dummies and region dummies. And ϵ_i is a conventional error term, assumed to be uncorrelated with the regressors.

We estimate eq. (5.1) on the combined datasets without restrictions (Pooled) and imposing $Design_i = 1$ (After). Obviously, vector $\hat{\boldsymbol{\beta}}$ obtained by estimating eq. (5.1) without restrictions is the same as the one obtained by imposing $Design_i = 0$ (Before). In this way, the null hypothesis that the effects of the covariates on transitions out of employment are equal in the two surveys is tested (H_0: $\delta_j = 0$). If the interactions turn out to be statistically significant, then significant differences in the effects of the covariates rise with the redesign. The redesign's specific effect in the probability to exit employment is given by the point estimate of ρ.

We estimate eq. (5.1) through Probit and Linear Probability Model (LPM). These standard estimators needed to be adjusted to survey data, because the observations are not typically *independent and identically distributed* (i.i.d.) due to different sample inclusion probabilities that derive from the multistage sample stratification procedure (section 3.1). To compensate the unequal probabilities of selection, sampling weights are incorporated into the estimation procedures in order to get unbiased estimates of the parameters and correct standard errors.

In the context of LMP, the sampling weights can be incorporated into the estimation of the regression parameters via the use of weighted least squares estimation (WLS), where the contribution of each sample element to the residual sum of squares is made proportional to its population weight. As complex sample survey data violates the assumption of independence of observations, the straightforward application of maximum likelihood estimation procedures is no longer possible. Therefore, the Probit estimates are obtained through an iterative estimation procedure used to determine the values of the coefficients that maximize a weighted pseudo-likelihood function. In addition, pseudo-R^2 is not reported for the Probit estimates, because it is computed using log likelihoods and log likelihoods assume that sample elements are all independent of each other, which is violated by survey data.[13]

[13] See Appendix B and Pfeffermann (1993) for details.

5.1.1 Results

Tables 5.1 and 5.2, and Tables 5.3 and 5.4 show the estimates of eq. (5.1) in the combined datasets (Pooled) and imposing $Design_i = 1$ (After) by Probit and LPM, respectively. Since the majority of the regressors are categorical variables, the marginal effects in Probit are the average partial effects (Margins).[14]

An individual interviewed under the revised LFS is 16.1 p.p. (35.6 p.p. by LMP) more likely to be recorded as transitioning out of employment than an individual interviewed before the redesign, with every other covariates held fixed. This evidences that the new methodology significantly increased the probability to record transitions out of employment, relatively to the old method. The question now is whether the effects of labor market mobility determinants also come significantly different after the redesign.

The results are broadly consistent with the idea that young people are more likely to move out of employment. After the redesign, an individual between 45 and 54 years old is in average 9.8 p.p. less likely to exit employment, compared to an individual in the 15-24 age cohort. This value was 4.3 p.p. before the redesign. In the remain cohorts, the magnitude of the age affects also appear higher *after*.

The results provide evidence that permanent contracts prevent individuals from exit employment. Still, there are considerable differences in the strength of the contractual effect across surveys: while under the previous LFS an individual with an open-ended contract is in average 2.3 p.p. less likely to move out of employment, relatively to other type of contract, in the revised LFS this probability comes about three times higher. Nevertheless, these results remain fairly consistent with the notion that open-ended contracts are better protected than others and people with such contracts are less likely to experience a departure from employment (consistently to section 4.4.1).

All individuals with higher levels of education than Educ1 are less likely to exit employment. It is worth noting that in the previous LFS only the transition probabilities of individuals with Educ5 were statistically significant, whilst in the revised LFS the transition probabilities appear significant for both Educ4 and Educ5. Again, these results are quite coherent with the conventional wisdom that investments in education positively affect labor market outcomes.

Those who live in other regions than *Lisboa* seem to have a low probability to exit employment. However, these region effects are solely significant for those living in Azores (the 10 percent level) and in the Center (only in the revised survey).

Contrary to what could be expected, before the redesign all sectors (except Public Administration in Probit) have significant higher probabilities to exit employment compared to Agriculture. For example, an individual working in Services is 3.7 p.p. more likely to move out of employment, relatively to another working in Agriculture. After the redesign all the signs change and Public Administration becomes statistically significant, while Electricity and Construction appear significant at the 10 percent level.

The effects of the covariates on transitions out of employment appear to be somewhat different between surveys. Tables 5.2 and 5.4 present the interaction terms in Probit and LPM, respectively. The interactions with "Contract", "Center" and with all industries are found statistically significant. The LPM estimates are consistent with Probit, however the interactions with age cohorts come significant in LMP, too. Additionally, we tested the null hypothesis that all interaction terms are jointly zero (H_0: $\delta_j = 0, j = 1, \ldots, k$) through a Wald test, and it was rejected at any level of significance in both models.

[14] Computed through $\frac{1}{N}\sum_{i=1}^{N}\left[G\left(\hat{\alpha} + \hat{\beta}_1 x_{i,1} + \cdots + \hat{\beta}_{k-1} x_{i,k-1} + \hat{\beta}_k\right) - G(\hat{\alpha} + \hat{\beta}_1 x_{i1} + \cdots + \hat{\beta}_{k-1} x_{i,k-1})\right]$ where $G(.)$ is the standard normal cdf.

Table 5.1: Probit model for transitions out of employment

	(1) Pooled		(2) After	
	β / p-value	Margins	β / p-value	Margins
Design Effects				
Design (=1 after the LFS redesign)	1.718	0.161	–	–
	(0.000)			
Demographics				
Male (=1)	–0.078	–0.007	–0.070	–0.010
	(0.275)		(0.203)	
Married/living as married (=1)	–0.142	–0.013	–0.131	–0.018
	(0.069)		(0.027)	
Age Cohorts				
25-34 years old	–0.195	–0.024	–0.374	–0.068
	(0.082)		(0.000)	
35-44 years old	–0.454	–0.048	–0.585	–0.096
	(0.000)		(0.000)	
45-54 years old	–0.395	–0.043	–0.604	–0.098
	(0.002)		(0.000)	
55-64 years old	–0.366	–0.041	–0.383	–0.069
	(0.008)		(0.001)	
65 years old or older	–0.154	–0.020	–0.283	–0.054
	(0.384)		(0.028)	
Type of Contract				
Contract (=1 if Permanent)	–0.405	–0.038	–0.738	–0.101
	(0.000)		(0.000)	
Education				
Educ2	–0.173	–0.020	–0.081	–0.014
	(0.236)		(0.409)	
Educ3	–0.140	–0.016	–0.157	–0.025
	(0.409)		(0.192)	
Educ4	–0.246	–0.027	–0.354	–0.051
	(0.161)		(0.005)	
Educ5	–0.511	–0.047	–0.506	–0.067
	(0.005)		(0.000)	
Industry				
Extractive and Manufactoring	0.338	0.024	–0.565	–0.086
	(0.032)		(0.000)	
Electricity and Construction	0.499	0.039	–0.174	–0.032
	(0.003)		(0.090)	
Services	0.479	0.037	–0.403	–0.067
	(0.000)		(0.000)	
Public Administration	0.288	0.019	–0.897	–0.114
	(0.180)		(0.000)	
Region				
Azores	–0.240	–0.020	–0.255	–0.035
	(0.066)		(0.012)	
Alentejo	–0.021	–0.002	–0.113	–0.016
	(0.857)		(0.170)	
Algarve	0.175	0.019	0.019	0.003
	(0.101)		(0.811)	
Center	0.019	0.002	–0.250	–0.034
	(0.867)		(0.004)	
Madeira	–0.056	–0.005	–0.079	–0.012
	(0.653)		(0.397)	
North	–0.058	–0.005	–0.115	–0.017
	(0.559)		(0.110)	
Observations	19655		9087	
F-Statistic	28.319		32.073	
Prob > F	0.000		0.000	

Source: Author's calculations based on LFS.
Note: p-values in parentheses. Reference groups: 15-24 years old; Educ1; Agriculture; Lisboa.

Table 5.2: Probit model for transitions out of employment (*Cont.*)

	(1) Pooled
Interactions with Demographics	
Male×Design	0.008
	(0.928)
Married×Design	0.011
	(0.909)
Interactions with Age Cohorts	
25-34 years old×Design	–0.178
	(0.250)
35-44 years old×Design	–0.131
	(0.436)
45-54 years old×Design	–0.209
	(0.225)
55-54 years old×Design	–0.017
	(0.926)
65 years old or older×Design	–0.128
	(0.558)
Interactions with Type of Contract	
Contract×Design	–0.333
	(0.000)
Interactions with Education	
Educ2×Design	0.092
	(0.600)
Educ3×Design	–0.017
	(0.937)
Educ4×Design	–0.108
	(0.618)
Educ5×Design	0.005
	(0.983)
Interactions with Region	
Azores×Design	–0.016
	(0.925)
Alentejo×Design	–0.092
	(0.510)
Algarve×Design	–0.156
	(0.245)
Center×Design	–0.269
	(0.058)
Madeira×Design	–0.023
	(0.884)
North×Design	–0.056
	(0.646)
Interactions with Industry	
Extractive and Manufactoring×Design	–0.903
	(0.000)
Electricity and Construction×Design	–0.673
	(0.001)
Services×Design	–0.882
	(0.000)
Public Administration×Design	–1.185
	(0.000)
Observations	19655
F-Statistic	28.319
Prob > F	0.000

Table 5.3: LPM for transitions out of employment

	(1) Pooled	(2) After
Design Effects		
Design (=1 after the LFS redesign)	0.356	–
	(0.000)	
Demographics		
Male (=1)	–0.005	–0.013
	(0.248)	(0.069)
Married/living as married (=1)	–0.008	–0.014
	(0.078)	(0.073)
Age Cohorts		
25-34 years old	–0.025	–0.089
	(0.038)	(0.000)
35-44 years old	–0.037	–0.111
	(0.002)	(0.000)
45-54 years old	–0.034	–0.112
	(0.004)	(0.000)
55-64 years old	–0.034	–0.086
	(0.005)	(0.000)
65 years old or older	–0.023	–0.039
	(0.138)	(0.200)
Type of Contract		
Contract (=1 if Permanent)	–0.025	–0.102
	(0.000)	(0.000)
Education		
Educ2	–0.010	–0.032
	(0.342)	(0.243)
Educ3	–0.007	–0.036
	(0.546)	(0.205)
Educ4	–0.014	–0.059
	(0.244)	(0.039)
Educ5	–0.025	–0.071
	(0.027)	(0.011)
Industry		
Extractive and Manufactoring	0.020	–0.130
	(0.007)	(0.000)
Electricity and Construction	0.028	–0.077
	(0.002)	(0.000)
Services	0.028	–0.114
	(0.000)	(0.000)
Public Administration	0.019	–0.133
	(0.035)	(0.000)
Region		
Azores	–0.010	–0.033
	(0.107)	(0.010)
Alentejo	–0.001	–0.013
	(0.851)	(0.244)
Algarve	0.015	0.009
	(0.066)	(0.463)
Center	0.000	–0.034
	(0.986)	(0.002)
Madeira	–0.002	–0.012
	(0.728)	(0.366)
North	–0.003	–0.016
	(0.553)	(0.096)
Observations	19655	9087
R^2	0.104	0.110
F-Statistic	19.101	30.056
Prob > F	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. Reference groups: 15-24 years old; Educ1; Agriculture; Lisboa.

Table 5.4: LPM for transitions out of employment (*Cont.*)

	(1) Pooled
Interactions with Demographics	
Male×Design	–0.009
	(0.288)
Married×Design	–0.005
	(0.557)
Interactions with Age Cohorts	
25-34 years old×Design	–0.064
	(0.014)
35-44 years old×Design	–0.074
	(0.004)
45-54 years old×Design	–0.077
	(0.003)
55-54 years old×Design	–0.052
	(0.055)
65 years old or older×Design	–0.017
	(0.622)
Interactions with Type of Contract	
Contract×Design	–0.078
	(0.000)
Interactions with Education	
Educ2×Design	–0.022
	(0.454)
Educ3×Design	–0.029
	(0.349)
Educ4×Design	–0.045
	(0.145)
Educ5×Design	–0.046
	(0.130)
Interactions with Region	
Azores×Design	–0.023
	(0.113)
Alentejo×Design	–0.012
	(0.362)
Algarve×Design	–0.006
	(0.705)
Center×Design	–0.034
	(0.008)
Madeira×Design	–0.009
	(0.530)
North×Design	–0.012
	(0.263)
Interactions with Industry	
Extractive and Manufactoring×Design	–0.150
	(0.000)
Electricity and Construction×Design	–0.105
	(0.000)
Services×Design	–0.142
	(0.000)
Public Administration×Design	–0.153
	(0.000)
Observations	19655
R^2	0.104
F-Statistic	19.101
Prob > F	0.000

5.2 Longitudinal approach

There might be time-invariant individual-specific unobserved factors that make longitudinal methods particularly suited to model transitions out of employment. For example, if Blanchard and Diamond's theory holds (section 2.2), then individuals with a strong attachment to the labor market are less likely to exit employment, which is a source of unobserved heterogeneity. This and the need to strengthen the findings of the previous section lead us to implement a longitudinal approach. For that, we constructed a panel dataset from the LFS microdata, ranging from the second quarter of 2009 to the fourth of 2012. As discussed in section 4.1, the redesign changed the unique identifiers, becoming impossible to match individuals between the fourth quarter of 2010 to the first of 2011. Therefore, we were forced to drop the first quarter of 2011, which led to $T = 14$ quarters. Given the rotating structure of the survey our panel is necessarily unbalanced. Still, 95 percent of individuals are at least five quarters in the sample.

We consider the following model specification:

$$Y_{it} = \boldsymbol{\beta}'\boldsymbol{x}_{it} + \rho Design_{it} + \sum_{j=1}^{k} \delta_j (Design_{it} \times x_{j,it}) + \mu_i + \epsilon_{it}, \tag{5.2}$$

which is an extended version of eq. (5.1) to longitudinal data and where variables are defined similarly: Y_{it} assumes value 1 if individual i is employed in $t-1$ and non-employed in t, and 0 otherwise; $\boldsymbol{x}_{it}$ is a vector that contains the same variables as defined above; $Design_{it}$ assumes value 1 in the quarters after the redesign and 0 before; μ_i is a random individual-specific effect and ϵ_{it} is an idiosyncratic error. We also assume $\mu_i \sim iid(0, \sigma_\mu^2)$ and $\epsilon_{it} \sim iid(0, \sigma_\epsilon^2)$.

We would like to state that this approach is hampered by some drawbacks: (i) The estimations do not take into account the nature of survey data, since we had no access to essential survey design elements for panel survey estimation, and ignoring that might lead to inaccurate point estimates and standard errors;[15] (ii) The change in the unique identifiers made us unable to follow the waves initiated prior to the redesign, which turned our variable of interest $Design_{it}$ time-invariant. As a consequence, we have to frame our analysis in the random effects (RE) assumptions; (iii) It is not possible to control for the external economic situation by including time dummies, since $Design_{it}$ is a linear combination of the time quarter dummies after the redesign.

We estimate eq. (5.2) by the LPM through Pooled OLS (POLS), Between and RE estimators.[16] These three estimators are consistent under the assumption that both μ_i and ϵ_{it} are uncorrelated with the regressors. To correct for correlation in the combined error $u_{it} = \mu_i + \epsilon_{it}$, we use cluster robust standard errors in POLS.

5.2.1 Results

Tables 5.6 and 5.7 present the results. Consistently to what was found in the cross-sectional approach, using longitudinal data one finds that, everything held fixed, an individual interviewed under the revised LFS is roughly one third more likely to be recorded as doing a transition out of employment than an individual interviewed under the old method. In particular, the point estimates of the design effect are: 28.1 p.p. by POLS estimator, 34.0 p.p. by RE estimator and 39.5 p.p. by Between estimator. These estimates are close to the point estimate of 35.6 p.p. obtained by LPM in the cross-sectional approach.

[15] Researchers frequently use the Stata module `gllamm` to carry out panel survey estimations (Rabe-Hesketh and Skrondal, 2008). Unlike the standard Stata module used to survey data analysis (`svyset`), `gllamm` requires that all levels of the multistage sample procedure be declared. Unfortunately, this information is not available in our panel survey dataset, which made us unable to incorporate the nature of survey data in the estimation procedures.

[16] We use only LMP due to computational reasons, however, the fact that the results in section 5.1 did not change dramatically with the chosen method gave some strength to this option, see Angrist and Pischke (2009).

Table 5.5: Average Transitions

	(1) Sample Averages	(2) Predicted Values			(3) Simulated Transitions		
		(a)	(b)	(c)	(a)	(b)	(c)
Cross-section							
Before	0.026	0.026	0.026	-	0.104	0.106	-
After	0.103	0.104	0.104	-			
Longitudinal							
Before	0.022	0.022	0.035	0.031	0.093	0.139	0.140
After	0.089	0.090	0.134	0.123			

Note: Cross-section: (a) Probit; (b) LPM; Longitudinal: (a) POLS; (b) Between; (c) RE. Column (1) shows the sample averages. Column (2) shows the average in-sample predicted transitions for each sample. Column (3) shows the average out-of-sample predicted transitions computed over the sample *before* using the coefficients fitted over the sample *after*.

The signs of the covariates are consistent either to those obtained in the cross-sectional approach or between panel estimators, still, some differences emerge in the magnitude of the coefficients. The most noticeable feature seems to be the statistical significance of the majority of the covariates. Although, we need to take some care in drawing conclusions from this, because ignoring the survey elements in the estimation procedures may lead to estimates of the standard errors that are smaller than the *true* standard errors (Pfeffermann, 1993).

In the cross-sectional approach, we found the interaction terms with "Contract", with "Center", with the industries and with the age cohorts statistically significant (the latter only by LMP). Here, the interaction terms with these covariates are also found significant (except for the "Center"), but significant differences in the effects of "Male" and education levels are also found. Again, a Wald test allows us to reject the null hypothesis (H_0: $\delta_j = 0$, $j = 1, \ldots, k$) that interactions are all zero.

5.3 Out-of-sample predictions

In the cross-sectional and longitudinal approaches, we found broadly evidence of a design effect increasing the probability to record movements out of employment for those interviewed under the revised LFS. In addition, significant differences in the factors affecting labor market transitions are found between the previous and the revised surveys.

In order to draw a strong conclusion on what lies behind the differences in the proportion of exits between surveys, we conduct the following experiment: with the coefficients *after* (obtained by imposing $Design_i = 1$ in both eq. (5.1) and eq. (5.2)) we compute the out-of-sample predicted transition probabilities ($\hat{Y}_{it}$) over the sample of the previous LFS.

Table 5.5 shows the results. In all cases, the average transitions obtained using the coefficients *after* given the population characteristics *before* (Column 3) are significantly higher than the sample averages *before*, but come very close to the sample averages *after* (Column 1). For example, in the cross-section drawn from the fourth quarter of 2010 the sample average is 2.6 percent (as the average in-sample predicted values). However, the average out-of-sample predicted transitions come substancially higher (10.4 percent by Probit and 10.6 percent by LMP), but very close to the sample average of the cross-section drawn from the fourth quarter of 2012 (10.4 percent). This suggests that the composition of the sample of the revised LFS (recall section 3.2) might have not positively influenced the likelihood to observe a higher level of transitions out of employment, and thus the differences in the proportion of exits seem to be mostly explained by a redesign effect that rises from the major changes brought by the new method, in particular the definitional ones.

Table 5.6: Longitudinal LPM for transitions out of employment

	(1) POLS	(2) Between	(3) RE
Design Effects			
Design (=1 after the LFS redesign)	0.281	0.395	0.340
	(0.000)	(0.000)	(0.000)
Demographics			
Male (=1)	–0.009	–0.013	–0.014
	(0.000)	(0.000)	(0.000)
Married/living as married (=1)	–0.005	–0.007	–0.009
	(0.000)	(0.017)	(0.001)
Age Cohorts			
25-34 years old	–0.026	–0.046	–0.029
	(0.000)	(0.000)	(0.000)
35-44 years old	–0.034	–0.056	–0.041
	(0.000)	(0.000)	(0.000)
45-54 years old	–0.034	–0.056	–0.039
	(0.000)	(0.000)	(0.000)
55-64 years old	–0.029	–0.049	–0.034
	(0.000)	(0.000)	(0.000)
65 years old or older	–0.026	–0.048	–0.024
	(0.000)	(0.000)	(0.000)
Type of Contract			
Contract (=1 if Permanent)	–0.020	–0.035	–0.025
	(0.000)	(0.000)	(0.000)
Education			
Educ2	–0.005	–0.008	–0.005
	(0.073)	(0.182)	(0.363)
Educ3	–0.010	–0.017	–0.012
	(0.001)	(0.011)	(0.066)
Educ4	–0.011	–0.022	–0.015
	(0.000)	(0.002)	(0.025)
Educ5	–0.018	–0.032	–0.023
	(0.000)	(0.000)	(0.000)
Industry			
Extractive and Manufactoring	0.017	0.022	0.019
	(0.000)	(0.000)	(0.000)
Electricity and Construction	0.028	0.038	0.042
	(0.000)	(0.000)	(0.000)
Services	0.013	0.020	0.013
	(0.000)	(0.000)	(0.003)
Public Administration	0.013	0.018	0.019
	(0.000)	(0.015)	(0.002)
Region			
Azores	–0.012	–0.018	–0.016
	(0.000)	(0.001)	(0.002)
Alentejo	0.001	0.001	0.001
	(0.706)	(0.764)	(0.811)
Algarve	–0.001	0.002	0.002
	(0.767)	(0.681)	(0.707)
Center	–0.011	–0.016	–0.016
	(0.000)	(0.001)	(0.000)
Madeira	–0.007	–0.009	–0.009
	(0.001)	(0.080)	(0.072)
North	–0.005	–0.007	–0.007
	(0.002)	(0.099)	(0.093)
Observations	183866	183866	183866
Adjust. R^2	0.105	0.196	
F-Statistic	149.940	383.584	
Chi2-Statistic			14028.030
Prob > chi2	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. Reference groups: 15-24 years old; Educ1; Agriculture; Lisboa.

Table 5.7: Longitudinal LPM for transitions out of employment (*Cont.*)

	(1) POLS	(2) Between	(3) RE
Interactions with Demographics			
Male×Design	–0.018	–0.031	–0.021
	(0.000)	(0.000)	(0.000)
Married×Design	–0.004	–0.007	–0.003
	(0.130)	(0.090)	(0.393)
Interactions with Age Cohorts			
25-34 years old×Design	–0.043	–0.063	–0.058
	(0.000)	(0.000)	(0.000)
35-44 years old×Design	–0.052	–0.074	–0.073
	(0.000)	(0.000)	(0.000)
45-54 years old×Design	–0.052	–0.073	–0.072
	(0.000)	(0.000)	(0.000)
55-54 years old×Design	–0.023	–0.027	–0.028
	(0.001)	(0.003)	(0.001)
65 years old or older×Design	0.058	0.068	0.080
	(0.000)	(0.000)	(0.000)
Interactions with Type of Contract			
Contract×Design	–0.066	–0.101	–0.058
	(0.000)	(0.000)	(0.000)
Interactions with Education			
Educ2×Design	–0.028	–0.021	–0.031
	(0.000)	(0.015)	(0.000)
Educ3×Design	–0.028	–0.020	–0.037
	(0.001)	(0.036)	(0.000)
Educ4×Design	–0.040	–0.038	–0.050
	(0.000)	(0.000)	(0.000)
Educ5×Design	–0.047	–0.052	–0.063
	(0.000)	(0.000)	(0.000)
Interactions with Region			
Azores×Design	–0.009	–0.010	–0.009
	(0.034)	(0.189)	(0.238)
Alentejo×Design	–0.004	–0.004	–0.001
	(0.340)	(0.576)	(0.885)
Algarve×Design	0.005	0.008	0.012
	(0.278)	(0.235)	(0.086)
Center×Design	0.002	0.007	0.011
	(0.592)	(0.267)	(0.085)
Madeira×Design	–0.002	0.004	0.004
	(0.620)	(0.621)	(0.608)
North×Design	–0.002	–0.002	0.000
	(0.640)	(0.670)	(0.931)
Interactions with Industry			
Extractive and Manufactoring×Design	–0.119	–0.171	–0.146
	(0.000)	(0.000)	(0.000)
Electricity and Construction×Design	–0.080	–0.114	–0.113
	(0.000)	(0.000)	(0.000)
Services×Design	–0.115	–0.166	–0.138
	(0.000)	(0.000)	(0.000)
Public Administration×Design	–0.112	–0.159	–0.147
	(0.000)	(0.000)	(0.000)
Observations	183866	183866	183866
Adjust. R^2	0.105	0.196	
F-Statistic	149.940	383.584	
Chi2-Statistic			14028.030
Prob > chi2	0.000	0.000	0.000

6 Conclusions

This paper provides a summary of a wide range of information about the Portuguese labor market flows for the period between 1999 and 2012. However, its main contribution is to document the construction, the business cycle characteristics and the impact of the LFS redesign on labor market flows. The data computed for this paper include the worker flows in the usual three-state set-up, the flows disaggregated by micro-characteristics of the respondents and the flows in a four state set-up, where employment is disaggregated between permanent and temporary, and inactivity is disaggregated between those who *want a job* and those who *do not want a job.* Furthermore, we investigate the micro determinants of labor market mobility and the redesign's effect on the probability to move out of employment, using cross-sectional and longitudinal methods.

The facts set out in this paper might prove useful to researchers working in several spheres of the Portuguese labor market. The main conclusions are:

1. In the first quarter of 2011 the LFS was redesigned, thereafter it is not sufficient to report a paid or non-paid work or an absence from work to be classified as employed. The nature of the non-paid work, the reasons for the absence and the expectation of how long the absence will last are determinant to the respondent's classification as employed or non-employed. In turn, the distinction between unemployed and inactive is not merely based on the respondent assessment of his active job search. Now, the definition of unemployment incorporates the job search diligences made by the respondent. If those meet the criteria to be considered *active*, the respondent is classified as unemployed, otherwise (s)he joins the pool of inactive. Hence, transitions in and out of employment, unemployment and inactivity might show-up in the revised LFS, while before the redesign none would actually occur.

2. State dependence greatly accounts for the probability to find a job or return to the labor force. The redesign increased the returning probabilities of those who came from inactivity, but regardless of the survey mode, passing through non-employment is still particularly damaging for the Portuguese workers: the job-finding rate of the unemployed that have been inactive or unemployed two quarters earlier is roughly two times lower than in the UK.

3. The redesign had an effect of 1.175 p.p. increase in flows from U to E (with the following change in the average flows: 1.139 percent *before* vs. 2.464 percent *after*) and of 0.901 p.p. increase in flows from E to U (1.768 percent vs. 3.385 percent). However, the effects of the redesign concentrate in the older respondents (45 to 64 years old) moving between temporary employment and unemployment. The flows from E to U have a countercyclical behavior mostly driven by employment separations of the young (25-34 and 35-44 years old) and less educated individuals (Educ2 and Educ3).

4. The redesign had an effect of 3.867 p.p. increase in flows from E to I (1.222 percent vs. 4.124 percent) and of 3.654 p.p. increase in flows from I to E (1.178 percent vs. 3.841 percent). However, the effects of the redesign concentrate in the older and less educated respondents moving between temporary employment and inactivity. The flows from the inactives who *want a job* into employment are procyclical, which suggests that in Portugal recessions mostly affect the job finding rate of the marginally attached, since the cyclical coefficient of the flows from U to E also indicates a procyclical pattern, yet not statistically significant.

5. The redesign had an effect of 0.539 p.p. increase in flows from U to I (1.081 percent vs. 2.048) and of 0.996 p.p. increase in flows from I to U (1.157 percent vs. 2.401percent). However, the effects of the redesign concentrate in flows between the unemployed and the inactives who

want a job. The countercyclicality of flows between U and I is driven by flows of individuals who are less than 44 years old: (i) moving from unemployment into a limbo between inactivity and the labor force; and (ii) moving from both groups of inactivity into unemployment.

6. Every quarter, an average of 1.630 percent (after the redesign: 6.872 percent) of the employed with a fixed term contract changed to an open-ended one, while 0.731 percent (after the redesign: 3.842 percent) did the reverse movement. Moreover, the average percentage of permanent employment in total employment changed from 58.8 percent, when measured under the old method, to 61.8 percent under the revised LFS. However, significant redesign effects uncover these differences: the redesign had an effect of 2.626 p.p. increase in flows from permanent employment to temporary employment and of 2.720 p.p. in the reverse movement. The flows from permanent employment to temporary employment are found to be countercyclical, which indicates that in dual labor markets recessions are periods where permanent employment is somehow converted into temporary.

7. Every quarter, an average of 18.927 percent (after the redesign: 23.529 percent) of the inactives who *want a job* abandon their intentions by the following quarter, while an average of 0.513 percent (after the redesign: 3.793 percent) of the inactives who *do not want a job* become available to work in the following quarter. Moreover, the average percentage of inactives who *want a job* changed from 2.6 percent, when measured under the old method, to 11.2 percent under the revised LFS. These differences are probably a result of the major definitional changes that broaden the spectrum of inactivity. In fact, the redesign had a statistically significant effect of 0.503 p.p. increase in flows from the inactives who *want a job* to the inactives who *do not want a job*, and 1.372 p.p. increase in the reverse movement.

8. It is the redesign that determines the jump of the flows where the redesign and the business cycle are found simultaneously significant. Indeed, the reaction of the flows to recessions is not significantly different after the redesign, and do not justify their structural break since then. Moreover, Centeno and Novo (2013) estimate labor market flows using social security data and find no break in the employment outflows series. It is also found that during recessions the reaction of the Portuguese labor market is mostly driven by the reduction of hirings, which strengthens the view that the jump is mostly due to the redesign.

9. The proportion of transitions out of employment changed from 2.2 percent before the redesign (2009:2 to 2010:4) to 8.9 percent after the redesign (2011:2 to 2012:4). Evidence suggests that the new methodology increased the probability to record transitions out of employment: everything held fixed, an individual interviewed under the revised LFS is about one third more likely to exit employment than an individual interviewed under the old method (this value changes to 16.1 p.p. when estimated through Probit in the cross-sectional dataset). The effects of the remain covariates are broadly consistent with the conventional wisdom on labor market mobility determinants, nevertheless some of these effects are significantly different when estimated over the previous or the revised LFS samples.

10. We compute the out-of-sample transitions using the coefficients after the redesign given the sample prior to the redesign. The average out-of-sample predicted transitions come very close to the sample averages after the redesign. Hence, the composition of the sample of the revised LFS seems to have not positively influenced the likelihood to observe a higher level of exit transitions and thus, the differences in the proportion of exits seem to be due to a redesign effect caused by the major changes brought by the new method, in particular the definitional ones.

Appendix

A A note on transition probabilities

This section presents the expressions used to compute the transition probabilities or hazard rates throughout this paper. These were built upon the methodology of Shimer (2012).

Unconditional transition probabilities

$$\lambda_t^{ij} = \frac{N_t^{ij}}{\sum_k N_t^{ik}} \text{ where } i,j,k \in \{E,U,I\} \text{ for } i \neq j \text{ and } i \neq k$$

$$\lambda_t^{EU} = \frac{N_t^{EU}}{N_t^{EE}+N_t^{EU}+N_t^{EI}},\ \lambda_t^{EI} = \frac{N_t^{EI}}{N_t^{EE}+N_t^{EU}+N_t^{EI}},\ \lambda_t^{EE} = \frac{N_t^{EE}}{N_t^{EE}+N_t^{EU}+N_t^{EI}}$$

$$\lambda_t^{UE} = \frac{N_t^{UE}}{N_t^{UE}+N_t^{UI}+N_t^{UU}},\ \lambda_t^{UI} = \frac{N_t^{UI}}{N_t^{UE}+N_t^{UI}+N_t^{UU}},\ \lambda_t^{UU} = \frac{N_t^{UU}}{N_t^{UE}+N_t^{UI}+N_t^{UU}}$$

$$\lambda_t^{IE} = \frac{N_t^{IE}}{N_t^{IE}+N_t^{IU}+N_t^{II}},\ \lambda_t^{IU} = \frac{N_t^{IU}}{N_t^{IE}+N_t^{IU}+N_t^{II}},\ \lambda_t^{II} = \frac{N_t^{II}}{N_t^{IE}+N_t^{IU}+N_t^{II}}$$

Conditional transition probabilities

$$\lambda_{t|X_{t-2}}^{ij} = \frac{N_t^{ij}|X_{t-2}}{\sum_k N_t^{ik}|X_{t-2}} \text{ with } X \in \{E,U,I\}$$

Thus for each $X \in \{E,U,I\}$ we have:

$$\lambda_{t|X_{t-2}}^{EU} = \frac{N_t^{EU}|X_{t-2}}{(N_t^{EU}|X_{t-2})+(N_t^{EI}|X_{t-2})+(N_t^{EE}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{EI} = \frac{N_t^{EI}|X_{t-2}}{(N_t^{EU}|X_{t-2})+(N_t^{EI}|X_{t-2})+(N_t^{EE}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{EE} = \frac{N_t^{EU}|X_{t-2}}{(N_t^{EU}|X_{t-2})+(N_t^{EI}|X_{t-2})+(N_t^{EE}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{UE} = \frac{N_t^{UE}|X_{t-2}}{(N_t^{UE}|X_{t-2})+(N_t^{UI}|X_{t-2})+(N_t^{UU}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{UI} = \frac{N_t^{UI}|X_{t-2}}{(N_t^{UE}|X_{t-2})+(N_t^{UI}|X_{t-2})+(N_t^{UU}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{UU} = \frac{N_t^{UU}|X_{t-2}}{(N_t^{UE}|X_{t-2})+(N_t^{UI}|X_{t-2})+(N_t^{UU}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{IE} = \frac{N_t^{IE}|X_{t-2}}{(N_t^{IE}|X_{t-2})+(N_t^{IU}|X_{t-2})+(N_t^{II}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{IU} = \frac{N_t^{IU}|X_{t-2}}{(N_t^{IE}|X_{t-2})+(N_t^{IU}|X_{t-2})+(N_t^{II}|X_{t-2})}$$

$$\lambda_{t|X_{t-2}}^{II} = \frac{N_t^{II}|X_{t-2}}{(N_t^{IE}|X_{t-2})+(N_t^{IU}|X_{t-2})+(N_t^{II}|X_{t-2})}$$

B A note on survey data analysis

Survey data are characterized by unequal probabilities of selection of the sample elements due to the multistage sample stratification procedure from which the data is collected. Thus, in order to obtain correct point estimates and standard errors, one has to include the survey variables (sampling weights, stratums, primary sampling units and clusters) in the estimation procedures, whenever they are available. That is what was done throughout this paper, namely to compute the proportions of population characteristics between samples (section 3.2) and to estimate the Probit and the Linear Probability Model (LPM) in the cross-sectional approach (section 5). Our aim in this section is to present the methods adopted to deal with the specific nature of the survey data. The following description of the survey methods draws heavily on Pfeffermann (1993) and Heeringa et al. (2010).

Descriptive Analysis

The estimation of the mean for a continuous variable with survey data is given by the following non-linear ratio of two estimated finite population totals:

$$\hat{\mu} = \frac{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i} y_i}{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i}}, \tag{B.1}$$

where h is a stratum index, α is the primary sampling unit index and i is an index for the elements within the α-th cluster.

The estimation of a proportion is a straightforward extension of the ratio estimator for the mean of a continuous variable. By recoding the original response categories to a single indicator variable, the ratio mean estimator counts the prevalence of "1s", such that:

$$\hat{p} = \frac{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i}\, \mathcal{I}(y_i = 1)}{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i}}, \tag{B.2}$$

where the survey variables have the same meaning as in eq. (B.1). This was the expression that we used to estimate the proportions of the population characteristics between the samples of the previous and the revised LFS, analyzed in section 3.2 and presented in Tables C.1, C.2 and C.3.

Linear Regression Estimation

The specific nature of survey data can be incorporated into the estimation of linear regression parameters through the use of the weighted least squares estimator (WLS), where the contribution of each sample element to the residual sum of squares is made proportional to its population weight, which gives us to the following analytic formula for weighted survey estimator:

$$\hat{\boldsymbol{\beta}}^W = (X'WX)^{-1}X'W\boldsymbol{y}, \tag{B.3}$$

where W is an $n \times n$ matrix with zeros off the diagonal and n values of the sampling weights on the diagonal. This is the estimator used to estimate eq. (5.1) by LPM in section 5.1, whose estimates are presented in Tables 5.3 and 5.4.

The unequal selection probabilities of sample elements in survey data impedes the use of conventional variance estimators, instead, methods based on the Taylor series linearization or replication variance methods are employed. In the context of the present study we used the standard Taylor series linearization approach. We will present the Taylor series linearization variance estimation in the context of the simple linear regression model. The weighted survey estimator of $\hat{\beta}$ in the simple regression model is given by:

$$\hat{\beta} = \frac{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i} y_{h\alpha i} x_{h\alpha i}}{\sum_{h=1}^{H}\sum_{\alpha=1}^{a_h}\sum_{i=1}^{n_{h\alpha}} w_{h\alpha i} x_{h\alpha i}^2} = \frac{A}{B}. \tag{B.4}$$

Applying the Taylor series linearization approximation method, an estimate of the sampling variance can be obtained as follows:

$$Var(\hat{\beta}) = \frac{Var(A) + \hat{\beta}^2 Var(B) - 2 \cdot \hat{\beta} \cdot Cov(A,B)}{B^2}. \tag{B.5}$$

The extension of the Taylor series method to multiple linear regression model is a direct extension of the technique for the simple linear regression model. However, it is more algebraically complicated and thus, we will not reproduce it here, but it can be found in Heeringa et al. (2010).

Generalized Linear Models for Binary Survey Variables

The Probit estimates presented in section 5.1 (Tables 5.1 and 5.2) are obtained by maximizing the following weighted pseudo-likelihood function:

$$\mathcal{PL}(\boldsymbol{\beta}|\boldsymbol{x}_i) = \prod_{i=1}^{N} \left\{ G(\boldsymbol{x}_i\boldsymbol{\beta})^{y_i} [1 - G(\boldsymbol{x}_i\boldsymbol{\beta})]^{1-y_i} \right\}^{w_i}, \tag{B.6}$$

where $G(.)$ is the standard normal cdf. The sampling variances and covariances of the parameters were computed using a multivariate version of the Taylor series linearization. The result is a sandwich-type variance estimator based on a weighted score function:

$$Var(\hat{\boldsymbol{\beta}}) = (\boldsymbol{J}^{-1}) Var[(S(\hat{\boldsymbol{\beta}})](\boldsymbol{J}^{-1}), \tag{B.7}$$

where $\boldsymbol{J}$ is a matrix of second derivatives with respect to the $\hat{\beta}_j$ of the pseudo-likelihood function and $Var[(S(\hat{\boldsymbol{\beta}})]$ is the variance-covariance matrix of the weighted score function. The variance estimator of expression (B.7) is the most widely used variance estimator in applied survey data analysis and it is also the one we use to compute the standard errors presented in Tables 5.1. and 5.2. We will not reproduce here the weighted score function, but it can be found in Heeringa et al. (2010).

C Estimation Results

Table C.1: Means by population characteristics

	Before	After	*t* - test	
	(1)	(2)	(2)−(1)	$p - value$
Labor Force Status				
Employed	0.465	0.425	-0.040	0.000
Unemployed	0.055	0.084	0.029	0.000
Inactive	0.480	0.491	0.011	0.033
Demographics				
Male	0.484	0.486	0.002	0.732
Married/living as married	0.542	0.476	-0.066	0.000
Age				
< 15	0.144	0.145	0.002	0.675
15-24 years old	0.109	0.104	-0.005	0.116
25-34 years old	0.142	0.130	-0.011	0.005
35-44 years old	0.150	0.156	0.006	0.118
45-54 years old	0.143	0.144	0.001	0.723
55-64 years old	0.125	0.126	0.001	0.715
65 years or older	0.188	0.194	0.006	0.088
Region				
Azores	0.025	0.023	-0.002	0.001
Alentejo	0.074	0.071	-0.004	0.038
Algarve	0.042	0.041	-0.001	0.230
Center	0.220	0.228	0.008	0.082
Lisboa	0.252	0.256	0.003	0.480
Madeira	0.025	0.024	-0.001	0.161
North	0.361	0.359	-0.003	0.577
Education				
Educ1	0.146	0.087	-0.060	0.000
Educ2	0.411	0.314	-0.097	0.000
Educ3	0.166	0.174	0.009	0.021
Educ4	0.134	0.152	0.018	0.000
Educ5	0.098	0.128	0.030	0.000
Observations	32119	30209	–	–

Source: Author's calculations based on LFS.
Note: Here *Before* indicates the fourth quarter of 2010 and *After* the fourth quarter of 2012. Means computed considering the sampling weights and stratums.

Table C.2: Means by industry of the employed

	Before	After	*t* - test	
	(1)	(2)	(2)−(1)	$p-value$
Agriculture	0.053	0.046	-0.007	0.000
Extractive	0.002	0.001	-0.001	0.092
Manufacturing	0.077	0.070	-0.006	0.025
Electricity	0.001	0.001	-0.000	0.990
Construction	0.046	0.029	-0.017	0.000
Services	0.794	0.825	0.032	0.000
Public Administration	0.028	0.027	-0.001	0.538
Observations	32119	30209	–	–

Source: Author's calculations based on LFS.
Note: Here *Before* indicates the fourth quarter of 2010 and *After* the fourth quarter of 2012. Means computed considering the sampling weights and stratums.

Table C.3: Means by occupation of the non-employed

	Before	After	*t* - test	
	(1)	(2)	(2)−(1)	$p-value$
Agriculture	0.039	0.015	-0.024	0.000
Extractive	0.001	0.000	-0.001	0.015
Manufacturing	0.071	0.035	-0.035	0.000
Electricity	0.002	0.001	-0.001	0.000
Construction	0.046	0.023	-0.001	0.358
Services	0.848	0.916	0.068	0.000
Public Administration	0.016	0.010	-0.006	0.000
Observations	32119	30209	–	–

Source: Author's calculations based on LFS.
Note: Here *Before* indicates the fourth quarter of 2010 and *After* the fourth quarter of 2012. Means computed considering the sampling weights and stratums.

Table C.4: RD estimates: Employment outflows

	E→U		E→I	
	$p=1$	$p=2$	$p=1$	$p=2$
Design Effects				
Design dummy	0.843 (0.000)	0.882 (0.000)	3.653 (0.000)	3.860 (0.000)
Polynomials				
$\widetilde{\text{Time}}$	0.014 (0.000)	0.006 (0.397)	-0.013 (0.000)	-0.020 (0.084)
$\widetilde{\text{Time}}^2$	–	-0.000 (0.295)	–	-0.000 (0.508)
Interactions				
Design$\times\widetilde{\text{Time}}$	0.120 (0.000)	0.150 (0.117)	-0.101 (0.014)	-0.241 (0.101)
Design$\times\widetilde{\text{Time}}^2$	–	-0.003 (0.815)	–	0.021 (0.287)
Observations	55	55	55	55
Adjust. R^2	0.934	0.933	0.946	0.945
F-Statistic	254.410	150.433	315.699	188.204
Prob > F	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point and "p" denotes the polynomial order.

Table C.5: RD estimates: Unemployment outflows

	U→E		U→I	
	$p=1$	$p=2$	$p=1$	$p=2$
Design Effects				
Design dummy	1.174 (0.000)	1.075 (0.000)	0.528 (0.000)	0.340 (0.001)
Polynomials				
$\widetilde{\text{Time}}$	0.013 (0.032)	0.012 (0.000)	0.009 (0.142)	0.012 (0.000)
$\widetilde{\text{Time}}^2$	0.000 (0.881)	–	-0.000 (0.602)	–
Interactions				
Design$\times\widetilde{\text{Time}}$	-0.125 (0.109)	-0.018 (0.408)	-0.082 (0.273)	0.079 (0.000)
Design$\times\widetilde{\text{Time}}^2$	0.015 (0.154)	–	0.023 (0.025)	–
Observations	55	55	55	55
Adjust. R^2	0.932	0.932	0.889	0.881
F-Statistic	150.154	248.932	87.686	134.721
Prob > F	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point and "p" denotes the polynomial order.

Table C.6: RD estimates: Inactivity outflows

	I→E		I→U	
	$p=1$	$p=2$	$p=1$	$p=2$
Design Effects				
Design dummy	3.716 (0.000)	3.655 (0.000)	0.802 (0.000)	0.976 (0.000)
Polynomials				
$\widetilde{\text{Time}}$	-0.018 (0.000)	-0.006 (0.517)	0.010 (0.000)	-0.011 (0.081)
$\widetilde{\text{Time}}^2$	–	0.000 (0.149)	–	-0.000 (0.001)
Interactions				
Design$\times\widetilde{\text{Time}}$	-0.128 (0.000)	-0.183 (0.115)	0.039 (0.115)	0.060 (0.459)
Design$\times\widetilde{\text{Time}}^2$	–	0.006 (0.716)	–	0.000 (0.965)
Observations	55	55	55	55
Adjust. R^2	0.962	0.962	0.895	0.913
F-Statistic	452.600	273.621	154.325	114.224
Prob > F	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point and "p" denotes the polynomial order.

Table C.7: RD estimates: Employment outflows, controlling for business cycle effects

	E→U		E→I	
	$p=1$	$p=2$	$p=1$	$p=2$
Design Effects				
Design dummy	0.900 (0.000)	0.901 (0.000)	3.678 (0.000)	3.867 (0.000)
Business Cycle Effects				
GDP cyclical component	-5.364 (0.006)	-5.342 (0.007)	-2.374 (0.447)	-1.894 (0.550)
Polynomials				
$\widetilde{\text{Time}}$	0.014 (0.000)	0.008 (0.228)	-0.012 (0.000)	-0.019 (0.100)
$\widetilde{\text{Time}}^2$	–	-0.000 (0.370)	–	-0.000 (0.542)
Interactions				
Design$\times\widetilde{\text{Time}}$	0.091 (0.001)	0.145 (0.106)	-0.114 (0.011)	-0.243 (0.101)
Design$\times\widetilde{\text{Time}}^2$	–	-0.007 (0.585)	–	0.020 (0.323)
Observations	55	55	55	55
Adjust. R^2	0.942	0.941	0.945	0.945
F-Statistic	219.951	144.308	235.004	154.856
Prob > F	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point and "p" denotes the polynomial order.

Table C.8: RD estimates: Unemployment outflows, controlling for business cycle effects

	U→E		U→I	
	$p = 1$	$p = 2$	$p = 1$	$p = 2$
Design Effects				
Design dummy	1.080 (0.000)	1.175 (0.000)	0.378 (0.000)	0.539 (0.000)
Business Cycle Effects				
GDP cyclical component	-0.456 (0.784)	-0.227 (0.892)	-3.575 (0.028)	-3.177 (0.046)
Polynomials				
$\widetilde{\text{Time}}$	0.012 (0.000)	0.013 (0.034)	0.012 (0.000)	0.010 (0.085)
$\widetilde{\text{Time}}^2$	–	0.000 (0.874)	–	-0.000 (0.707)
Interactions				
Design$\times\widetilde{\text{Time}}$	-0.020 (0.389)	-0.125 (0.112)	0.059 (0.009)	-0.085 (0.241)
Design$\times\widetilde{\text{Time}}^2$	–	0.015 (0.165)	–	0.021 (0.036)
Observations	55	55	55	55
Adjust. R^2	0.931	0.931	0.890	0.896
F-Statistic	183.334	122.625	110.499	78.541
Prob > F	0.000	0.000	0.000	0.000

Table C.9: RD estimates: Inactivity outflows, controlling for business cycle effects

	I→E		I→U	
	$p = 1$	$p = 2$	$p = 1$	$p = 2$
Design Effects				
Design dummy	3.713 (0.000)	3.654 (0.000)	0.863 (0.000)	0.996 (0.000)
Business Cycle Effects				
GDP cyclical component	0.292 (0.907)	0.111 (0.965)	-5.733 (0.002)	-5.349 (0.001)
Polynomials				
$\widetilde{\text{Time}}$	-0.018 (0.000)	-0.006 (0.521)	0.011 (0.000)	-0.009 (0.123)
$\widetilde{\text{Time}}^2$	–	0.000 (0.155)	–	-0.000 (0.001)
Interactions				
Design$\times\widetilde{\text{Time}}$	-0.127 (0.001)	-0.183 (0.119)	0.008 (0.750)	0.054 (0.454)
Design$\times\widetilde{\text{Time}}^2$	–	0.006 (0.717)	–	-0.003 (0.752)
Observations	55	55	55	55
Adjust. R^2	0.961	0.961	0.912	0.929
F-Statistic	332.890	223.373	141.571	117.906
Prob > F	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point and "p" denotes the polynomial order.

Table C.10: RD estimates: Employment outflows by education

	E→U					E→I				
	Educ1	Educ2	Educ3	Educ4	Educ5	Educ1	Educ2	Educ3	Educ4	Educ5
Design Effects										
Design dummy	0.002	0.131	0.183	0.079	0.155	0.404	1.166	0.301	0.211	0.158
	(0.866)	(0.083)	(0.004)	(0.055)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Business Cycle Effects										
GDP cyclical component	-0.164	-2.962	-1.388	-0.545	-0.527	-0.010	-2.637	-0.385	0.123	0.093
	(0.377)	(0.003)	(0.081)	(0.293)	(0.336)	(0.992)	(0.283)	(0.595)	(0.813)	(0.832)
Polynomials										
$\widetilde{\text{Time}}$	0.000	-0.003	0.005	0.006	-0.000	-0.005	-0.013	0.000	-0.001	-0.001
	(0.738)	(0.403)	(0.111)	(0.001)	(0.894)	(0.192)	(0.134)	(0.985)	(0.783)	(0.747)
$\widetilde{\text{Time}}^2$	0.000	-0.000	0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000	-0.000
	(0.479)	(0.051)	(0.959)	(0.211)	(0.209)	(0.934)	(0.455)	(0.650)	(0.807)	(0.403)
Interactions										
Design$\times\widetilde{\text{Time}}$	0.028	0.142	0.069	0.102	0.013	0.164	0.376	0.000	0.025	0.019
	(0.002)	(0.002)	(0.062)	(0.000)	(0.595)	(0.001)	(0.002)	(0.998)	(0.302)	(0.337)
Design$\times\widetilde{\text{Time}}^2$	-0.004	-0.014	-0.006	-0.011	0.002	-0.020	-0.044	0.002	-0.003	-0.002
	(0.002)	(0.026)	(0.219)	(0.001)	(0.504)	(0.003)	(0.006)	(0.718)	(0.310)	(0.425)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.411	0.828	0.877	0.926	0.847	0.836	0.877	0.784	0.788	0.787
F-Statistic	7.279	44.265	65.083	114.087	50.879	46.806	65.037	33.595	34.369	34.342
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

Table C.11: RD estimates: Unemployment outflows by education

	U→E					U→I				
	Educ1	Educ2	Educ3	Educ4	Educ5	Educ1	Educ2	Educ3	Educ4	Educ5
Design Effects										
Design dummy	0.050	0.161	0.194	0.108	0.124	0.031	0.071	0.151	0.128	0.026
	(0.017)	(0.026)	(0.003)	(0.005)	(0.008)	(0.095)	(0.282)	(0.006)	(0.002)	(0.408)
Business Cycle Effects										
GDP cyclical component	0.049	-0.173	0.072	-0.399	0.210	-0.268	-1.140	-0.675	-1.002	-0.136
	(0.851)	(0.848)	(0.927)	(0.397)	(0.713)	(0.249)	(0.176)	(0.314)	(0.054)	(0.736)
Polynomials										
$\widetilde{\text{Time}}$	-0.000	-0.000	0.000	0.012	0.002	0.001	0.001	0.006	0.002	0.000
	(0.987)	(0.976)	(0.912)	(0.000)	(0.365)	(0.378)	(0.789)	(0.021)	(0.323)	(0.924)
$\widetilde{\text{Time}}^2$	0.000	-0.000	-0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000	-0.000
	(0.719)	(0.725)	(0.190)	(0.000)	(0.456)	(0.269)	(0.344)	(0.459)	(0.650)	(0.315)
Interactions										
Design$\times\widetilde{\text{Time}}$	-0.012	0.076	0.033	0.062	-0.002	-0.003	-0.006	-0.018	-0.012	0.042
	(0.324)	(0.072)	(0.369)	(0.006)	(0.943)	(0.753)	(0.873)	(0.558)	(0.623)	(0.027)
Design$\times\widetilde{\text{Time}}^2$	0.001	-0.010	-0.004	-0.008	0.004	0.000	0.003	0.005	0.006	-0.004
	(0.370)	(0.078)	(0.450)	(0.012)	(0.284)	(0.834)	(0.559)	(0.206)	(0.082)	(0.124)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.157	0.617	0.775	0.942	0.832	0.244	0.561	0.841	0.857	0.714
F-Statistic	2.677	15.500	32.014	147.208	45.617	3.900	12.487	48.708	54.735	23.489
Prob > F	0.025	0.000	0.000	0.000	0.000	0.003	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

Table C.12: RD estimates: Inactivity outflows by education

	I→E					I→U				
	Educ1	Educ2	Educ3	Educ4	Educ5	Educ1	Educ2	Educ3	Educ4	Educ5
Design Effects										
Design dummy	0.456	1.186	0.137	0.113	0.068	0.020	0.180	0.212	0.084	0.141
	(0.000)	(0.000)	(0.011)	(0.008)	(0.035)	(0.154)	(0.021)	(0.000)	(0.111)	(0.000)
Business Cycle Effects										
GDP cyclical component	0.268	-1.250	0.765	-0.343	0.318	-0.367	-2.384	-1.361	-1.475	-0.154
	(0.744)	(0.498)	(0.252)	(0.514)	(0.432)	(0.042)	(0.016)	(0.029)	(0.029)	(0.747)
Polynomials										
$\widetilde{\text{Time}}$	-0.003	-0.006	0.003	0.002	-0.003	0.000	-0.010	0.003	0.001	-0.002
	(0.390)	(0.366)	(0.222)	(0.208)	(0.048)	(0.732)	(0.005)	(0.199)	(0.594)	(0.212)
$\widetilde{\text{Time}}^2$	0.000	0.000	0.000	0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.000
	(0.825)	(0.599)	(0.005)	(0.032)	(0.071)	(0.724)	(0.001)	(0.675)	(0.328)	(0.009)
Interactions										
Design$\times\widetilde{\text{Time}}$	0.134	0.405	0.070	0.021	0.081	0.018	0.084	0.048	0.075	0.008
	(0.001)	(0.000)	(0.026)	(0.391)	(0.000)	(0.032)	(0.065)	(0.095)	(0.017)	(0.704)
Design$\times\widetilde{\text{Time}}^2$	-0.020	-0.054	-0.009	-0.002	-0.010	-0.003	-0.007	-0.005	-0.006	-0.000
	(0.000)	(0.000)	(0.038)	(0.459)	(0.000)	(0.006)	(0.226)	(0.173)	(0.139)	(0.930)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.884	0.928	0.728	0.642	0.714	0.417	0.624	0.896	0.843	0.763
F-Statistic	69.377	117.148	25.059	17.108	23.503	7.448	15.951	78.312	49.334	29.920
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

Table C.13: RD estimates: Employment outflows by age cohort

	E→U					E→I				
	15-24	25-34	35-44	45-54	55-64	15-24	25-34	35-44	45-54	55-64
Design Effects										
Design dummy	0.066	0.122	0.077	0.177	0.067	0.235	0.199	0.144	0.269	0.563
	(0.127)	(0.131)	(0.095)	(0.000)	(0.003)	(0.000)	(0.000)	(0.008)	(0.000)	(0.000)
Business Cycle Effects										
GDP cyclical component	-0.576	-1.947	-1.594	-0.848	-0.489	1.179	-1.041	0.450	-0.614	-1.616
	(0.291)	(0.061)	(0.008)	(0.130)	(0.082)	(0.102)	(0.119)	(0.499)	(0.399)	(0.217)
Polynomials										
$\widetilde{\text{Time}}$	-0.005	-0.001	0.006	0.007	0.001	-0.002	-0.001	-0.002	-0.003	-0.006
	(0.021)	(0.767)	(0.006)	(0.001)	(0.272)	(0.477)	(0.674)	(0.326)	(0.207)	(0.196)
$\widetilde{\text{Time}}^2$	-0.000	-0.000	0.000	0.000	-0.000	0.000	0.000	-0.000	-0.000	-0.000
	(0.013)	(0.050)	(0.403)	(0.036)	(0.950)	(0.165)	(0.716)	(0.726)	(0.418)	(0.265)
Interactions										
Design$\times\widetilde{\text{Time}}$	0.059	0.117	0.088	0.067	0.030	-0.049	-0.048	0.063	0.092	0.176
	(0.021)	(0.016)	(0.002)	(0.011)	(0.025)	(0.140)	(0.117)	(0.044)	(0.008)	(0.005)
Design$\times\widetilde{\text{Time}}^2$	-0.007	-0.007	-0.009	-0.007	-0.003	0.006	0.006	-0.008	-0.012	-0.021
	(0.041)	(0.290)	(0.023)	(0.048)	(0.153)	(0.158)	(0.139)	(0.069)	(0.015)	(0.012)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.429	0.859	0.910	0.922	0.876	0.598	0.407	0.584	0.805	0.858
F-Statistic	7.773	55.757	92.263	106.675	64.607	14.382	7.168	13.614	38.092	55.387
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

Table C.14: RD estimates: Unemployment outflows by age cohort

	U→E					U→I				
	15-24	25-34	35-44	45-54	55-64	15-24	25-34	35-44	45-54	55-64
Design Effects										
Design dummy	0.080	0.161	0.157	0.180	0.059	0.110	0.045	0.097	0.060	0.059
	(0.192)	(0.006)	(0.004)	(0.000)	(0.001)	(0.015)	(0.430)	(0.021)	(0.095)	(0.130)
Business Cycle Effects										
GDP cyclical component	-0.453	-0.107	-0.304	0.406	-0.038	-1.152	-0.915	-0.942	-0.732	0.422
	(0.559)	(0.881)	(0.650)	(0.403)	(0.859)	(0.044)	(0.214)	(0.077)	(0.111)	(0.390)
Polynomials										
$\widetilde{\text{Time}}$	-0.002	0.002	0.009	0.005	0.000	-0.004	0.001	0.003	0.004	0.004
	(0.559)	(0.507)	(0.001)	(0.008)	(0.633)	(0.046)	(0.595)	(0.076)	(0.009)	(0.033)
$\widetilde{\text{Time}}^2$	-0.000	-0.000	0.000	0.000	-0.000	-0.000	-0.000	0.000	0.000	0.000
	(0.478)	(0.094)	(0.041)	(0.069)	(0.926)	(0.036)	(0.743)	(0.593)	(0.399)	(0.972)
Interactions										
Design$\times\widetilde{\text{Time}}$	-0.005	0.090	-0.043	0.038	0.068	0.014	0.008	-0.021	0.011	-0.009
	(0.885)	(0.008)	(0.170)	(0.092)	(0.000)	(0.577)	(0.808)	(0.378)	(0.597)	(0.696)
Design$\times\widetilde{\text{Time}}^2$	0.002	-0.009	0.007	-0.005	-0.010	0.006	0.000	0.003	-0.001	0.002
	(0.724)	(0.042)	(0.120)	(0.137)	(0.000)	(0.096)	(0.956)	(0.311)	(0.743)	(0.549)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.109	0.896	0.816	0.894	0.896	0.815	0.454	0.706	0.814	0.782
F-Statistic	2.101	78.386	40.923	76.561	78.326	40.638	8.471	22.645	40.410	33.237
Prob > F	0.070	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

Table C.15: RD estimates: Inactivity outflows by age cohort

	I→E					I→U				
	15-24	25-34	35-44	45-54	55-64	15-24	25-34	35-44	45-54	55-64
Design Effects										
Design dummy	0.053	0.064	0.122	0.281	0.433	0.247	0.133	0.049	0.079	0.123
	(0.462)	(0.156)	(0.004)	(0.000)	(0.000)	(0.000)	(0.034)	(0.179)	(0.035)	(0.001)
Business Cycle Effects										
GDP cyclical component	1.402	-0.352	-1.285	-0.171	-0.353	-1.586	-1.870	-1.387	-0.732	-0.402
	(0.128)	(0.536)	(0.017)	(0.781)	(0.691)	(0.039)	(0.020)	(0.004)	(0.123)	(0.342)
Polynomials										
$\widetilde{\text{Time}}$	0.001	-0.003	-0.001	0.002	0.000	-0.008	-0.005	-0.000	0.001	0.003
	(0.873)	(0.190)	(0.722)	(0.433)	(0.950)	(0.008)	(0.092)	(0.827)	(0.467)	(0.079)
$\widetilde{\text{Time}}^2$	0.000	0.000	0.000	0.000	0.000	-0.000	-0.000	-0.000	-0.000	0.000
	(0.001)	(0.693)	(0.457)	(0.218)	(0.847)	(0.002)	(0.006)	(0.087)	(0.447)	(0.679)
Interactions										
Design$\times\widetilde{\text{Time}}$	0.032	0.015	0.038	0.046	0.180	0.099	0.011	0.055	0.056	0.004
	(0.448)	(0.569)	(0.116)	(0.111)	(0.000)	(0.006)	(0.769)	(0.011)	(0.012)	(0.837)
Design$\times\widetilde{\text{Time}}^2$	-0.004	-0.001	-0.007	-0.008	-0.023	-0.008	-0.000	-0.007	-0.006	-0.001
	(0.476)	(0.798)	(0.051)	(0.036)	(0.000)	(0.082)	(0.973)	(0.024)	(0.038)	(0.669)
Observations	55	55	55	55	55	55	55	55	55	55
Adjust. R^2	0.785	0.467	0.566	0.823	0.916	0.843	0.567	0.772	0.836	0.805
F-Statistic	33.900	8.890	12.721	42.881	98.755	49.327	12.800	31.470	46.832	38.037
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Source: Author's calculations based on LFS.
Note: p-values in parentheses. The tilde denotes the forcing variable centered at the discontinuity point.

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