Temporary Contracts across Generations: Long-term effects of a labour market reform at the margin

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Abstract:

We analyze the impact of a labour market reform *at the margin* (an easier use of temporary contracts launched in Spain in 1984) across generations. As this type of reforms applies to new entrants into the labour market (or, in general, new hired workers), we use a regression discontinuity design to estimate a long-lasting effect on the mean temporary employment rates for generations entering into the labour market after the labour market respect to those already in the labour market. The results show a relatively small impact related with the reform at the margin. By educational levels, the estimated effect of the reform at the margin on the mean temporary employment rate is close to zero for those with university level for both genders.

1   Introduction

In the 1980s, all Western countries implemented different labour market reforms in order to increase labour market flexibility and, thereby, employment (Booth *et al*., 2002; Blanchard and Landier, 2002; Homlund and Storrie, 2002; Dolado *et al*., 2002; Barbieri and Sestito, 2008). Everywhere, these reforms were implemented as gradual and/or partial changes of the institutional framework of the labour market. A type of this partial or gradual reform was the *flexibility at the margin*, i.e., affecting only to the new entrants in the labour market and those moving to new jobs.

Temporary and fixed-term contracts are probably the most important type of flexibility at the margin. Originally, they decrease hiring costs, but at the same time they are also characterized by much lower firing costs. Usually, temporary contracts have low or even null severance payments and very low bureaucratic costs linked to the end of the contract in contrast to an open-ended contract. Many European countries have promoted, under different regimes, the use of temporary and fixed-term contracts, but Spain is the most prominent example. While in the beginning in the 1980s, the proportion of wage and salary workers with a temporary contract (the temporary employment rate, TER) was around 10 per cent and concentrated on construction and tourism industry (Fina *et al*., 1989), in the mid 1990s it rose to 33 per cent and affected to all economic activities (Toharia and Malo, 2000; Dolado *et al*., 2002), remaining around 30 percent even after the implementation of different labour market reforms aiming to decrease this rate in 1994, 1997 and 2006. In addition, the widespread of temporary contracts is not mainly linked to temporary work agencies as they were forbidden until 1994. In fact, the TER was above 30 per cent when these agencies began their activity, and nowadays they manage around 16 per cent of the total gross flow of temporary contracts (Amuedo-Dorantes et al., 2008). On the above grounds, Spain is probably the most appropriate country to study any topic related to temporary contracts.

Previous research on temporary work has focused on the effects of fixed-term contracts on training provision, work injuries, or on specific groups such as young people, women or low-skilled workers. Although some authors have analyzed the relevance of different workers' characteristics (as age) of temporary workers (for example, Kahn, 2007), to our knowledge there is not previous research about the relevance of temporary contracts on long-term working trajectories using a generation

approach. We will apply this approach to analyze the impact of the labour market reform fostering temporary contracts on working lives from a long-term perspective, using generations (defined as birth cohorts) as the main unit of analysis and the aggregate temporary employment rate of different generations at different ages as the dependent variable. In these terms, this article adds to the current interest in the long-term effects of flexibility in two-tier labour markets, originated by labour market reforms at the margin in different countries (Boeri, 2009). Therefore, we will check whether there is a long-term impact on the temporary employment rate of different generations according to their different exposure to the labour market reform at the margin easing the use of temporary contracts by firms.

Probably, the most prominent challenge is how to evaluate the impact of a labour market reform at the margin. After the implementation of such reform all individuals are exposed to be hired using a temporary contract. At first sight, there is not any 'non-treated' group as the exposure is complete for young generations entering into the labour market after the legal change and the exposure is partial (not zero) for older generations already in the labour market when the reform was implemented. Therefore, the labour market reform at the margin potentially affected to the working lives of all individuals. Notice that our research will not compare working lives of individuals affected by the reform with a counterfactual of working lives of individuals not affected by such reform. Such comparison is impossible, because we only observe the 'real world' with the implementation of this change in labour market regulation. What we can compare is the working lives of those entering into the labour market after the implementation of this legal change with the working lives of those already in the labour market before the reform. In this vein, we will focus on estimating the long-term impact (if any) of the labour market reform at the margin of 1984 on the mean TER of younger cohorts along their observed working lives. Therefore, we are looking for a sort of 'temporary contracts trap' (or 'long-term precariousness') for younger generations consisting on a relatively higher TER during their whole life course. This is relevant because a higher TER can negatively affect to crucial vital events as relevant delays leaving parents' home, declining fertility rates, lower probability of being eligible for a mortgage, poorer career prospects, higher risk of unemployment, etc. According to this rationale, we present a regression discontinuity design to isolate whether there is a long-run effect of the labour market reform at the margin implemented in 1984 on younger

cohorts (i.e., on those cohorts entering into the labour market after such implementation), respect to older cohorts already in the labour market when the legal change was implemented. Our results will be disaggregated by educational level and gender.

We use micro-data from the Spanish Labour Force Survey (LFS) from 1987 to 2010. To focus on generations, we will use the micro-data from the Spanish LFS to provide a novel picture of information by generational groups (i.e. birth cohort groups) along their life cycles. For this purpose we use artificial (synthetic) cohorts' methodology, widely used in Demography and Epidemiology. We use this data aggregation in descriptive and econometric analyses.

We will show that at a descriptive level it is obvious that younger cohorts have a higher temporary employment rate at the beginning of their working careers, but according to their characteristics (educational level and gender) some of them have relatively faster declines in such rates. On the other hand, some workers experiment higher job mobility irrespective of their generation (as workers with a low educational level). Older cohorts have a higher share of workers with low educational levels and when these workers lose their jobs are also more relatively 'exposed' to a positive long-term effect on their mean temporary employment rate along their working career (as they have a higher probability of being re-hired under the new legal regulation). In fact, the TER of older generations with a low educational level increased very rapidly just after the implementation of the legal reform. The regression discontinuity analysis shows that the long-run increases in the TER are rather low, especially for those with a university educational level, and even more if they are females. Therefore, although the observed differential in mean TER along the life cycle is the largest for younger cohorts of people with a university degree for cohorts closer to the discontinuity, such differences are not mainly linked to the reform at the margin but to differences between these cohorts.

2   Background

The extensive use of fixed-term and temporary contracts in Spain stems from the 1984 labour market reform intended to foster employment providing more flexibility to firms (Toharia and Malo, 2000; Dolado *et al.*, 2002). The legal reform in 1984

established a new type of temporary contract that allows firms to hire employees performing regular activities. Previously, temporary contracts existed, but they were mainly used for seasonal activities as in agriculture or tourism firms, or for economic activities with a very specific task as in the building sector, where many contracts are limited to the construction of the specified building, road, etc. (Fina et al., 1989).

The 1984 reform was implemented amidst the international debate on labour market flexibility and, in the Spanish case, it was a response by the recently elected Socialist government to the pressures applied by employers, who would in fact have preferred a more sweeping reform (Dolado and Malo de Molina, 1985; Toharia and Malo, 2000). Although the trade union UGT (which was very close to the Socialist Party) originally supported this legal reform, later this union jointly with the other main union (CCOO) heavily criticized this reform for increasing job instability and spreading temporary contracts to all industries. This legal reform led to a fast (and unexpected at that time[1]) increase in the TER, reaching 30 per cent by the beginning of the 1990s, and affecting more to women who reached rates slightly below 40 per cent in 1992 (see Figure 1).

In legal terms, the change was 'small' as there was only the introduction of a new type of contract available for employers and, therefore, not affecting to current employees only to job seekers expecting to be hired. However, the legal change was relevant because allowed a much easier use of this type of temporary contract as it was possible hiring workers for permanent tasks of the firm on a temporary basis. This was a remarkable novelty respect to the traditional foundations of the Spanish Labour Law.

As there was a legal change implemented by a new government (with a political ideology markedly different respect to the previous government) and changing key concepts of Spanish Labour Law (in other words, 'innovating' in a way difficult to predict in that time), this legal reform can be considered as exogenous in terms that workers and employers could not anticipate this legal change in order to delay or to anticipate a relevant amount of key decisions affecting in the long term to the working

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[1] Later, many authors (for example, Dolado et al., 2002) linked this extensive use of temporary contracts by firms with the wide gap in firing costs respect to open-ended contracts. Although next labour market reforms tried to partially close this gap, de facto the firing costs gap has remained almost unchanged (García-Martínez and Malo, 2007).

lives of different generations. Here, manipulation by workers means that, for example, individuals enrolled in university anticipating the legal reform would quit their studies in order to be hired before the reform to decrease the risk to be hired with a temporary contract instead of an open-ended contract. This is not plausible because the expected returns of pursuing in university studies were higher than the alternative of being hired for a non-university job before the reform. In addition, the high TER reached later and its negative side effects were not anticipated by any researcher, labour practitioner, policy maker, etc. On the side of firms, it is possible to argue that some employers postponed hiring decisions in order to use a temporary contract for vacancies related with permanent tasks instead of using open-ended contracts before the reform. However, such manipulating behaviour must have been very limited to immediately before the legal change and without any relevant impact on working lives of different workers' generations. As we said before, this legal change did not follow a years-long social and political debate. In fact, it was a change taken by a new government in the first part of its first legislature of fours years and affecting Labour Law in a way not easy to anticipate as the reform broke a key principle of the Spanish Labour Law tradition. On the above grounds, we will consider this reform at the margin as a discontinuity in terms of a regression discontinuity design.

The spread of temporary contracts was so huge that negative effects became visible in the 1990s. Many authors have stressed that such a huge proportion of temporary and fixed-term contracts on total wage and salary workers has different unintended and worrying effects: (a) on economic performance as less probability of participating in training (Albert *et al.*, 2005), a lower productivity growth (Bentolila and Dolado, 1994) or higher injury rates (Guadalupe, 2003); (b) on the postponement of family formation and fertility (Ahn and Mira, 2001; Adsera, 2004; McGrath and Keister, 2008); and (c) on working lives as a longer and more precarious period of labour market integration (OECD, 1998).

In the two next decades, different legal changes in 1994, 1997 and 2006 have tried to decrease the aggregate TER of the Spanish economy. In 1994, the legal regulation of fixed-terms contracts was restricted and in 1997 some kinds of temporal

contracts were abolished[2]. Moreover, a new permanent contract with lower severance pay for dismissed workers was created although it was not applicable to all the new hires[3] and financial subsidies for employers using permanent contracts were launched (Toharia and Malo, 2000). These financial subsidies were changed in 2006 (see Toharia *et al.*, 2005 or Toharia and Cebrián, 2007).

However, in general, all these reforms have had partial but small effects on the aggregate TER[4] and it has remained around 30 per cent[5]. Only very recently, since 2007, we can see a slow decreasing change, providing descriptive evidence that the legal reform implemented in 2006 slightly decreased average TER (specially in the private sector and in small firms; Malo and González-Sánchez, 2010). Nevertheless, the relevant decrease observed in 2008 is not related with any policy but with the severe employment adjustment because of the current economic recession (heavily focused on temporary contracts and in the construction sector at the beginning and soon extended to the rest of the economy).

Concerning the effects of temporary contracts on labour trajectories, previous research focuses on the analysis of transitions from temporary to permanent employment (Toharia *et al.*, 1998; Alba, 1998; Amuedo-Dorantes, 2000; Hernánz, 2003; Güell and Petrongolo, 2007). The main finding of these analyses is that fixed-term contracts contribute to a high level of transitions between jobs, even for temporary to permanent jobs. On the other hand, descriptive evidence from administrative longitudinal records does not seem to show a 'long-term trap' in general, although several specific and small groups might remain for long time in temporary employment (Toharia and Cebrián, 2007).

Anyway, temporary contracts are mainly concentrated on new entrants, dominated by young people (everywhere, and not only in Spain; see, for example, Khan, 2007). Older people (those already in the labour market before the labour market

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[2] See, for example, García-Martínez and Malo (2007) and Malo y Toharia (2008) for further details on 1994 and 1997 legal regulation reforms; on the 2006 reform see Toharia and Cebrián (2007).

[3] The exception was males aged 30-45 years old with unemployment spells below one year but in 2001 it was extended to other workers

[4] For instance, Kugler *et al.* (2002) obtain that the reform in 1997 seems to have had a positive net effect on permanent employment for young men and women but not for older men.

[5] Slightly below 30 per cent when considering exclusively the private sector (Dolado *et al.*, 2002).

reform was implemented) will have temporary contracts when they are re-hired after a dismissal or when they have a delayed entry (or re-entry) into the labour market. Such situation for older workers would be usually more frequent for those with lower educational levels and for women with an intermittent working career because of family reasons.

The unequal generational allocation of temporary contracts (and their costs and benefits) has been interpreted as an implicit intergenerational agreement (Garrido, 1996). While temporary contracts (and short-term unemployment) are concentrated among young people who remain in their parents' home, open-ended contracts and job security is concentrated among male breadwinners. Then, since the 1980s, parents (mainly husbands) were working under permanent contracts with a relatively higher employment security. They paid taxes to finance unemployment benefits and they provided direct financial support to their sons and daughters who were enrolled in the educational system or with temporary contracts in the labor market. The result was a drastic change in the organization of Spanish families, with a very important postponement of family formation for young people and a decrease of the fertility rate below 1.3, which is usually known as a lowest-low fertility rate (Garrido and Malo, 2005; Billari and Kohler, 2004).

This unequal distribution of job instability and unemployment by generations linked to adaptations and changes in family organization would be behind the 'social peace' of Spanish society, although the unemployment rate has been relatively high since the 1980s compared to other OECD countries (Garrido, 1996; Toharia and Malo, 2000). However, a relevant cost of this 'social peace' has been a high aggregate TER linked with many structural problems of the Spanish economy (Toharia et al., 2005).

3   Database and main variables: generation, age and educational level

We use data from the Spanish LFS[6] for the period 1987-2010, launched by the Spanish Statistical Office (*Instituto Nacional de Estadística*) following EUROSTAT standards that are based on International Labour Organization recommendations about

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[6] In Spanish *Encuesta de Población Activa,* or EPA in short.

labour market statistics. The Spanish LFS covers the population residing in private households[7]. The sample size each quarter is approximately 65,000 households (around 200,000 individuals).

The LFS has information regarding the personal and labour characteristics of individuals (sex, age, employment status, employment characteristics of the main job, labour status, previous work experience, search for employment, etc.). In the second quarter of 1987 a question was added in order to capture the type of contract of the individual. Therefore, our empirical analysis begins with 1987 and it finish with 2010, and all observations correspond to the period following the implementation in 1984 of a labour market reform at the margin allowing a much easier use of temporary contracts. Therefore, these data do not allow any before-after analysis of this labour market reform, but we can observe what happens for different groups (here generations) along their working lives after the reform. Specifically, we can compare long-term results (in terms of their temporary employment rate) generations entering into the labour market after the implementation of the 1984 reform with generations already in the labour market when such reform was implemented.

As we want to focus on generations we define artificial (synthetic) cohorts and we follow them for the whole period covered by our database (1987 to 2010). For artificial or synthetic cohort analysis, it is not necessary to follow the same individuals over time. It is enough to simply observe a representative sample of individuals with the same characteristics over time (as the LFS does). For example, in the survey of year 1 we have a representative picture of individuals aged 20-25. In the survey of year 2 we have a representative picture of individuals aged 21-26. As the sample of the survey is partially renewed, the interviewees are not exactly the same group in both years. However, they are equivalent from a statistical perspective, because in each year the sample is designed to give a right representation of population. If we define groups of individuals according to their birth cohort in the first available year, we can follow this group until the last available year (in our case, along 23 years, from 1987 to 2010)[8].

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[7] Foreign nationals are included in the resident population if they have lived or intend to live in Spain for more than one year.

[8] There is an implicit assumption in this reasoning: there is not any external shock adding new individuals to the birth cohorts. However, in Spain the proportion of foreign immigrants has grown significantly since 2000, changing the composition of population living and working in Spain. In order to maintain the

Individuals are assigned to cohorts based on year of birth, from 1921 to 1995, each cohort consisting of 5 birth years. Later, in the econometric analysis, we will restrict ourselves to 12 generations, beginning with the 1926-30 birth years and ending with 1981-1985. We apply this restriction because we observe the oldest generations only at the end of their labour trajectory while for the youngest ones we only observe their first steps into the labour market. We aggregate data in cells considering age (in 5 years intervals), educational level (3 levels), birth cohort and year. We use this aggregation for men and women separately. As we are not interested in seasonal changes of TER, we only take the second quarter of each year (as it is the trimester less affected by seasonality[9]). Concerning age variable, it consists of five-year groups from 25-30 to 56-60. Notice that below 25 we would have many individuals enrolled in the education system (especially for those with university level) and beyond 60 early retirements become relatively frequent. Focusing on ages from 25 to 60 we observe individuals with a higher attachment to the labour market. On educational level, we consider three levels: up to the mandatory level, secondary level (post-mandatory secondary education and vocational training), and university level. We use only three levels in order to have enough observations in the corresponding cells, in special for the lowest level. Finally, we have for each gender, a dataset of 1,020 cells when restricting to 12 birth cohorts[10]. Each cell is weighted using the weights provided by the LFS which are coherent for different years when using artificial cohorts (Garrido and Chuliá, 2005). Weights are used in descriptive and econometric analyses.

In Figures 2 and 3 we show the TER of the different generations for each age group[11] (for males and females, respectively). Cohorts younger than those born in 1961-1965 (and, therefore, entering into the labour market once the 1984 reform was implemented) have the highest peaks in the TER. These cohorts have a TER above 70

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homogeneity of our birth cohorts along the whole time period, we restrict the analysis to Spanish individuals who were born in Spain. We exclude those not born in Spain because a non negligible proportion of immigrants from Latin America are eligible for double nationality and they usually apply for when they have the right to do so.

[9] In fact, figures for the second quarter are the closest to the mean of the corresponding year.

[10] We have 1,020 cells instead of 4,140 (=24 years x 12 cohorts x 5 years intervals x 3 educational levels) because we can not follow all cohorts in all covered years. In fact older cohorts are mainly observed in the first part of the period while younger cohorts are mainly observed in the second part. This is also the reason to leave aside the oldest and the youngest cohorts and to restrict the dataset to 12 cohorts.

[11] In Figure 2, 3 and 4, successive five-year age groups overlap in order to have smoother shapes.

per cent until they are 22-26 years old. The rate decreases considerably until they are 30-34 years old. In this age group the TER becomes rather stable at a level of around 30 per cent (slightly higher for women than for men).

The incidence of temporary contracts by gender shows that women have a slower decreasing pattern in TER. In addition, female older cohorts born before 1956 suffered a clear increase in TER immediately after the reform (notice that the first observation year for these cohorts is 1987). These differences are consistent with the higher TER observed with cross-section data in Figure 1.

The level of education plays a major role in the labour market. It is well-known that a higher level of education leads to a greater probability of employment and the opportunity to secure better jobs. In this sense, in cross-section data, those with a university degree show a lower TER than the rest of the population (Dolado et al., 2002; Toharia et al., 2005). Figure 4 shows the TER for the two extremes of education up to the mandatory level and university (both by gender). For men and women the TER is much higher and remains higher for longer periods for individuals up to mandatory educational level in all cohorts. The difference is especially intense for older cohorts: while those with university level have a TER below 10 per cent when they are over 35 years old, those up to mandatory educational level deal with rates over 20 per cent even at the end of their working careers. For cohorts born after 1956-60 we see that, in the case of those with a university level, there is a relatively rapid decrease in their TER, from 80 per cent when they are 16-20 years old to below 30 per cent when they are 30 years old. Those up to mandatory educational level experiment the same high TER at the beginning of their employment trajectories but we do not observe the same decrease over time; on the contrary, these cohorts exhibit a relatively higher TER during their whole life cycle. So fixed-term contracts are the main way of entering the labour market for young cohorts independently of their level of education; the difference is that those with university studies improve their employment situation (in terms of job stability) rapidly while those with the lowest levels of education do not. By gender the general pattern by educational level is rather similar, although with a higher TER in each educational level and a slower decrease as age increase, especially for those women up to the mandatory educational level.

4   Econometric analysis

4.1   A regression discontinuity design

We will estimate the impact of the labour market reform at the margin on the average TER along the working life on different generations using a regression discontinuity design (RDD). Following Lee and Lemieux (2010), we use the framework of treatment effects literature to present the main characteristics of the RDD.

Let us consider an individual i (or a unit as a birth cohort) and two potential outcomes for this individual: $Y_i(1)$ if the individual or the unit is treated and $Y_i(0)$ otherwise. Of course, the causal effect of the treatment will be the difference $Y_i(1)$-$Y_i(0)$. However, the basic problem is that we can not observe both results because an individual is only observed as treated or not treated. The empirical strategy consists of focusing on average effects of the treatment over two populations, treated and non-treated groups. The difference $Y_i(1)$- $Y_i(0)$ between both populations only captures the causal effect of the treatment if the characteristics of treated and non-treated populations are the same. Therefore, individuals or units must be as randomly assigned to the treatment and to the non-treatment (or control) group.

In an RDD the randomization between treated and non-treated observations (the so-called 'unconfoundedness' assumption; Rosenbaum and Rubin, 1983) is trivially satisfied if the discontinuity separating treated and non-treated groups is really exogenous and individuals can not manipulate their assignment into the treated and non-treated groups. In fact, as Lee (2008) formally shows, RDD does not assume randomization, but it is a consequence of agents' inability to precisely manipulate the assignment variable near the discontinuity cut off. Following Lee and Lemieux (2010), when the variable used to assign the treatment is above a well defined threshold, the treatment dummy is always equal to 1. When the assignment variable is below the threshold, the treatment dummy is always equal to 0. Therefore, conditional on the assignment variable, there is not any other variation in the dummy treatment variable and, as the cut off defining the threshold is exogenously determined, it is not correlated with any other factor.

Formally, considering that c is the cut off in the assignment variable Z we only observe $E[Y_i(1)|Z]$, for example, to the right of the cut off (the treatment group) and

E[Y$_i$(0)|Z] to the left of the cut off. Defining as ε the bandwith around the cut off, the average causal effect of the treatment at the cut off *c* is the following:

$$\lim_{\varepsilon \downarrow 0} E[Y_i \mid Z_i = c + \varepsilon] - \lim_{\varepsilon \uparrow 0} E[Y_i \mid Z_i = c + \varepsilon] = E[Y_i(1) - Y_i(0) \mid Z = c]$$

In words, we can estimate the average causal effect of the treatment defined by the discontinuity *c* if *Z* (and any other factor) is continuous and therefore the group of those right below the cut off (the non-treatment group) is a valid counterfactual for those right above the cut off (the treatment group). A consequence of this reasoning is that randomization in RDD is only strictly guaranteed in the vicinities of the cut off (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Therefore, a crucial issue is the size of the bandwith around the cut off. However, a closer approach to the threshold has costs, because it will decrease the number of cases included in the estimations and, therefore, the precision of estimated coefficients might be much lower (standard errors will be larger). On the other hand, including cases far from the cut off will improve precision (standard errors will be smaller), but at the risk of losing 'unconfoundedness'. When including more individuals far from the threshold, the likelihood of having other variables than the cut off affecting the outcome variable will be higher. The length of the bandwith in the assignment variable is a common problem in RDD. The classical solution consists of estimating models with different bandwiths and including some covariates as controls in estimations. Some authors (as Imbens and Kalyanaraman, 2009) propose strictly quantitative methods to estimate the optimal bandwith.

How do we confront in this research these usual worries of RDD? First, we explained at the beginning of the second section that the labour market reform at the margin implemented in 1984 was fully exogenous. Therefore, the key issue is how to define the cut off in meaningful terms of our research, that is, whether there are systematic differences in TER along the life cycle for generations entering into the labour market after the 1984 reform respect to those generations already in the labour market before the reform.

The cut off is defined according to the assignment variable to the 'treatment'. Therefore, the assignment variable is the age in 1984. As we are interested in results aggregated by birth cohort, our assignment variable is the mean age for each cell (by age group, cohort, educational level and year). The simplest definition of the cut off is the minimum legal age for working which is 16. Therefore, individuals with less than

16 in 1984 were fully exposed to the effects of the reform (a wider use of temporary contracts by firms) during their whole working lives. On the other hand, individuals with more than 16 years were potentially into the labour market when the reform was implemented and therefore they would be only affected if they lose their jobs or they have a delayed entry into the labour market.

Nevertheless, we must acknowledge that those following secondary level education and university education will have a delayed entry into the labour market respect to the minimum legal age to participate into the labour market in 1984. Therefore, we will estimate separate models for these individuals changing the cut off and considering the most common age finishing the corresponding educational levels: 18 for secondary education and 23 for university education[12]. Usually individuals enter into the labour market after finishing their studies, but for checking robustness of results we will also include estimations with the 16 years old cut off for the secondary and university levels and 18 years old also for the university level. As usual, the cut offs are defined as dummy variables where 1 means entering into the labour market after the implementation of the labour market reform (having less than 16, 18 or 23) and 0 the opposite.

Figures 5 and 6 show the TER considering the assignment variable for the full sample and by educational levels for both genders, with different cut offs (16, 18 and 23 in 1984). In all of them, the picture is rather different in both sides of the cut off. Maybe, the most remarkable difference is the higher dispersion for younger individuals especially for those with university level. However, for those up to the mandatory educational levels, observations with only a bit more than 16 years in 1984 have a dispersion rather similar to those entering in the labour market after 1984. In addition, in all cases for much older generations the concentration is in rather lower TER. For both genders, the picture is similar, although dispersion is larger for women irrespective of the educational level. Therefore, the visual examination of the outcome variable

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[12] Therefore, we are considering as 5 years the normal period for finishing university studies. In 1984, university studies usually lasted 5 years with two main exceptions. On one hand, some degrees lasted 3 years (the so-called *Diplomaturas*). On the other hand, physicians, architects and some engineers lasted 6 years. While the cut-offs of 16 and 18 are clear, there is more room for heterogeneity defining 23 years old in 1984 as the cut off for ending university studies and being exposed to the effects of the labour market reform at the margin.

provides a first approach to the eventual effects of the discontinuity (the labour market reform at the margin of 1984) on the outcome variable (TER).

About the bandwith in the assignment variable (mean age), we have chosen a definition in terms of birth cohorts as this is more meaningful for our analysis than simply consider an interval of some years above and below the corresponding cut off. This will allow us a most direct link of the results with differences between generations. Anyway, we will check different possibilities including more or less birth cohorts in estimations, beginning with estimations including all generations.

The econometric specification will be the following:

$$Y_{acjt} = \beta_1 AGE1984_{acjt} + \beta_2 AGE1984_{acjt} * AGE_{acjt} + \beta_3 (1 - AGE1984_{acjt}) * AGE_{acjt} + \alpha X'_{acj} + \varepsilon_{acjt}$$

where a denotes age group, c corresponds to birth cohort, j is educational level, and, finally, t is time (year). All these variables and the aggregation procedure were described in section 3.

We estimate this regression using OLS and we report results with robust standard errors, clustered when possible by birth cohort (defined as 5 year intervals as described in previous sections). In addition, we estimate different models by gender and three educational levels (up to the mandatory level, secondary education and university level). The left hand side variable, Y, is the TER.

Our primary interest focuses on β_1 as the coefficient of *AGE1984*, which is having a specific age for working in 1984 (the year the reform at the margin was implemented). As we explained before, we will use these different ages in 1984 (16, 18 and 23) for different educational levels. As the above expression shows (see second and third terms), the assignment variable (defined as mean age in each cell centred at the corresponding cut off) can have a different form above and below the cut off age.

We have considered a reduced set of covariates[13] (*X*): a set of dummies of the educational levels considered, a linear time trend, and step dummies for 1994, 1997 and

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[13] Usually, covariates are included in RDD. However, notice that as 'unconfoundedness' is granted around the threshold of the assignment variable covariates should be redundant as treated and non-treated individuals would be as randomly selected considering any observable and not observable variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). However, covariates are included to control some remaining heterogeneity for some variables especially relevant, as educational level, time trend and

2006. The expected effect of education is a decrease in the TER when educational level increases. The rationale is that with a higher educational level workers are potentially more productive and more valuable for firms and, therefore, more suitable to sign an open-ended contract. We introduce step dummies in the three years previously mentioned to control for effects related with the legal regulation changes of these years, as some of them tried to affect to the relative use of temporary contracts (as we explained in the second section). However, these dummies are only controls and they are not a proper evaluation for the effects of such reforms.

A common issue in RDD is that the results can be sensible to the specification of the model. This is the reason because some authors propose the use non-parametric models when using an RDD (Lee and Lemieux, 2010). We have estimated some non-parametric models and we will comment such results later[14].

Finally, as behaviour and outcomes of men and women in the labour market are markedly different, models have been estimated separately by gender.

4.2 Results and discussion

We have to sets of results. The first one (Table 1) shows the estimations including all generations and clustering by 12 birth cohorts. The second one (Table 2) includes the results considering only the closest cohorts to the corresponding cut off, but without clustering (because of the small number of clusters).

Now, we comment Table 1. The above panel corresponds to males. Considering the 16 years cut off, the impact of the reform of 1984 ranges from an increase of 3.9 percentage points (pp) in TER for the full sample and for those with lower educational level to an increase of 3.9 pp in TER for. The effect is stronger for secondary and university level for the 18 years cut off: a TER increase of 4.1 pp and 5.6 pp, respectively. Finally, the threshold of 23 years shows a smaller impact of 3.3 pp on the TER of males with university level. Therefore, in general the reform increased the job instability of cohorts entering into the labour market after the reform. To have a reference point to evaluate the meaning of these coefficients is useful to have a look

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before-after regulation changes in our case. Anyway, covariates should not have a discontinuity around the threshold (Lee and Lemieux, 2010). For the educational level, we have checked this continuity assumption with our data using graphs. They are available upon request.

[14] The non-parametric results are not included in the text but they are available upon request.

again to Figure 1. This Figure shows that for all men the observed TER was around 30 percent for the most part of the decades of 1990s and 2000s. Therefore, although the estimated effects are not negligible (and for the 18 years cut off of university males it reaches 5.6 pp) they do not seem really large. A remarkable feature of these results is that the lowest educational level does not show a clearly higher effect and even it is lower than the most part of the estimated coefficients for the secondary and university level. At the same time in the first column, the estimation for all individuals, the lowest educational level has a fixed effect of 14.2 pp respect to the other two educational levels. Both types of results are not contradictory. While having up to the mandatory educational level increases TER in 14.2 pp respect to the other two educational levels, younger cohorts up to the mandatory level have a TER 3.9 pp higher than older cohorts up to the mandatory educational level. In a complementary way, although the university level is related with a lower TER (and higher job stability) respect to be below the secondary level, younger cohorts with university level have a higher TER than older cohorts with a university degree. In other words, they have higher job instability than their older counterparts.

For females (second panel of Table 1), the impact of the 1984 reform is only statistically significant for those with university level. For this group of women the reform increased their TER from 5.1 pp to 6.7 pp (23 years and 18 years cut off, respectively). Also for women, the reform increased job instability for younger cohorts respect to older cohorts of women with a university level, although they have more job stability than women below the secondary level (as the estimation of the first column shows). However, for the rest of women the reform does not create a significant difference in TER between younger and older cohorts.

As we explained in previous section, the effects of the discontinuity should be also checked considering data around the cut off. Table 2 shows the results considering exclusively 2 cohorts, 1961-65 and 1966-70, with the exception of individuals with a university degree considering the cut off of 23 years. For this last case, the two cohorts are 1956-60 and 1961-65, to have one cohort in both sides of the corresponding cut off.

In these estimations, standard errors are robust but they are not clustered by cohorts as we are only considering two cohorts[15].

The estimated effects shown in Table 2 are smaller than those reported in Table 1. In special, for those with a university degree the estimated effect is very close to zero (0.66 pp for men and 0.91 pp for women)[16]. For males, the effect is an increase of 2.38 pp for those up to the mandatory educational level, and an increase from 3.37 to 3.62 pp for the secondary level (cut off of 16 and 18, respectively). For women, the estimated effect is negative but rather small (-1.44 pp) and positive for the cut off of 18 years for those with secondary level (2.88 pp).

Thanks to Figures 7 (males) and 8 (females) we can compare the observed change in mean TER for different cohorts with the size of estimated effects related with the labour market reform at the margin. The black vertical line divides cohorts considered in estimations for the two lower educational levels and the grey vertical line does the same for the university level. For the lowest educational level and secondary education, the observed difference in the mean TER between 1961-65 and 1966-70 cohorts is around the double of the change strictly linked with the labour market reform (for both genders). However, the observed difference in the mean TER between 1956-60 and 1961-65 for individuals with university education is slightly below 10 pp (for both genders) but the estimated change in TER comparing both cohorts attributed to the labour market reform at the margin is below 1 pp.

Definitely, these results as a whole do not support the view that the labour market reform at the margin has created a key difference among generations leaving younger cohorts in a sort of 'permanent trap' of precariousness. On the contrary, our results show that in the worst case the reform might explain an increase in the mean TER of younger generations of slightly below 4 pp for cohorts with secondary level strictly around the implementation of the reform. In special for the case of people with university level the observed differences in mean TER between cohorts are mainly

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[15] Because of cells' weighting jointly with the lack of clustering all coefficients in Table 2 are statistically significant. Therefore, the precision of estimations of Tables 1 and 2 are not strictly comparable.

[16] Considering the other cut offs, the largest effect is for the cut off of 18 years (3.22 pp for men and 4.96 for women).

related with differences in the characteristics of the cohorts and not with the reform at the margin.

Finally, as a robustness check, we have estimated non-parametric models[17] (not reported here but available upon request). In these estimations, the bandwiths are narrower[18] than those considered in estimations shown in Tables 1 and 2. The effect of the labour market reform at the margin is either non significant or slightly negative (but always very close to zero) for the mandatory educational level and the secondary level. For the university level, the estimated effect of the reform was positive and around 2 pp. Therefore, although non-parametric models provide results a bit different than those obtained with the lineal models of Tables 1 and 2, they do not change the main conclusion for those with university level, as the effect of the labour market reform at the margin is very low compared to the observed difference shown in Figures 7 and 8. However, these non-parametric results do not compare different cohorts (as we wanted) but cells inside a specific narrower bandwith around the corresponding cut off, and therefore they do not have an interpretation in terms of birth cohorts as the rest of our results.

5   Conclusions

In this research we have analysed the incidence of temporary employment across generations and the long-term impact of a labour market reform easing the use of temporary contracts by firms. Our data come from Spain where this type of labour market reform was implemented in 1984 and where the temporary employment rate has been among the highest in Europe in the last two decades. We evaluate the impact of this reform at the margin on different generations (defined as birth cohorts). As a reform at the margin only affects to those entering (or re-entering) into the labour market, we focus on the comparison of those entering into the labour market after the

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[17] For our estimations we have used the 'rd' command for Stata developed by Nichols (2011). The 'rd' command allows estimating local linear regression models on both sides of the cut off, using a triangle kernel. In addition, we have also used the syntax programs provided by G. Imbens in his personal web page: (http://www.economics.harvard.edu/faculty/imbens/imbens.html), which is based in the notes by Fuji *et al.* (2009).

[18] The default bandwidth of the command 'rd' is based on Fuji *et al.* (2009) to minimize MSE, or squared bias plus variance, in a sharp RD design.

implementation of the reform at the margin and those already in the labour market in 1984. Therefore, we are not providing a comparison before-after the reform nor a counterfactual of what would have happened without such reform, but a comparison of workers at the margin and workers only at the margin if they lose their job or have a delayed entry into the labour market.

For this evaluation we use micro-data of the Spanish Labour Force Survey to define artificial or synthetic birth cohorts from 1987 to 2010. We follow for this period 12 birth cohorts (defined as 5 years groups). There are relevant observed differences by gender, as women usually have a higher TER along their working lives almost irrespective of their birth cohort. Descriptive differences by educational level are remarkable. For those up to the mandatory educational level, the TER increased relatively rapidly after the implementation of the reform for older cohorts (those already in the labour market in 1984) and for those entering after the reform at the margin the TER was initially relatively high and slowly decreasing as age increases. For those with a university degree, TER remained almost unchanged for older cohorts after the reform, and although for younger cohorts the TER is very high at the beginning of their working life, it decreases at a faster pace as age increases.

Econometric estimations of the effect of the labour market reform at the margin are based on a regression discontinuity design. The discontinuity is the reform as it can be considered as exogenous respect to the behaviour of workers and firms. We define the cut off as the entering age into the labour market by educational level (16 for those up to the mandatory educational level, 18 for those with secondary level, and 23 for those with a university degree). The estimated impact on the mean TER for cohorts entering into the labour market after the legal change of 1984 is relatively small and it is not supportive of the view that the reform has heavily affected to younger generations creating a long-term relevant increase in TER along their working lives. Especially, this is not true for those with a university degree. Although for those with university level the observed difference between the cohort entering after the reform and the cohort already in the labour market is the largest one (around 10 percentage points), almost the whole difference is related with differences between both cohorts and not with the discontinuity created by the labour market reform at the margin of 1984. In the vicinities of the cut off of 23 years in 1984, the effect strictly related with the reform at the margin is 0.66 percentage points for males and 0.91 for females.

Of course, these results do not support that this reform was 'good' or 'positive' for the welfare of individuals. That is a different question concerning a comparison of the observed results under the reform and a counterfactual scenario without such reform (or with a reform for all and not at the margin). Our results are based in comparisons with data obtained after the implementation of the reform for all cohorts. Therefore, our regression discontinuity analysis stresses that observed differences in mean TER for different cohorts (in special for those with university level) is mainly related with differences between cohorts entering after and before the implementation of the labour market reform at the margin of 1984 and not strictly with a heavy impact of the easier use of temporary contracts on younger cohorts.

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Table 1. Regression Discontinuity results on the TER$_{ajct}$ (weighted data and clustered errors by 12 birth cohorts).

MALES	ALL		EDUC1		EDUC2		EDUC2		EDUC3		EDUC3		EDUC3	
Cut off: 16 years in 1984	0,039	**	0,039	**	0,038	***			0,058	***				
Cut off: 18 years in 1984							0,041	***			0,056	**		
Cut off: 23 years in 1984													0,033	***
Age*(1-Cut off)	-0,009	***	-0,009	***	-0,009	***	-0,009	***	-0,009	***	-0,009	***	-0,007	***
Age*(Cut off)	0,010	***	0,004	***	0,010	***	0,011	***	0,019	***	0,018	***	0,018	***
Educ1: Up to mandatory educ. level	0,142	***												
Educ2: Secondary level	-0,003													
Linear time trend	-0,007	***	-0,006	***	-0,008	***	-0,008	***	-0,009	***	-0,009	***	-0,009	***
Reform 1994 (1=Yes)	0,065	***	0,073	***	0,046		0,046		0,045		0,045		0,045	
Reform 1997 (1=Yes)	-0,026		-0,028	**	-0,023		-0,024		-0,013		-0,014		-0,014	
Reform 2006 (1=Yes)	-0,038	***	-0,045	***	-0,023		-0,023		-0,032		-0,031		-0,032	
Constant	0,290	***	0,423	***	0,309	***	0,284	***	0,305	***	0,275	***	0,201	***
R2	0,7804		0,8119		0,7256		0,7302		0,729		0,7372		0,7505	
N	607665		351019		152004		152004		104642		104642		104642	
FEMALES	ALL		EDUC1		EDUC2		EDUC2		EDUC3		EDUC3		EDUC3	
Cut off: 16 years in 1984	0,0365		0,0344		0,0014				0,0586	**				
Cut off: 18 years in 1984							0,0208				0,0696	***		
Cut off: 23 years in 1984													0,0515	***
Age*(1-Cut off)	-0,0102	***	-0,0083	***	-0,0120	***	-0,0116	***	-0,0140	***	-0,0129	***	-0,0098	***
Age*(Cut off)	0,0106	***	0,0009		0,0080	***	0,0075	***	0,0184	***	0,0181	***	0,0199	***
Educ1: Up to mandatory educ. level	0,1185	***												
Educ2: Secondary level	-0,0049													
Linear time trend	-0,0069	***	-0,0047	***	-0,0063	**	-0,0063	**	-0,0107	***	-0,0108	***	-0,0106	***
Reform 1994 (1=Yes)	0,0493	**	0,0577	***	0,0520	*	0,0512	*	0,0290		0,0286		0,0299	
Reform 1997 (1=Yes)	-0,0215		-0,0363	***	-0,0267		-0,0268		0,0070		0,0059		0,0074	
Reform 2006 (1=Yes)	-0,0279	*	-0,0215	***	-0,0253		-0,0245		-0,0360		-0,0346		-0,0374	
Constant	0,3685	***	0,4514	***	0,3832	***	0,3542	***	0,4229	***	0,3804	***	0,2776	***
R2	0,7134		0,7785		0,7319		0,7333		0,8149		0,8231		0,8330	
N	368249		156096		100433		100433		111720		111720		111720	

*** Signifies statistically different from zero at the 1% level or better, **at the 5% level or better and *at the 10% level or better. Reform variables are step dummies (1=year of the reform onwards).

a: age group, j: educational level, c: birth-cohort (generation), t: time (year). EDUC3: University educational level.

Table 2. Regression Discontinuity results on the TER$_{ajct}$ (weighted data and robust standard errors; 2 birth cohorts).

MALES	ALL	EDUC1	EDUC2	EDUC2	EDUC3	EDUC3	EDUC3*
Cut off: 16 years in 1984	0,0231	0,0238	0,0337		0,0051		
Cut off: 18 years in 1984				0,0362		0,0322	
Cut off: 23 years in 1984							0,0066
Age*(1-Cut off)	-0,0205	-0,0199	-0,0179	-0,0163	-0,0274	-0,0233	-0,0142
Age*(Cut off)	-0,0129	-0,0258	-0,0122	0,0060	0,0136	0,0170	0,0191
Educ1: Up to mandatory educ. level	0,1522						
Educ2: Secondary level	-0,0106						
Linear time trend	-0,0149	-0,0130	-0,0153	-0,0153	-0,0215	-0,0215	-0,0097
Reform 1994 (1=Yes)	0,0511	0,0708	0,0252	0,0301	0,0197	0,0211	-0,0155
Reform 1997 (1=Yes)	-0,0581	-0,0546	-0,0565	-0,0626	-0,0619	-0,0635	-0,0224
Reform 2006 (1=Yes)	0,0070	-0,0163	0,0313	0,0325	0,0422	0,0424	0,0379
Constant	0,4785	0,5952	0,4757	0,4343	0,6083	0,5428	0,2661
R2	0,8745	0,8504	0,8639	0,8591	0,8193	0,8199	0,7413
N	164714	82448	53875	53875	28391	28391	33304
FEMALES	ALL	EDUC1	EDUC2	EDUC2	EDUC3	EDUC3	EDUC3*
Cut off: 16 years in 1984	-0,0096	-0,0144	-0,0200		0,0140		
Cut off: 18 years in 1984				0,0288		0,0496	
Cut off: 23 years in 1984							0,0091
Age*(1-Cut off)	-0,0205	-0,0172	-0,0153	-0,0124	-0,0306	-0,0261	-0,0181
Age*(Cut off)	0,0040	-0,0056	0,0065	-0,0004	0,0043	0,0125	0,0208
Educ1: Up to mandatory educ. level	0,1105						
Educ2: Secondary level	-0,0048						
Linear time trend	-0,0122	-0,0075	-0,0114	-0,0114	-0,0197	-0,0197	-0,0102
Reform 1994 (1=Yes)	0,0321	0,0503	0,0363	0,0365	-0,0064	-0,0032	-0,0375
Reform 1997 (1=Yes)	-0,0378	-0,0334	-0,0570	-0,0560	-0,0158	-0,0197	0,0124
Reform 2006 (1=Yes)	0,0057	-0,0169	0,0206	0,0195	0,0222	0,0229	0,0251
Constant	0,5242	0,5541	0,5022	0,4634	0,6631	0,5896	0,3320
R2	0,7930	0,8354	0,7999	0,7997	0,8559	0,8571	0,8088
N	118579	42697	39456	39456	36426	36426	38420

Robust standard errors. Weighted data. All coefficients are statistically significant. Reform variables are step dummies (1=year of the reform onwards).a: age group, j: educational level, c: birth-cohort (generation), t: time (year). EDUC3: University educational level.
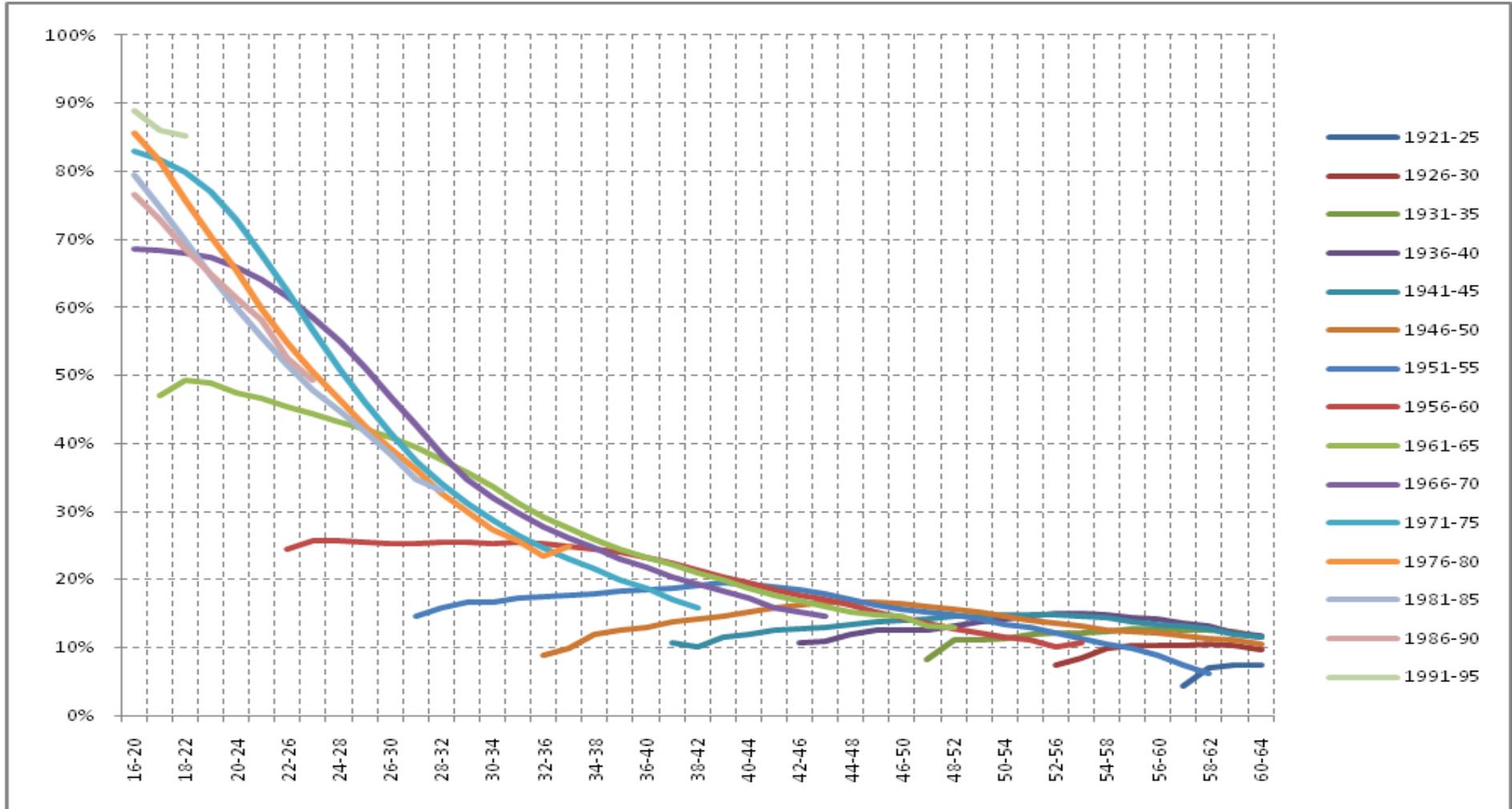
Figure 1. TER by gender in Spain. Source: LFS.

Figure 2. Temporary employment rate by generation (as five years birth cohort groups) and by age group, for males. Source: LFS.
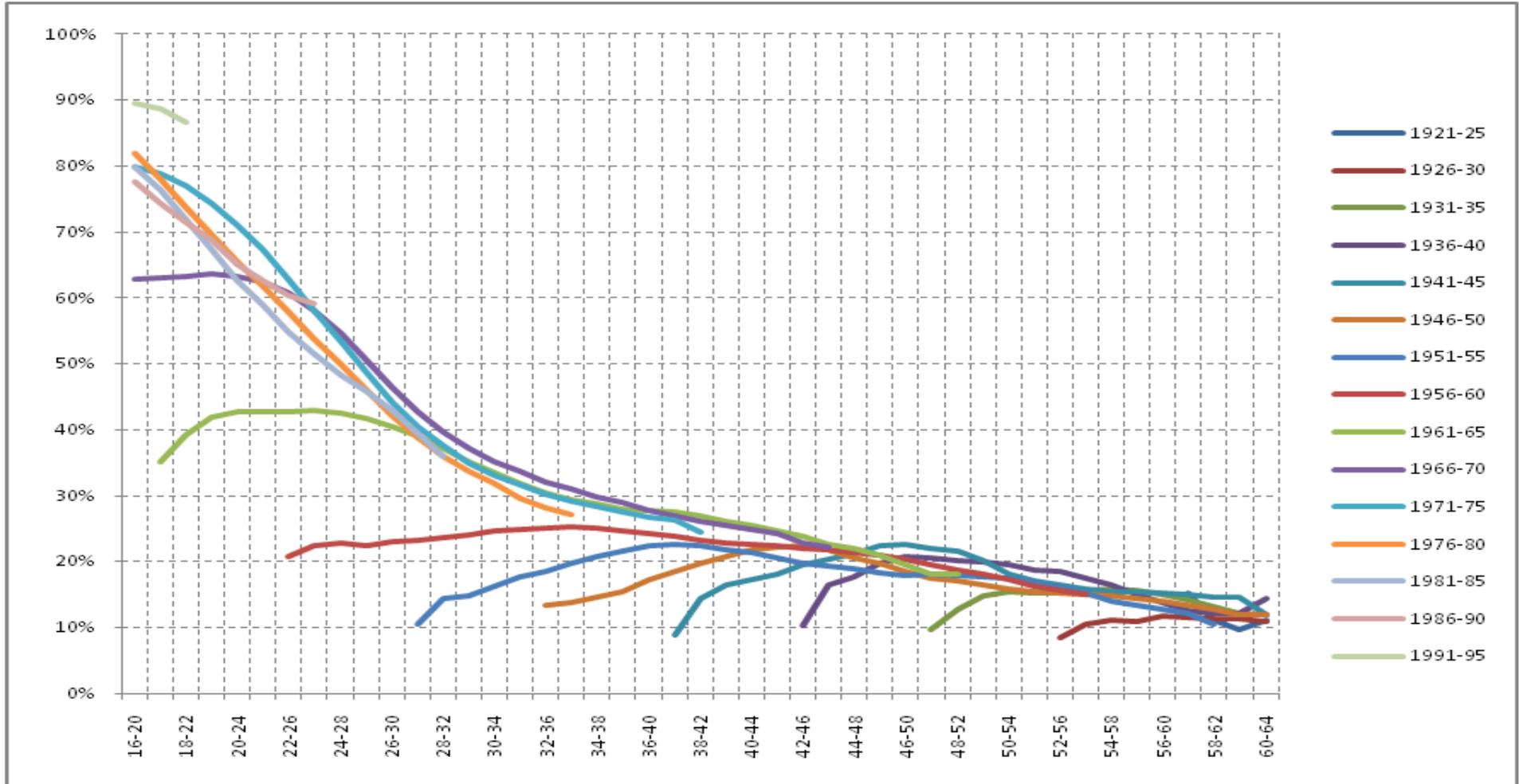
Figure 3. Temporary employment rate by generation (as five years birth cohort groups) and by age group, for females. Source: LFS.
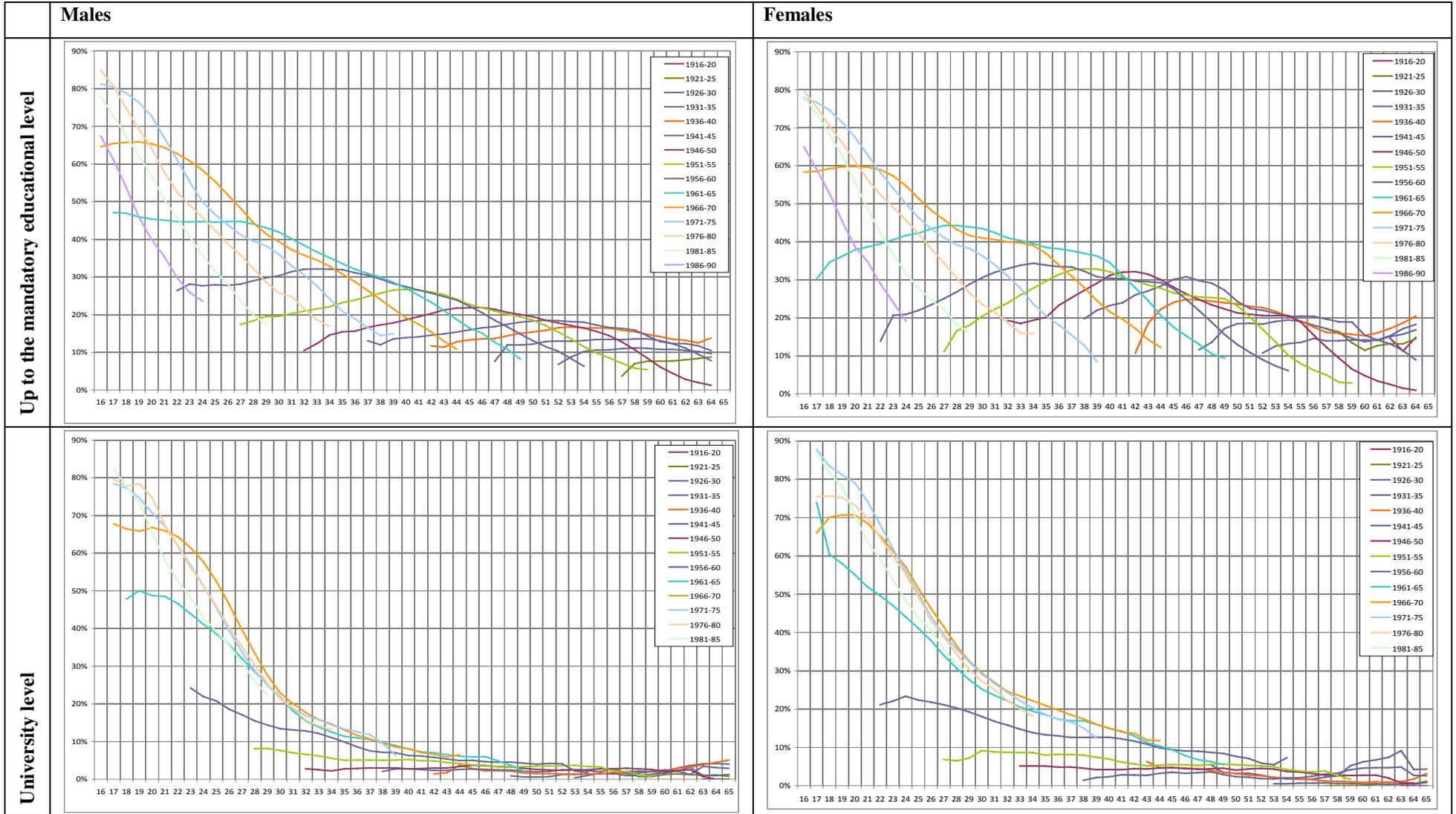
Figure 4. Temporary employment rate by birth cohort and age group for two educational levels, by gender. Source: LFS.
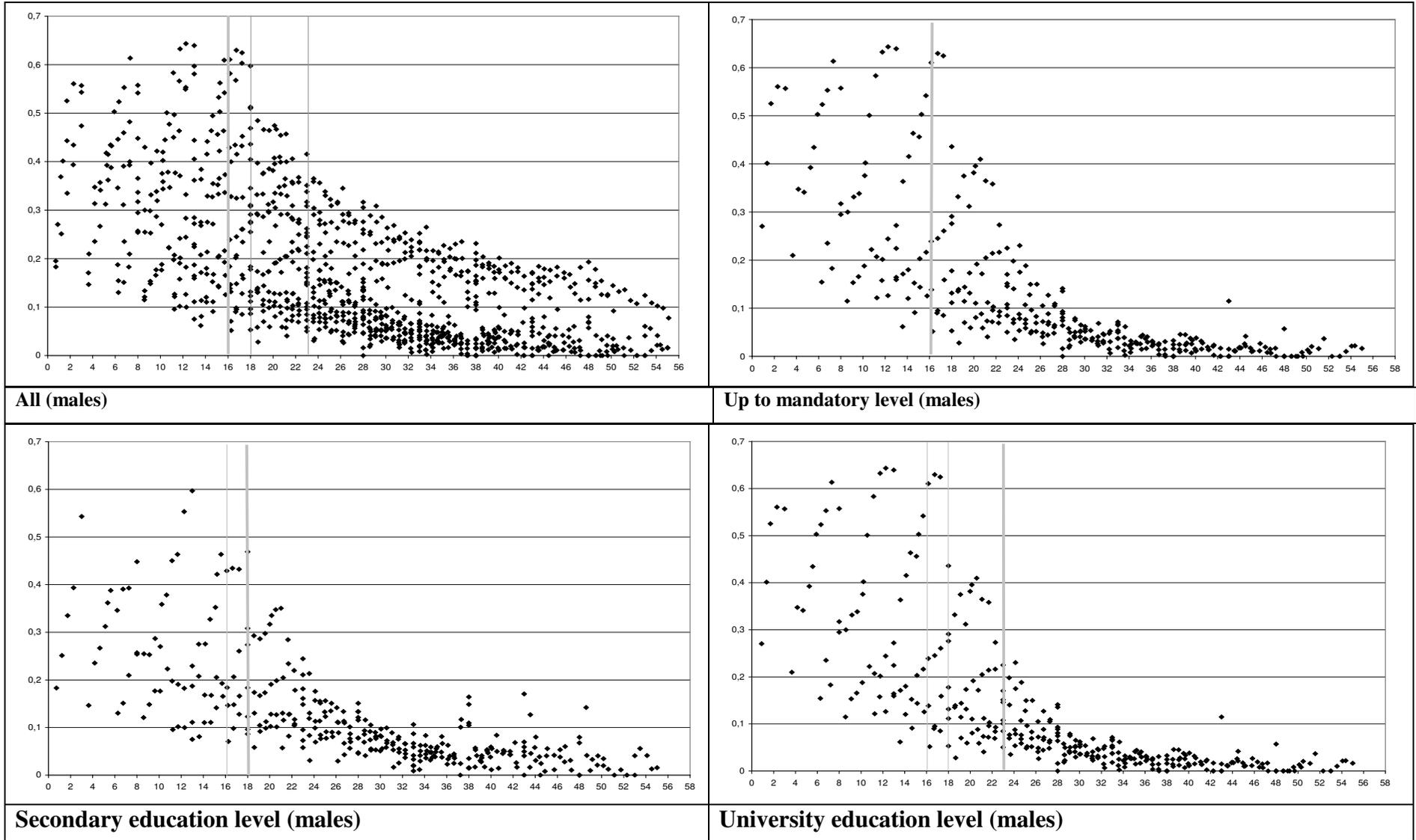
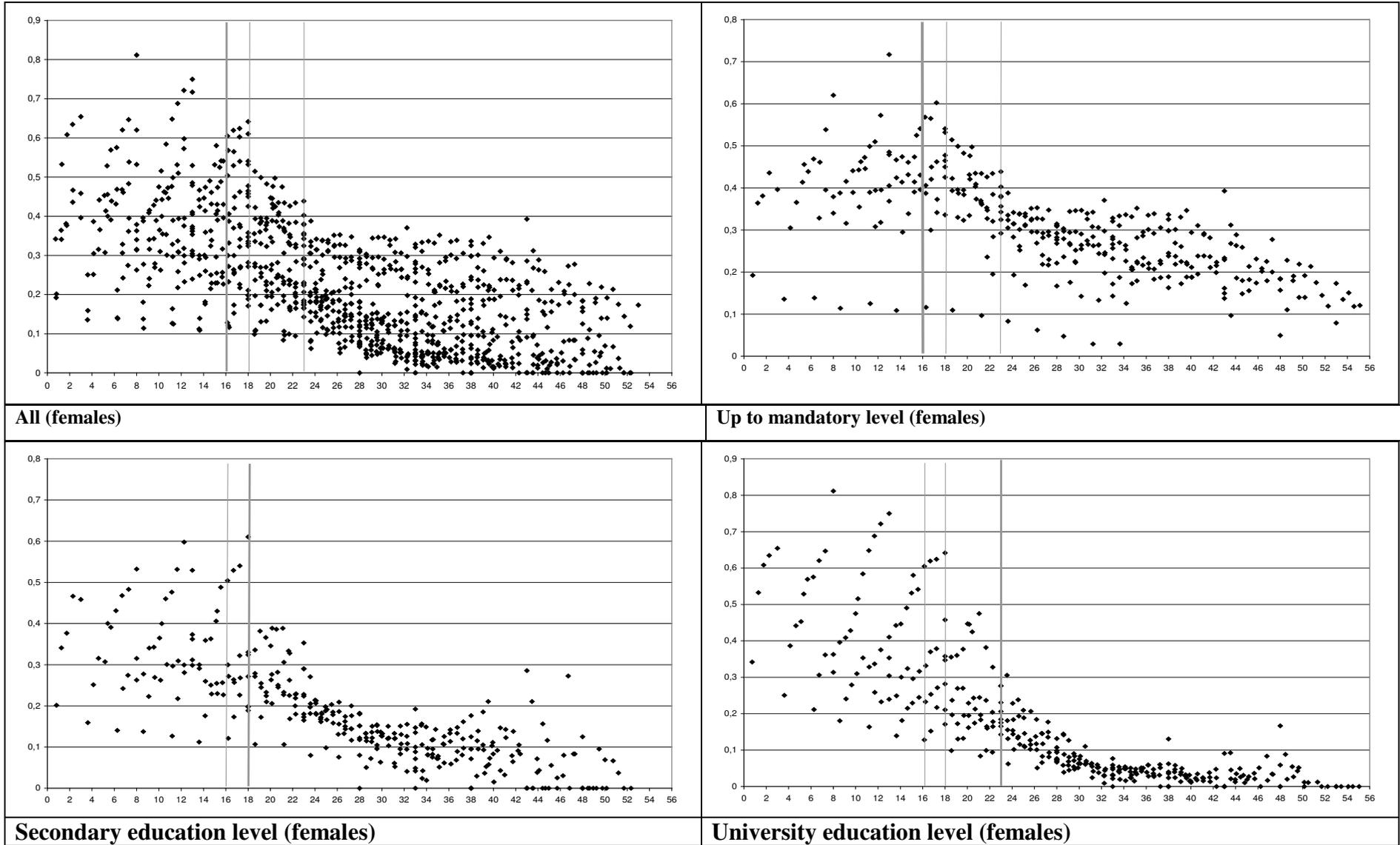Figure 5. Temporary Employment Rates by educational level with the corresponding cut offs (only males).

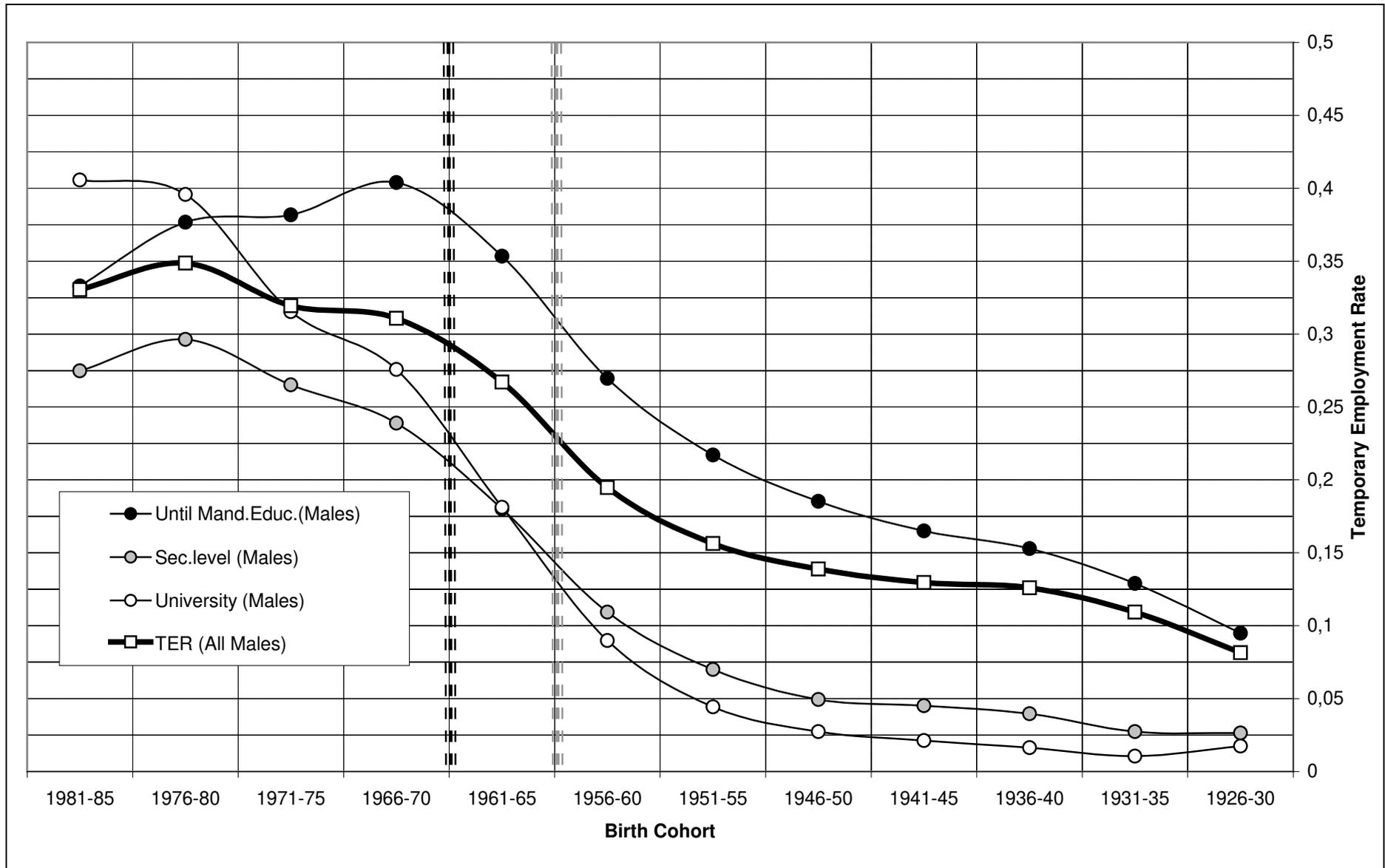Figure 6. Temporary Employment Rates by educational level with the corresponding cut offs (only females).

Figure 7. Mean Temporary Employment Rate for the whole life cycle of all cohorts by educational level (only males).
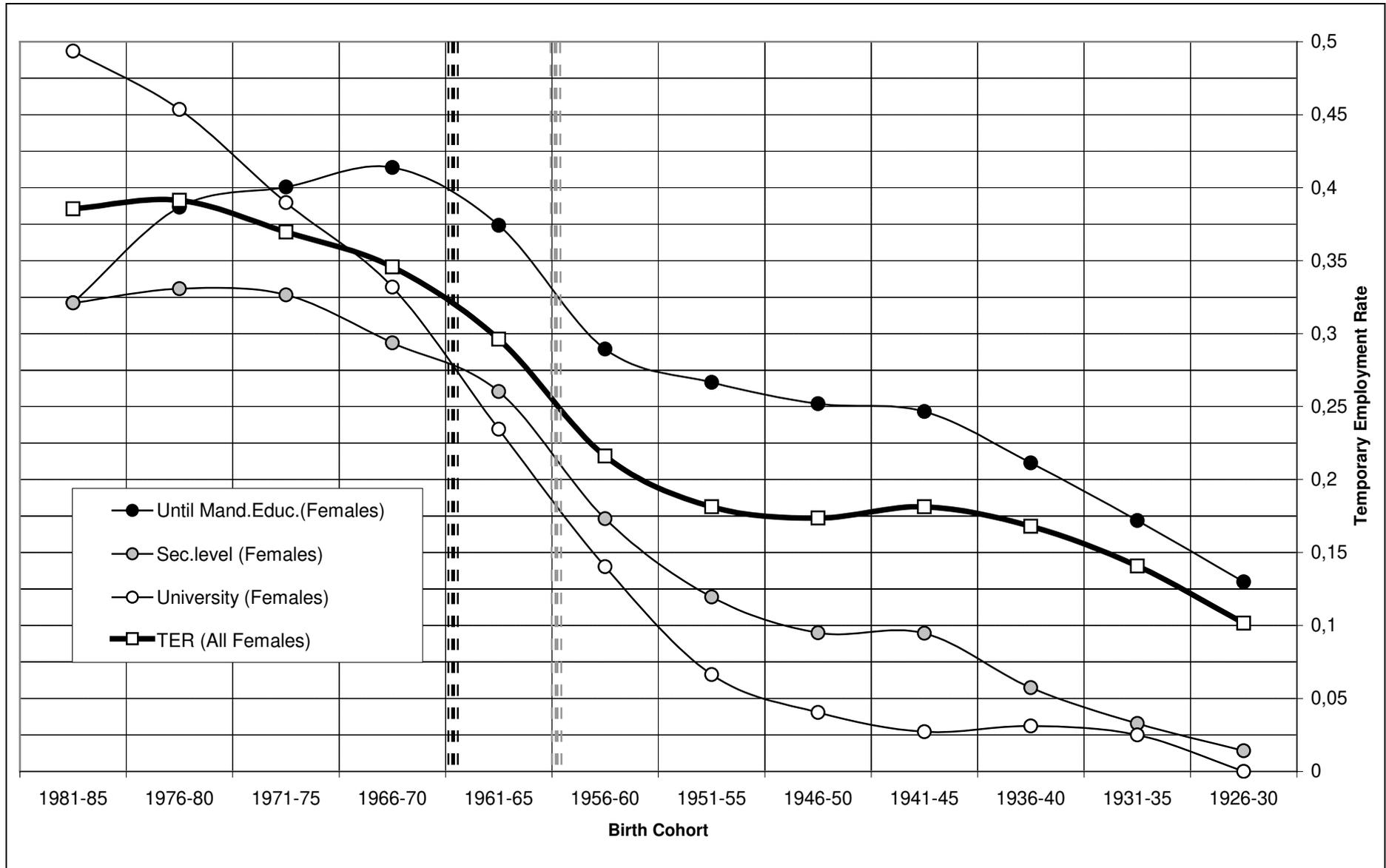
Figure 8. Mean Temporary Employment Rate for the whole life cycle of all cohorts by educational level (only females).