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THREE ESSAYS ON THE EFFECT OF UNEMPLOYMENT ON CONSUMPTION AND HEALTH

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Doing a Ph.D. has been one of the most exciting activities in my life. Probably one of the facts that made it so compelling is the unusual path that my career followed. I was 26 years old when I decided to attend the university and 27 when I started. Thus, my path was a bit far from the standard one. Indeed, I had doubts about my capacity to earn a bachelor's degree. I thought I could pass some courses but probably would need a longer than usual period to complete my degree. I was working full time, and I switched to part-time, knowing that I was paying a high price for my degree (what later I learned is called opportunity cost). I could not afford to lose my time, and therefore started working as hard as I could from the beginning. I did it fair enough in the first courses, what encouraged me to make an extra effort to get good grades. At that point, I decided to take it to the limit. And here I am. I learned a lot during the journey, and I met some extraordinary people I want to thank.

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INTRODUCTION

This thesis consists of three chapters analyzing the consequences of unemployment caused by the Great Recession in Spain on two commodities: cultural services and health. More precisely, it analyzes how the deterioration of the labor market as a consequence of the 2007-2009 economic downturn affected expenditure on cultural goods and services, health status, and mental health.

Prior to the Great Recession, the Spanish unemployment rate was around 8 percent. The 2007-2009 macroeconomic shock skyrocketed the unemployment rate, more than doubling it, up to 19 percent. After the financial crisis of 2007-2009, the unemployment rate continued growing, albeit at lower rates. In 2013 the unemployment rate peaked at 26 percent, and as a result of the slow recovery, the unemployment rate remained around 22 percent in 2015. Therefore, I exploit this macroeconomic shock to study how the production of certain commodities was affected during this period.

These adverse circumstances might be translated into a reduction of utility at individuals level. According to the household production theory (see Becker (1965) or Lancaster (1966)) households produce commodities through the household production function. In order to produce these commodities, two resources are needed: own time and market goods. Therefore, labor market status determines the availability of these resources and therefore is crucial for household production. Whether individuals are employed or unemployed determines time available and the capacity to purchase market goods through labor income. Consequently, a recession period that increases the unemployment rate creates the appropriate framework to learn about the response to a shock that increases one of the resources but reduces the other.

The first commodity analyzed is cultural consumption. When individuals become unemployed, cultural consumption becomes relatively cheaper, as far as the time used for it cannot be sold in the labor market. Nevertheless, as unemployment reduces disposable income, cultural expenditures absorb a higher share of household income, becoming expensive. The first chapter uses the Spanish Household Budget Survey data to analyze variation in cultural goods expenditure related to changes in employment status. The findings suggest that, while the probability of reporting positive expenditure on cultural goods is barely affected when a household member becomes unemployed, those households that do participate in cultural activities reduce their cultural expenditure.

The second commodity, health status, is analyzed separately in two different chapters. The theoretical approach relies on the health production model suggested by Grossman (1972) model, where individuals combine time and market goods to produce health. As unemployment affects time available and income, it serves as an appropriate example of an economic shock that affects own time and the capacity to acquire market goods due to the loss of labor income. The second chapter analyzes the effect of unemployment on self-reported health and health investments. It looks at how a micro-level shock, such as unemployment, and a macro-level shock, such as an economic downturn, affect a set of health indicators. To do so, this research utilizes different approaches. First, the analysis focuses on the effect of unemployment on self-reported standardized health status measurements. Then, it shifts to study how the impact of unemployment varies with the business cycle. In addition, this chapter also looks at the evolution of self-reported health trends of workers after an economic downturn, regardless of their employment status. Throughout, the impact of unemployment is assessed on multiple health inputs and outcomes, focusing on differences between unemployed and employed individuals, and how the Great Recession affected these potential differences. Finally, I discuss how unemployment effects on self-assessed health are related to the effects on inputs for health production.

In the third chapter, I extend the analysis regarding health and unemployment, focusing on mental health. More precisely, I analyze the differences in mental health between unemployed and employed individuals, and the consequences of an economic downturn on those differences. An unemployment spell is a shock that removes affected individuals from their reference points. First, unemployment might affect self-perceived mental health through the violation of a social norm. If individuals perceive that their social category implies being employed, unemployment means a deviation from the expected situation (see Akerlof & Kranton (2000)). Unemployment might then reduce the life valuation of unemployed individuals. Moreover, it reduces disposable income. Thus, individuals affected by job losses are also removed from their reference points regarding earnings. Henceforth, unemployment may affect both inherent standard reference points and social reference points. Furthermore, a recession period also affects an individual's reference points. A higher unemployment rate may be linked to worse future perspectives but can change social norms as well. For instance, those individuals who belong to high unemployment groups may experience a weaker violation of the social norm when they are unemployed.

Finally, the last chapter summarizes the conclusions reached throughout the dissertation, and denotes further research ideas.

2

CULTURAL EXPENDITURE OF THOSE WHO ENTER (OR EXIT) UNEMPLOYMENT

2.1 Introduction

We examine the relationship between the employment status of household members and their cultural expenditure and contribute evidence on the income elasticity of the demand for culture using the Spanish household budget survey (*Encuesta de Presupuestos Familiares*, EPF). More precisely, we use causal inference methods to estimate the impact on cultural expenditure and income when a household member enters (or exits) an unemployment spell, and then calculate the corresponding income elasticity.

Focusing on households where one of the members has begun a period of unemployment allows us to analyze the effect of the associated income variation within households. Entering an unemployment spell represents one of the largest income shocks an individual or household can experience, and therefore household income is highly correlated with labor market status.

However, using changes in the labor market status as a source of variation poses a problem, as the change in employment status, income, and expenditure might be jointly affected by unobserved confounders. In other words, labor market status changes do not provide an exogenous source of variability. To cope with this problem, we use well-known causal inference methods e.g.

the semi-parametric difference in differences (Abadie, 2005) and matching (Abadie & Imbens, 2011). These methods allow us to assess the effect of unemployment on the changes in cultural expenditure and income, thus controlling for unobserved confounders as long as they are time invariant or vary equally on average for those beginning a period of unemployment and those who remain employed.

The baseline results show that when a household member's labor status changes to unemployed, households reduce cultural expenditure, and their income falls significantly, more so when households already had an unemployed member to begin with. Our income elasticity estimates are economically sensible and comparable to those based on more traditional methods.

A distinguishing feature of cultural expenditure is that many households report zero expenditure. To account for this feature of the data, we extend the econometric procedure to a conditional on positive (COP) analysis. Our findings indicate that participation does not change significantly and participating households exhibit larger income elasticity. As an extension to the previous results, we analyze whether gender and educational attainment modify the effect of unemployment on expenditure and income.

An additional contribution made by this research is that we are able to assess the effect of unemployment on cultural expenditure not only of those who enter unemployment but also of those who exit unemployment and become employed and therefore determine whether the effects are symmetrical. While households that experience a change from zero to one member unemployed reduce their cultural expenditure significantly, those households that change from one to zero members unemployed increase their cultural expenditure insignificantly. However, household income changes by a similar amount but in opposite directions.

As a final contribution, we investigate how the results vary with the phase of the business cycle. Splitting the analysis into periods of a booming economy and periods of recession, we find that the results are not symmetrical. A recession reduces the cultural consumption of those who enter unemployment, but those who exit unemployment do not increase cultural expenditure significantly when the economy recovers.

The remainder of the chapter is organized as follows. Section 2.2 reviews some strands of related literature and provides some background information. Section 3.3 describes the dataset, the treatments analyzed and econometric methods used. Section 2.4 reports the main findings and some robustness checks which lend internal validity to our estimates. Sections 2.5 and 2.6 report some extensions and Section 3.7 concludes.

2.2 Background

This chapter provides income elasticity estimates of the demand for cultural goods and services in Spain. Our choice of cultural expenditure as the outcome of interest is not casual. There are three reasons for our choice. First, the cultural sector is an economically relevant sector in Spain. It represents about 2.5 per cent of the Spanish GDP (see Ministerio de Cultura y Deporte, 2019). Second, cultural goods and services are certainly not necessary and often considered luxury goods, and therefore should exhibit income elasticity above unity. Third, a distinguishing characteristic of household expenditure on culture data is that many households report zero expenditure. This particular feature of the data allows us first to analyze participation in these markets, and then carry out a conditional on positive analysis.

While a high income elasticity of the demand for cultural goods and services makes economic sense, the empirical evidence does not always back this reasoning. The surveys by Seaman (2006) and Lévy-Garboua & Montmarquette (2011) indicate that income elasticity estimates range from well below one to considerably above one depending on the type of cultural goods and services being analyzed. The meta-analysis by Legoux et al. (2014) suggest that income elasticity estimates depend on the level at which the analysis is carried out. When the analysis is carried out at the category level estimates tend to be above one, while studies at the organization or consumer levels find income elasticity below unity. In a study for Norway, Ringstad & Løyland (2011) reported an income elasticity above one for performing arts, and below one for cinema. Using German data on theater attendance, Zieba (2009) finds income elasticity near unity. Bonato et al. (1990) report income elasticities below unity for the performing arts in Italy. Using Spanish data, Prieto-Rodriguez et al. (2005) find income elasticities above one for three expenditure categories: performing arts, museums, cinema and other events; books, magazines and newspapers; and records, films and audiovisual materials. García-Enríquez & Echevarría (2018) report expenditure elasticities of 0.67 for shows, museums, internet, radio and TV licenses and 1.03 for non-text books and periodicals.

Previous studies on participation include Gray (2011) and Falk & Katz-Gerro (2016). In addition, Ateca-Amestoy (2008) and Ateca-Amestoy & Prieto-Rodriguez (2013) analyze participation in the United States. They conclude that a key determinant factor in cultural participation is cultural capital, making an even bigger contribution than education towards explaining participation.

We analyze a very interesting case study: Spain during the 2006-2015 decade. During this period, unemployment in Spain represented a large shock. Before the Great Recession, in 2007, the Spanish unemployment rate was 8.23, peaking in 2013 reaching 26.09, and then falling again to 22.06 in 2015. Figure 3.2 plots the evolution of the GDP per capita and unemployment rate over the last years of the Great Moderation, the Great Recession, when a large share of the active population became unemployed, and the posterior partial recovery of the Spanish economy. The unemployment rate started increasing before the GDP began to fall in 2009, and from that point, both variables exhibit a remarkably negative correlation.

On the other hand, it is interesting to notice that the unemployment shock affected a fairly heterogeneous population. Using data from EPF 2006-2015 waves, Figure 2.2 shows the share of unemployed individuals across several dimensions. It shows that unemployment affected males and females, individuals at all educational attainment levels, and all within working age. Therefore, the treated group is heterogeneous and not a particularly rare subsample.

Finally, the unemployment shock had a remarkable impact on income: during the 2006-2015 period, the average household income reduction as a result of unemployment ranged from EUR 396 to EUR 818 per month depending on the year. This is a quantitatively significant amount, especially taking into account that a large share of those affected by unemployment received unemployment insurance payments and other monetary benefits.

2.3 Data, Treatment Groups and Methods

2.3.1 Data

We use data from ten waves, 2006 to 2015, of the Spanish household budget survey (*Encuesta de Presupuestos Familiares,* EPF) conducted by *Instituto Nacional de Estadística* (2015).[1] In each yearly wave, nearly 22,000 households are interviewed, about half the sample rotates, and over eighty per cent of the households are interviewed twice. Households are chosen using stratified random sampling from census districts. Each household completes three questionnaires that record household characteristics, household members characteristics, and expenditure categories according to the Classification of Individual Consumption by Purpose (COICOP). Households are uniformly distributed across the 26 two-week slots of the year. During their corresponding two-week period, households report their expenditures which are then annualized.

We focus the analysis on two outcomes: cultural expenditure and income. We define cultural expenditure as the sum of expenditure on three categories: cinema and performing arts (COICOP category 09421), museums and other exhibits (09422), and books (0951).[2] According to Ministerio de Cultura y Deporte (2019), these three items account for 1.6 per cent of the Spanish GDP.

In addition to cultural expenditure, the EPF includes information on educational attainment, age, gender, and nationality for each household member. The EPF also contains household-specific covariates, such as region of residence and population of the town of residence and income. As the EPF reports expenditure at the household level, we are forced to use households as the units of analysis and aggregate covariates to the household level accordingly. Table (4.1) reports descriptive statistics for the 2015 wave of the EPF survey. Descriptive statistics for other waves are in line with those reported in this table. Columns (1) to (4) report descriptive statistics for the

1 We exclude households from the African territories Ceuta and Melilla from the analysis.

2 Notice the difference between our definition of cultural expenditure and the aggregate expenditure reported by INE termed “cultural services” which includes cinema and performing arts (item 09421) and museums and other exhibits (item 09422), as in our definition and, in addition, also includes pay per view TV/radio (item 09423), electronic equipment rentals (item 09424) and private party services, services for pets and photographic services (item 09425), but does not include books (item 0951).

raw, unweighted data, and columns (5) and (6) the means and standard deviations using the survey weights. The EPF reports *annual* household expenditures and *monthly* income. The average annual household cultural expenditure is EUR 117.4 and the average monthly household income is EUR 1,962.51. On average, households have 0.3914 household members whose educational attainment is primary education or less, 0.7348 household members whose educational attainment is lower secondary education, 0.4822 members whose educational attainment is upper secondary, and 0.6206 members with tertiary education. The average number of household members is greater in the upper age intervals. The average household size is 2.68 members, 1.38 of whom are females and 2.45 are Spanish citizens. Almost 37.94 per cent of households living in towns of more than 100,000 people, and 24.66 per cent of households live in towns of less than 10,000 people. Finally, the share of households by region ranges from 0.033 to 0.11.

In the sequel, when we restrict the analysis to subsamples of households with certain characteristics, we are forced to work with unweighted samples. Failing to account for sampling weights amounts to loosing representativeness. However, the descriptive statistics reported in Table 4.1 for the weighted and unweighted samples are fairly similar, what provides some external validity to our estimates.

2.3.2 Treatments

Restricting the analysis to those households that participate twice in the survey, we are able to observe changes in the labor market status of household members. This allows us to observe cultural expenditure before and after a household member became unemployed. More precisely, for each wave, we select households with the same number of members in and out of the labor force in the first and second periods. The treatment group includes those households that have one additional unemployed member in the second period. The control group is defined as those households with an equal number of unemployed members in both periods. We analyze two treatments, T01 and T12. Households in treatment group T01 have no unemployed members in the first period, and just one member unemployed in the second period. Households in the control group have all active members employed in both periods. Households in the treatment group T12 have one unemployed member in the first period, and two unemployed members in the second period. In this case, the control group has just one unemployed member in both periods. We do not study the effect of having a third or more unemployed member, as sample sizes would be too small.

Notice that treatments T01 and T12 are incremental. In other words, households exposed to treatment T12 start the first period with as many unemployed members as households exposed to treatment T01 ended up with in the second period. Therefore, treatments T01 and T12 are not independent, but levels of a multilevel treatment. However, households used to assess the effect of treatment T01 have no unemployed member in the first period (whether treated or not in the second period), and households used to assess the effect of treatment T12 have one unemployed member in the first period. Thus, these samples have no overlap.

Figure 2.3 illustrates the evolution of the average household expenditure on culture (dashed-dot line), the average household cultural expenditure conditional on reporting positive expenditure (dashed line), and the share of households reporting positive expenditure (solid line). Panel A shows that, for the entire sample, cultural expenditure decreased by about 40 per cent over a decade and the share of households reporting positive expenditure decreased from 43 per cent in 2006 to 32 per cent in 2015. Panels B and C of Figure 2.3 plot the same magnitudes for the samples used to analyzed treatments T01and T12, respectively. The patterns of participation and expenditure (conditional and unconditional) are very similar to the full sample. The only remarkable difference between the samples is that households used to analyze treatment T01, with no unemployed members in the first period, spend more than those used to analyze treatment T12, which already have an unemployed member.

2.3.3 Methods

In this section, we summarize the procedure described in Gardeazabal & Polo (2020) to estimate an income arc-elasticity for the demand for cultural goods and services. The identification strategy relies on basic economic principles. Suppose we were able to observe an individual income and cultural expenditure under two conditions, when employed and also when unemployed, holding everything else constant. Of course, this is unfeasible as we can observe an individual under only one of the two employment statuses, but not under both at the same time. For the sake of the explanation, suppose we were able to observe income and cultural expenditure under employment status, say I^0 and E^0, and unemployment, say I^1 and E^1. Notice that under the *ceteris paribus* clause, the price of cultural services, p, is the same in the two situations. Therefore, quantities are $Q^j = E^j/p$, $j = 0, 1$. Under these circumstances, we could calculate the income arc-elasticity of the demand for cultural services as

$$\epsilon = \frac{\frac{Q^1 - Q^0}{(Q^1+Q^0)/2}}{\frac{I^1 - I^0}{(I^1+I^0)/2}} = \frac{\frac{E^1 - E^0}{(E^1+E^0)/2}}{\frac{I^1 - I^0}{(I^1+I^0)/2}}. \tag{2.1}$$

The estimation procedure replaces the unknown quantities I^j and E^j, $j = 0, 1$, by the expected value of these quantities for those who enter an unemployment spell, $\mathbb{E}\left(E^j \mid D = 1\right)$ and $\mathbb{E}\left(I^j \mid D = 1\right)$, $j = 0, 1$, where D equals one for the unemployed and zero otherwise. Notice that an estimate of $\mathbb{E}\left(E^1 \mid D = 1\right)$ is simply the average expenditure of those who transit into unemployment. However, an estimate of $\mathbb{E}\left(E^0 \mid D = 1\right)$ is much more difficult to obtain, as we do not observe how much those who enter unemployment would have spent on cultural goods and services, had they remained employed. Also notice that, the numerator $E^1 - E^0$, would be replaced by the conditional expected value, $\mathbb{E}\left(E^1 - E^0 \mid D = 1\right)$, which is the Average Treatment Effect for the Treated (ATET). Similarly, the other terms in equation 2.1 would be replaced by their conditional expected value.

The empirical evidence is based on two estimation methods: the Semi-parametric Difference-in-Differences (SDID), e.g. Abadie (2005), and the matching method, e.g. Abadie & Imbens (2011). The matching method replaces the unknown values $\mathbb{E}\left(E^0 \mid D = 1\right)$ and $\mathbb{E}\left(I^0 \mid D = 1\right)$ by the average expenditure and income of untreated households (none of whose members transited into unemployment) that are similar in terms of household characteristics to the treated households (one of whose members entered unemployment). Instead, the SDID method estimates directly the ATET as a weighted average of the temporal variation in expenditure, weighting up (down) those households whose covariate values are under-represented (over-represented) among the untreated. These matching/weighting schemes make treatment and control groups more similar in terms of observable household characteristics, thus making the comparison licit. Furthermore, as the SDID and matching methods assess the effect of entering unemployment not on the "level" but on the "time variation" of cultural expenditure and income, these methods allow for differences between treatment and control groups in terms of unobservable household heterogeneity, as long as it is time invariant, or varies equally for the treated and untreated households on average. In addition, the matching estimates are computed in two different ways: a standard matching estimation and a "trimmed" version where the same procedure is applied to the previously matched sample.

2.4 Main Findings and Robustness Checks

2.4.1 Main Results

Table 2.2 reports the ATET estimates. According to the SDID estimate, treatment T01 reduces the average household annual cultural expenditure by around EUR 24.90 for the treated population. This fall represents over 16 per cent of the average household cultural expenditure among the treated for the period 2006-2015. According to the matching and trimmed matching estimates, the fall would be somewhat smaller, EUR 22.96 and EUR 17.08, respectively, although the latter is not significant. The impact on income is negative and significant, ranging from a fall of EUR 463.72 to EUR 495.64, depending on the estimation method. Estimated arc-elasticities range from 0.46 to 0.62, rather low compared with the evidence reported in the literature. The second panel of Table 2.2 reports the results for the T12 treatment, which are all significant. The estimated ATET on cultural expenditure ranges from EUR 30.16 to EUR 39.27. In this case, the expenditure drop represents from 19 to 26 per cent of the average household expenditure among the treated. Treatment T12 has a negative impact on income ranging from EUR 396.37 to EUR 464.70. The associated elasticity estimates range from 0.85 to 1.21. In comparison with treatment T01, treatment T12 has a larger negative effect on household expenditure, smaller negative effect on income, and results in larger income elasticity estimates.

2.4.2 Covariate Overlap

The identification of the influence of unemployment on cultural consumption rests on the assumption that both control and treatment groups are exchangeable. This means that if we switched treatment statuses, the ATET would remain the same. Under these circumstances, using the outcome from the control group as a proxy for the counterfactual outcome for treated is fair. Unfortunately, this assumption is not testable. However, if control units provide a reasonable counterfactual, treatment and control groups should not differ considerably in their characteristics. As in Abadie & Imbens (2011), and Imbens (2015), we assess covariate overlap using the normalized mean differences between treated and control groups. Table (2.3) shows normalized mean differences. Columns (1) to (3) report statistics for the T01 sample, and Columns (4) to (6) for the T12 sample. Columns (1) and (4) show the normalized differences for the full samples. Columns (2) and (5) display the normalized differences after trimming the sample, keeping the treated and matched untreated observations. Columns (3) and (6) present the normalized differences after weighting the covariates according to the SDID scheme. Column (1) indicates that only four covariates exhibit normalized differences above 0.3. Comparing columns (1) and (2) shows that matching reduces covariate imbalance somewhat as only one covariate has normalized differences above 0.3 after matching. Comparing Columns (4) and (5) shows that matching does not reduce covariate imbalance in the smaller sample used to analyze treatment T12. Columns (3) and (6) indicate that the weighting scheme used in the SDID estimator is very effective in reducing covariate imbalance.

2.4.3 Placebo Tests

In this section we perform placebo test *la* Bertrand et al. (2004). To do this, we generate a fictitious sample where treatment is randomly assigned, keeping the share of pseudo-treated observations the same as in the original sample. Then we estimate the effect of the placebo T01 treatment on cultural expenditure. We repeat this random assignment a large number of times and compute the share of false positives detected. Under the null hypothesis of no treatment effect, the percentage of false positives should equal the theoretical size of the test.

Table (2.4) reports the results of our placebo test. Columns (1) to (3) show the results for 2,000 simulations each, and column (4) their average. We report the share of false positives using 5 and 10 per cent nominal sizes. The SDID estimator of the ATET performs fairly well with empirical sizes close to the nominal ones for both treatments. The matching method results in a test size that sometimes even doubles the theoretical size, and therefore tends to over-reject the null of no significant treatment effect. Matching on the trimmed sample does much better than in the whole sample. Overall, the placebo results lend some internal validity to our estimates, especially for the SDID estimates.

2.5 Extension

2.5.1 Conditional on Positive Analysis

As explained above, to account for the fact that many households report zero cultural expenditure, we use a two-step procedure. First, we estimate the ATET on participation, that is, the average effect of unemployment on cultural markets participation for the treated. Second, we estimate the ATET-COP, that is, the ATET using only those observations reporting positive expenditure in the second period. Table (2.5) indicates that treatment T01 does not have a significant effect on participation. Depending on the estimation method, treatment T01 reduces cultural expenditure by between EUR 51.54 and EUR 54.04, and household income falls by an amount ranging from EUR 477.61 to EUR 482.81. The fall in income as a result of unemployment is about 24 per cent of the average household income, somewhat above the 17 per cent fall found by Kawano & LaLumia (2017) for the US. The implied elasticities range from 0.63 to 0.65.

The results for treatment T12 vary remarkably depending on the estimation method. The SDID estimate indicates that treatment T12 reduces participation by 4.69 per cent, although the effect is only significant at the ten per cent level. The ATET-COP for treatment T12 is EUR 122.41 when we use the SDID, and is only EUR 49.93 for the matching estimator using the trimmed subsample. Similarly, but to a lesser extent, the ATET-COP for treatment T12 reduces household income by EUR 559.00 according to the SDID estimator, and EUR 423.95 using the matching estimator with the trimmed sample. The implied income elasticity estimates range from 0.69 to 1.18. Finally, to put our results into perspective, we look at previous income elasticity estimates for cultural goods and services in Spain. Prieto-Rodriguez et al. (2005) estimated a demand system using data from a predecessor of the Spanish household budget survey and reported expenditure elasticity estimates from 1.24 to 1.75. García-Enríquez & Echevarría (2018) also estimated a demand system using the Spanish household budget survey and reported expenditure elasticity estimates for different cultural goods subgroups ranging from 0.67 to 1.03.

2.5.2 Inverse Treatment

In this section we investigate the inverse treatment effect: the effect on household cultural expenditure and income when a household member abandons unemployment to enter an employment spell. It should be remarked that the symmetric effect of unemployment on household income can be understood as a robustness check that provides internal validity to the analysis. In order to understand why this is the case, notice that, as argued above, the effect of leaving unemployment on cultural expenditure can conceivably be different from that of entering unemployment. However, the inverse treatment effect on income must necessarily be symmetric. Indeed, entries and exits from unemployment are accompanied by reductions and increases in income. As the estimated treatment effects of unemployment and employment on household income are symmetric, the analysis of the inverse treatment effect lends credibility to our estimates.

To this end, we define two additional treatments: T10 and T21. Households exposed to treatment T10 have one unemployed member in the first period and no unemployed members in the second period, while households in the control group have one unemployed member in both periods. Similarly, households exposed to treatment T21 are those with two unemployed members in the first period and one unemployed member in the second period, while households in the control group have two unemployed members in both periods. Notice that in the regular treatment T01 (respectively T12), we select a sample of households that have no (one) member unemployed in the first period, and in the inverse treatment T10 (respectively T21), we select a sample of households that have one (two) unemployed member in the first period. Therefore, these samples have no intersection.

To save on space, we restrict the analysis to SDID estimates. Table 2.6 reports the results for treatments T10 and T21, both the conditional on positive and unconditional estimates. In all cases, treatments have a positive impact on household cultural expenditure and income, although only the estimated effects on income are significant. In addition, treatment T10 significantly increases the share of households reporting positive cultural expenditure by 2.56 per cent while treatment T21 does not have a significant effect on participation.

Having the possibility of estimating the effect of a given treatment and the inverse treatment allows us to analyze whether their effects are symmetric. In other words, we investigate whether the impact of an additional unemployed household member is the same in absolute value as the effect of an unemployed member less, but goes in the opposite direction. To this end, Figure 2.4 displays estimates of the inverse treatments T10 and T21 reported in Table 2.6 as well as the direct treatments T01 and T12 estimates previously reported in Tables 2.2 and 2.5. In all cases, the inverse treatment effect estimates have the opposite sign from those obtained for the direct treatments. The inverse treatments T10 and T21 have an impact on income fairly symmetrical with respect to the direct treatments T01 and T12, both conditional on positive and unconditionally. However, the inverse treatments T10 and T21 affect cultural expenditure asymmetrically. Unconditionally, the inverse treatments T10 and T21 impacts on cultural expenditure are similar in size to the direct treatment effects but fail to be significant. The conditional on positive estimates are also statistically insignificant, and the estimated effect of T21 is smaller in absolute value than that of T12. A feasible explanation for this asymmetric impact could be that when a household experiences the shock of having a member unemployed, households decide to cut down on unnecessary consumption. However, when a household member becomes employed, households might have to repay debts incurred during the period with unemployed members or simply might want to restore savings to the pre-unemployment period level, and therefore cannot increase their cultural consumption immediately. More generally, cultural consumption might be related to household wealth. Thus, cultural consumption might not react immediately after a period of time with one or two more household members unemployed, which represents a large negative wealth shock. Finally, the asymmetric impact on cultural expenditure might be related to the differences between the inverse and direct treatments. The analysis of treatment T01 uses a sample of households with all active members employed in the first period, so the unemployment spell of some household members in the second period is shorter than or equal to a year. However, the analysis of treatment T10 uses a sample of households one of whose members is unemployed in the first period, and their unemployment spell is not restricted to be shorter than or equal to a year. Thus, those returning to employment after a long unemployment spell might not increase their cultural expenditure while waiting to see if the associated positive income shock is transitory or permanent.

2.5.3 Disaggregated Outcomes

Theoretically, the effect of unemployment on cultural expenditure ought to be different for the three components of cultural consumption: cinema and performing arts, museums and other exhibits, and books. While you can postpone the visit to a museum, that might not be feasible for cinema and the performing arts. Similarly, books can be bought or borrowed from libraries.

Accordingly, the effect on cinema and the performing arts could be larger. In addition, as the aggregate cultural expenditure is the sum of expenditure in the three categories, the effect on each of them should be smaller. Tables 2.7 - 2.9 present ATET estimates for the COP analysis disaggregated for the three cultural expenditure items. In general, the estimated effects tend to be smaller and less significant. When we look at the effects on cinema and the performing arts in Table 2.7, treatment T12 has a negative and significant effect on expenditure according to the SDID estimate, and on participation according to the trimmed matching estimates. As theory predicts, the implied income elasticity obtained from the SDID estimate for cinema and performing arts is larger than for the aggregate cultural expenditure estimate. However, none of the treatment effects are significant for museums and exhibitions and books.

2.6 Heterogeneous Effects of Unemployment

2.6.1 Treatment Effects by Demographic Characteristics

In this section, we assess whether the treatment effects are different across subsamples defined according to gender and educational attainment of the unemployed household member, using the same control group for all comparisons. Splitting the sample along these dimensions results in smaller treated groups, which precludes the analysis of treatment T12, so we focus just on treatment T01.

Table 2.10 presents SDID estimates of the ATET on income and cultural expenditure. The upper panel reports the unconditional analysis and the bottom panel the conditional on positive analysis. When we compare both panels, the results are very similar, with the only difference being that the treatment effect estimates on cultural expenditure are larger in the conditional case. The estimated ATET on expenditure is negative and significant when unemployment affects a man but fails to be significant when the affected household member is a woman. The treatment effect on income is always negative and significant and larger when unemployment affected a man than when the affected person is a woman. The results for educational attainment are reported in Columns (3) and (4). For this analysis, only two educational attainment groups are considered: tertiary education and below tertiary education. The estimated ATET on cultural expenditure is negative and significant when the unemployed household member achieved less than tertiary education, whereas it is not significant when the affected person achieved tertiary education. The estimated ATET on income is always significant and larger when unemployment hits someone with tertiary education. Additionally, participation in cultural consumption is not significantly affected in any case.

2.6.2 Treatment Effects Over the Business Cycle

The effect of unemployment on cultural expenditure might not be constant over the business cycle. Using Spanish aggregate data, Figure 2.5 shows how during periods when the number of unemployed people increases (decreases), the *fraction* of unemployed people covered by unemployment benefits decreases (increases). In addition, it also shows that unemployment duration mimics the ups and downs of unemployment figures. Therefore, a recession has the potential to generate more negative perspectives for the unemployed than a period of a booming economy.

Figure 3.2 plots the Spanish GDP and unemployment rate for our sample period, 2006-2015. Up to 2008 the Spanish economy experienced a period of positive growth, followed by a deep recession with negative growth up to 2013, and then positive growth during the last three years. Accordingly, we split the sample in two, pooling together waves of the EPF corresponding to years of positive growth (2006-2008 and 2014-2015) on the one hand and negative growth (2009-2013) on the other.

Table 2.11 and Figure 2.6 report conditional on positive ATET estimates for the positive and negative growth periods. Regarding participation, only treatment T01 has a significant and negative effect during periods of negative growth. Neither treatment T01 nor T12 have a significant effect on cultural expenditure during the positive growth periods. However, they do have a large and significant effect in the negative growth periods, with estimated falls in cultural expenditure as a result of treatments T01 and T12 of EUR 60.04 and EUR 143.28 respectively. These estimates are slightly above those obtained for the entire sample and are consistent with the idea of a heterogeneous effect on unemployment due to business cycles. Expectations can explain this marked difference between periods of positive and negative growth. Those who enter unemployment in a period of negative growth may have worse expectations regarding their future labor status than those who enter unemployment during a period of positive growth. The estimated impacts of treatments T01 and T12 on income are negative, statistically significant and similar to those corresponding to the entire sample period.

Finally, we compare each treatment impact with that of its analogous inverse treatment. Treatments T10 and T21 do not have a significant effect on cultural expenditure in positive or negative growth periods. Thus, households one of whose members enters unemployment reduce their cultural expenditure while the impact from the transition to work is not significantly different from zero. The response to losses is larger in absolute value than the response to gains, resulting in asymmetric consumer response, as suggested in prospect theory, e.g. Tversky & Kahneman (1981). However, the impact estimates of treatments T10 and T21 on income are significant and similar in magnitude with respect to the inverse treatments but in the opposite direction, both in positive and negative growth periods.

2.7 Conclusions and Discussion

We find that, on average, the first unemployed household member reduces cultural expenditure and income by EUR 24.90 and EUR 495.63, respectively, while the second unemployed household member has a negative impact on expenditure and income of EUR 30.16 and EUR 464.70. Combining these estimates, we assess that the income elasticity of cultural demand is around 0.62 for the first unemployed household member and 0.85 for the second.

To lend internal validity to our estimates, we run a placebo test, which shows that the estimation methods used perform fairly well, although the semi-parametric diff-in-diff method slightly outperforms the matching method. Covariate imbalance between treatment and control groups is somewhat reduced using matching methods, and more so when treated and untreated samples are weighted according to the weighting scheme used by the semi-parametric diff-in-diff method.

In addition, we perform an analysis conditional on participation in cultural activities. We find that an additional unemployed member barely affects household cultural participation. However, those households that do participate in cultural activities reduce their cultural expenditure by EUR 54.04 and EUR 122.41 for the first and second unemployed members, respectively. On average, income falls EUR 477.99 and EUR 559.00 for the first and second unemployed members. Thus, the impact on income is similar for the unconditional and conditional analysis, whereas we find considerable differences in estimated effects on cultural expenditure with respect to the unconditional analysis. As a result, the income elasticity of cultural demand is 0.65 for the first unemployed household member and 1.18 for the second.

In order to assess how heterogeneous our results are, we investigate how the impact of unemployment on cultural expenditure and income changes with gender and educational attainment. The reduction in cultural expenditure and income when the unemployed is a man is larger than when the unemployed household member is a woman. On the other hand, the reduction in cultural expenditure is larger, and the reduction in income smaller when the household member who becomes unemployed attained less than tertiary education.

Another contribution of this chapter is to provide evidence of the treatment effect of unemployment, and the inverse one, the treatment effect of employment. We find that inverse effects on income are symmetrical with respect to the direct ones. However, the inverse effects on cultural expenditure are not significant and therefore fail to be symmetrical.

A further arguably significant contribution is the analysis of how the impact of unemployment on cultural expenditure might vary with the business cycle. Thus, we repeat the analysis splitting the sample into those periods when the GDP is increasing, and those when the GDP is decreasing. Not surprisingly, our findings indicate that the reduction in cultural expenditure is larger in hard times, despite the fact that the effect of unemployment on household income does not fluctuate proportionally that much over the business cycle.

Traditionally, cultural consumption is linked to income, time availability, and prices. A feasible reading of our results could be that, in addition to the above traditional factors, consumer behavior might be driven by a taste for cultural goods and future income expectations.

Our finding that the effect of unemployment on cultural participation is insignificant could support the taste for cultural goods hypothesis. According to this hypothesis, cultural consumers build cultural capital as they consume culture over time and the higher the stock of cultural capital, the larger the utility they derive from cultural consumption. Thus, when exposed to a negative income shock, those with a large stock of cultural capital continue consuming culture, although spending less.

On the other hand, when unemployment affects household members with high educational attainment, they suffer larger reductions in income and smaller reductions in cultural expenditure as compared to the case when unemployment affects a household member with lower educational attainment. A potential mechanism explaining this result is that highly skilled persons consider that they have a higher chance of returning to their reference point, even after suffering a shock that deviates negatively from it.

Different expectations regarding future income might explain the finding of a changing effect depending on economic fluctuations. A negative income shock can lead to different expenditure responses depending on the perspectives regarding the future. Social expectations may also explain the differences across the business cycle. In a recession, people might moderate their consumption as a result of social norms.

Additionally, unemployment not only affects household income, but it also increases disposable leisure time. Therefore, it can be argued that the reduction in cultural expenditure as a result of unemployment is somewhat tempered as a consequence of more disposable time. However, Spanish full-time workers report large amounts of time disposable for leisure and personal care, see OECD (2013, 2015, 2017). Therefore, time does not seem to be a binding restriction in Spain as far as cultural consumption is concerned.

A caveat regarding using unemployment as the source of variation is in order. Our results may have low external validity. It might be risky to extrapolate the results obtained using unemployment as the source of variation to other settings. The change in cultural expenditure might be different depending on the type of income variation we analyze. For instance, the expenditure change might be different for lottery winners, those benefitting from particular subsidies or affected by a minimum wage rise. However, the population of households whose members experience unemployment is a particularly interesting group and often the focus of governmental policies. Therefore, our results have their own interest, independently of whether they can be extrapolated to other populations or not. Furthermore, the population to which the results can be generalized is fairly well defined: households with a member that experiences unemployment. Therefore, to improve on external validity, the estimation procedure suggested in this chapter might be applied to other populations experiencing other types of income shocks.

2.8 Figures

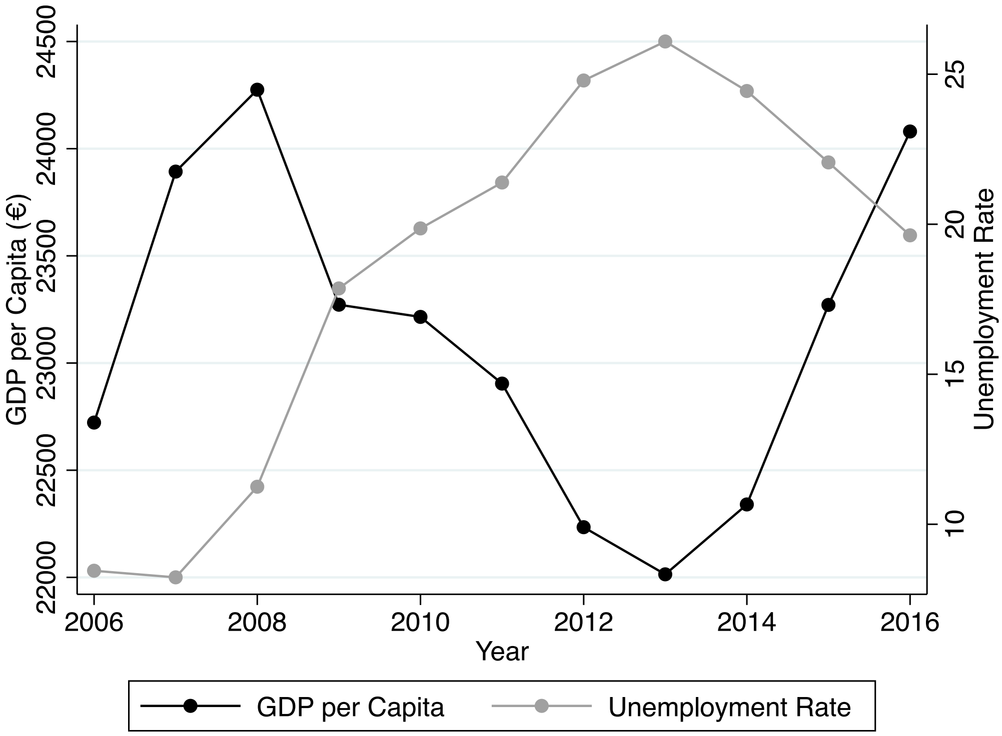

Figure 2.1: SPANISH GDP PER CAPITA AND UNEMPLOYMENT RATE

Notes: Figure shows the GDP per capita (black line), and unemployment rate (gray line, right scale). The vertical lines divide the sample in three sub-periods: the first and the third correspond to periods of positive growth and the second to a period of negative growth. Source: *Instituto Nacional de Estadstica*.

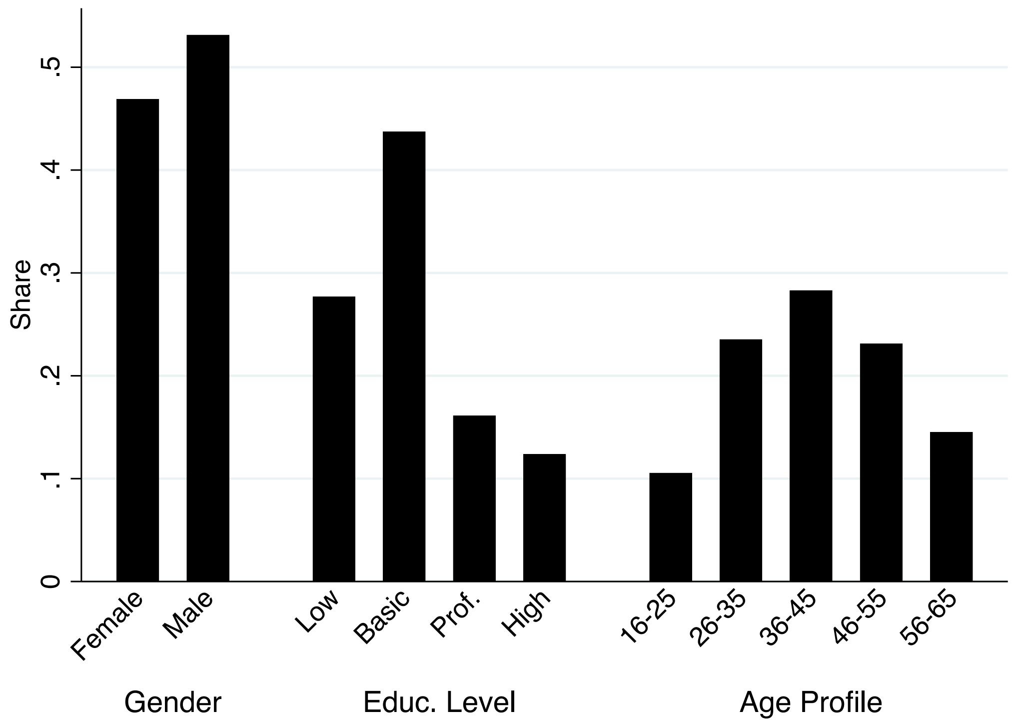

Figure 2.2: MAIN CHARACTERISTICS OF UNEMPLOYED MEMBERS
Notes: Figure shows the share of unemployed household members by gender, education attainment and age. Source: EPF waves 2006-2015.

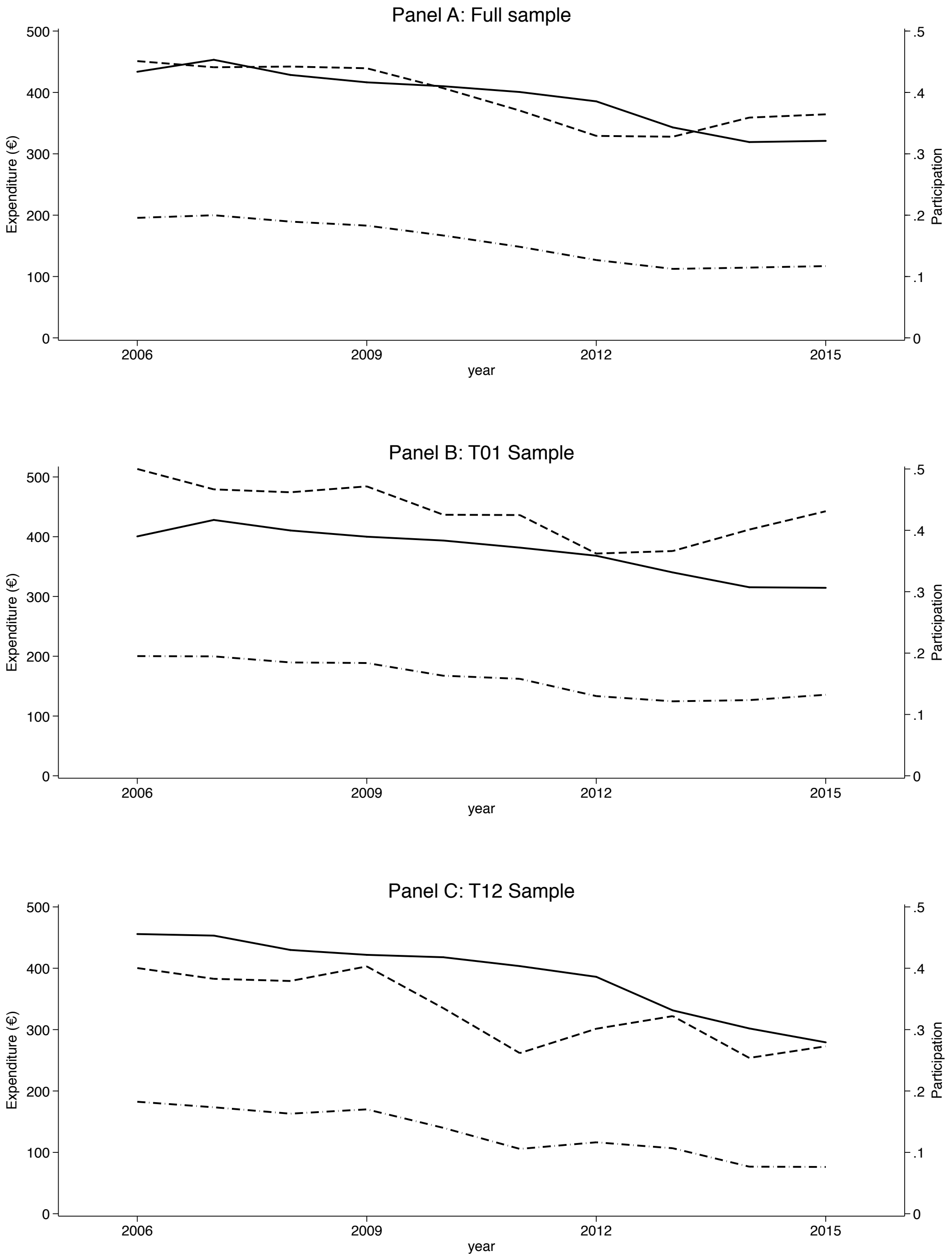

Figure 2.3: CULTURAL EXPENDITURE AND CULTURAL MARKETS PARTICIPATION

Notes: Figure shows the average cultural expenditure (dash and dotted line), average cultural expenditure conditional on reporting positive expenditure (dashed line), and share of households reporting positive expenditure (solid line, right scale).

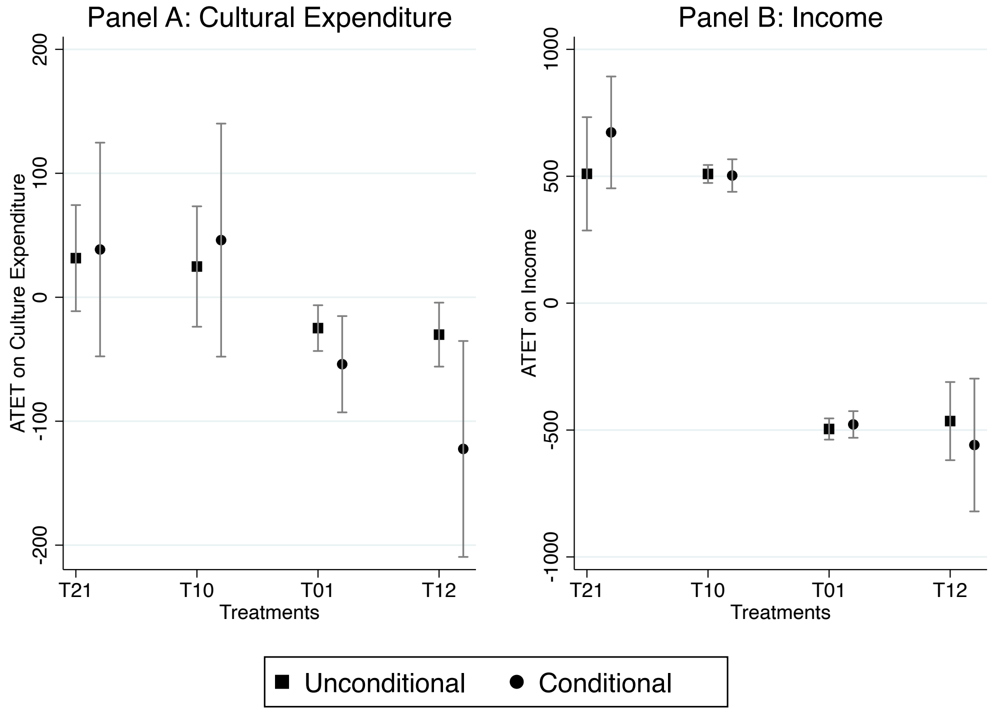

Figure 2.4: ATET ON CULTURAL EXPENDITURE AND INCOME

Notes: Figure shows the ATET estimates and 95% confidence intervals on Cultural expenditure and income for different treatments

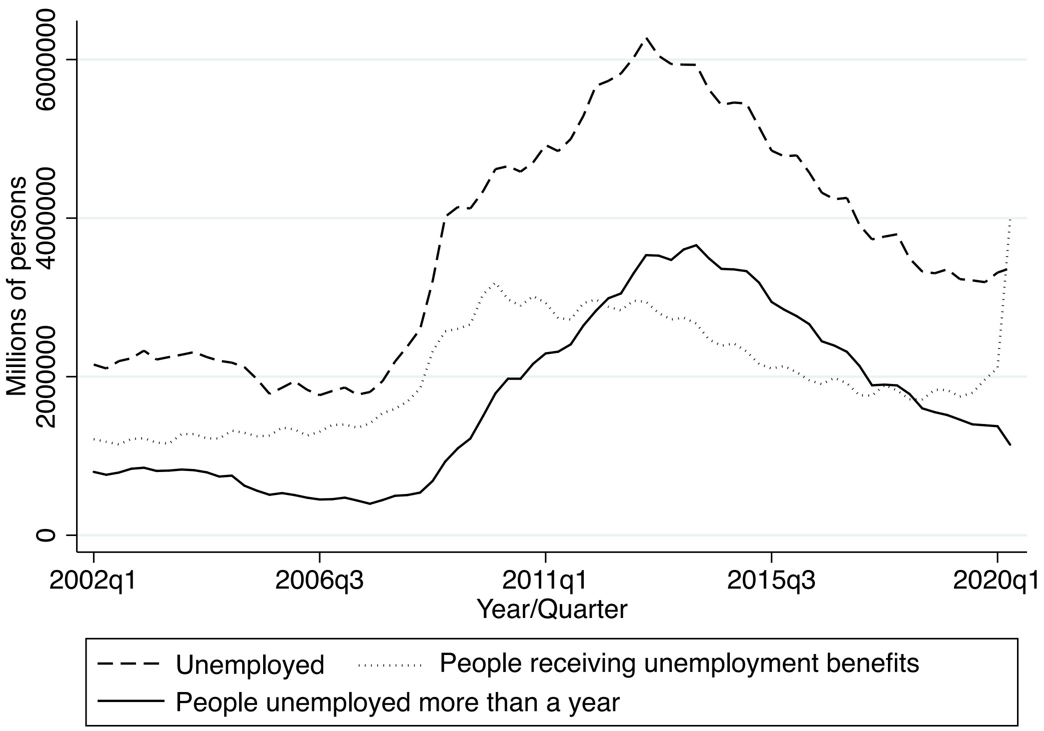

Figure 2.5: UNEMPLOYED INDIVIDUALS AND UNEMPLOYMENT BENEFITS

Notes: Figure shows the number of persons unemployed, receiving unemployment benefits and staying unemployed a year or more. *Sources*: Bank of Spain

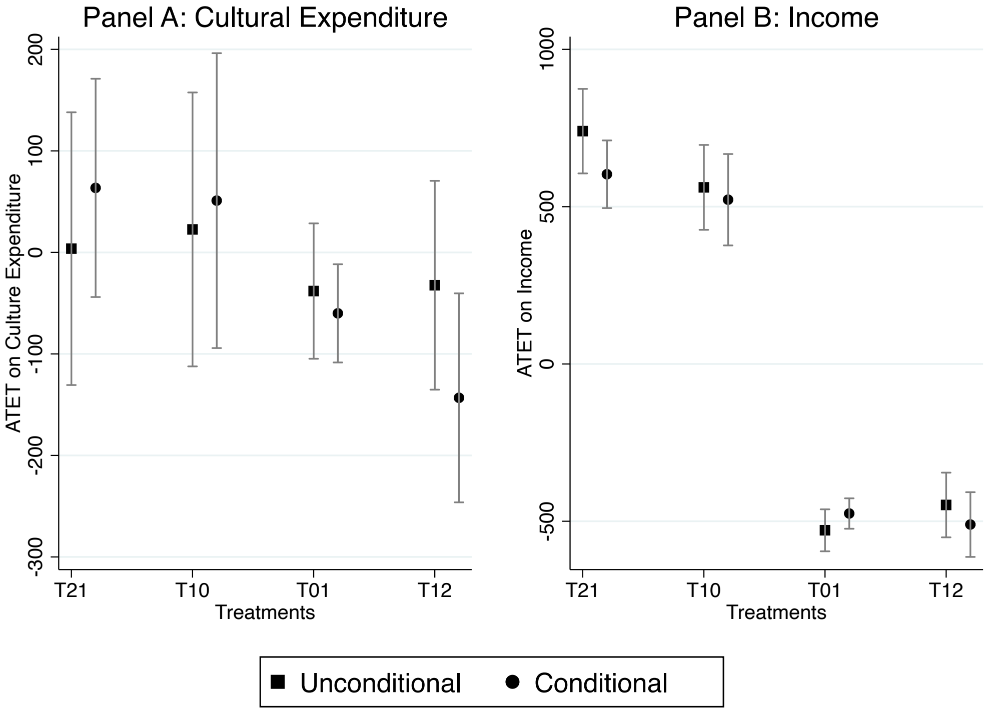

Figure 2.6: ATET ON CULTURAL EXPENDITURE AND INCOME

Notes: Figure shows the unconditional ATET estimates and 95% confidence intervals on cultural expenditure and income during periods of positive and negative growth.

2.9 Tables

Table 2.1: DESCRIPTIVE STATISTICS

	(1)	(2)	(3)	(4)	(5)	(6)
	Unweighted Sample				Weighted Sample	
	Mean	S. D.	Min	Max	Mean	S. D.
Panel A: Outcomes						
Cultural expenditure	117.4	443.5	0	15,272	118.32	456.79
Household income	1,962.51	1,297.59	0	14,065	1,862.47	1,250.70
Panel B: Number of household members by educational attainment						
Primary or less	0.391	0.731	0	6	0.373	0.709
Lower Secondary	0.735	0.908	0	8	0.676	0.887
Upper Secondary	0.482	0.736	0	5	0.460	0.722
Tertiary	0.621	0.820	0	6	0.609	0.806
Panel C: Number of household members in each age interval						
5 or less	0.148	0.430	0	1	0.147	0.426
From 6 to 15	0.302	0.634	0	8	0.235	0.563
From 16 to 25	0.288	0.599	0	6	0.267	0.584
From 26 to 35	0.264	0.562	0	4	0.327	0.616
From 36 to 45	0.418	0.691	0	3	0.425	0.684
From 46 to 55	0.457	0.713	0	3	0.382	0.382
From 56 to 65	0.355	0.647	0	3	0.292	0.593
65 or older	0.448	0.722	0	4	0.425	0.695
Panel D: Number of						
Females	1.379	0.824	0	8	1.27	0.831
Household members	2.680	1.243	1	16	2.499	1.259
Spanish citizens	2.450	1.315	0	16	2.220	1.334
Panel E: Town Size (Population)						
100,000 or more	0.379	0.485	0	1	0.418	0.493
50,000 to 100,000	0.114	0.318	0	1	0.128	0.334
20,000 to 50,000	0.155	0.362	0	1	0.164	0.370
10,000 to 20,000	0.105	0.307	0	1	0.089	0.285
less than 10,000	0.247	0.247	0	1	0.200	0.400

Notes: This table summarizes the descriptive statistics of the outcomes and covariates.

Table 2.2: THE EFFECT OF UNEMPLOYMENT ON CULTURAL EXPENDITURE AND INCOME

	(1) Semiparametric Diff in Diff	(2) Matching	(3) Trimmed Matching
Panel A: Treatment T01			
Cultural Expenditure	-24.8971	-22.9579	-17.0822
	(9.4244)	(10.1406)	(10.0788)
Income	-495.6369	-467.3977	-463.719
	(21.3268)	(15.7460)	(15.9086)
Implied Elasticity	0.6211	0.6067	0.4612
Treated	2,810	2,810	2,810
Controls	45,719	45,719	7,150
Panel B: Treatment T12			
Cultural Expenditure	-30.1574	-39.2673	-38.2420
	(13.1699)	(14.0119)	(14.0153)
Income	-464.7045	-393.5317	-396.3689
	(78.5842)	(31.4574)	(32.1215)
Implied Elasticity	0.8480	1.2142	1.1889
Treated	529	529	529
Controls	6,088	6,088	1,322

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.3: NORMALIZED DIFFERENCES

	(1)	(2)	(3)	(4)	(5)	(6)
		Treatment T01			Treatment T12	
	Full	Trimmed	Weighted	Full	Trimmed	Weighted
Panel A: Outcome						
Household Income	0.1065	0.1841	-0.0082	0.1109	0.3581	-0.0357
Panel B: Number of household members by age						
5 or less	0.1869	0.0945	-0.0014	0.0867	0.1374	0.0115
From 6 to 15	0.1341	0.0783	0.0000	0.1021	0.0494	-0.0164
From 16 to 25	0.2429	0.2377	-0.0181	0.3642	0.3307	-0.0417
From 26 to 35	0.4441	0.2215	-0.0137	0.2613	0.2484	-0.0690
From 36 to 45	0.2364	0.0813	-0.0076	0.0470	0.1153	-0.0181
From 46 to 55	0.1469	0.1036	-0.0061	0.0920	0.0912	-0.0412
From 56 to 65	0.0100	0.0442	-0.0056	-0.0307	0.0239	-0.0152
65 or older	-0.5875	-0.1976	0.0079	-0.2893	-0.0238	0.0072
Panel C: Number of household members by educational attainment						
Primary or less	-0.0985	0.0749	-0.0076	0.2313	0.2396	-0.1096
Lower Secondary	0.3066	0.1775	-0.0240	0.2997	0.1541	-0.0315
Upper Secondary	0.1910	0.1749	-0.0049	-0.0150	0.1170	-0.0051
Tertiary	0.0101	0.0163	0.0024	-0.1435	0.0552	0.0224
Panel D: Town Size (Population)						
100,000 or more	-0.0234	-0.0746	0.0025	-0.0947	-0.0911	-0.0142
50,000 to 100,000	0.0203	0.0304	-0.0007	0.0409	0.0686	0.0147
20,000 to 50,000	0.0668	0.0339	-0.0027	0.0135	-0.0166	0.0098
10,000 to 20,000	0.0146	0.0413	0.0018	0.0293	0.1294	0.0196
less than 10,000	-0.0552	0.2621	-0.0012	0.0375	-0.0268	-0.0187
Panel E: Number of						
Household members	0.4521	0.4047	-0.0282	0.4509	0.5392	-0.1158
Spanish citizens	0.2620	0.2621	-0.0094	0.2662	0.3194	0.0099

Notes: This table shows the normalized differences between treatment and control group.

Table 2.4: PLACEBOS AND THE EMPIRICAL SIZE OF TEST

	Test size	(1)	(2)	(3)	(4)
		Simulations			Average
		1	2	3	
Panel A: Treatment T01					
Semiparametric DID	0.05	0.0555	0.0460	0.0430	0.0481
	0.10	0.1035	0.0960	0.0970	0.0988
Matching	0.05	0.1140	0.1215	0.1310	0.1221
	0.10	0.1805	0.1975	0.2075	0.1951
Matching (Trimmed)	0.05	0.0560	0.0575	0.0595	0.0576
	0.10	0.1175	0.1125	0.1105	0.1135
Panel B: Treatment T12					
Semiparametric DID	0.05	0.0610	0.0520	0.0460	0.0530
	0.10	0.0965	0.1005	0.0965	0.0978
Matching	0.05	0.0940	0.0895	0.0880	0.0905
	0.10	0.1460	0.1470	0.1595	0.1508
Matching (Trimmed)	0.05	0.0595	0.0655	0.0745	0.0665
	0.10	0.1200	0.1265	0.1335	0.1266

Notes: This table shows the results from the randomized placebo test.

Table 2.5: CONDITIONAL ON POSITIVE RESULTS

	(1) Semiparametric Diff in Diff	(2) Matching	(3) Trimmed Matching
Panel A: Treatment T01			
Participation	-0.0078	-0.0077	-0.0039
	(0.0094)	(0.0103)	(0.0105)
Cultural Expenditure	-54.0379	-51.5423	-51.5585
	(19.8164)	(22.9025)	(23.4581)
Income	-477.9851	-477.6103	-482.8123
	(26.7517)	(25.8250)	(26.1266)
Implied Elasticity	0.6486	0.6398	0.6340
Treated	1,199	1,199	1,199
Controls	16,332	16,332	3,038
Panel B: Treatment T12			
Participation	-0.0469	-0.0351	-0.0400
	(0.0281)	(0.0243)	(0.0248)
Cultural Expenditure	-122.4067	-69.2272	-49.9349
	(44.4435)	(37.3595)	(34.4619)
Income	-559.0007	-417.2700	-423.9457
	(133.4800)	(55.2483)	(54.6502)
Implied Elasticity	1.1765	0.9346	0.6890
Treated	214	214	214
Controls	2,174	2,174	513

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.6: THE INVERSE TREATMENT EFFECT

	(1)	(2)	(3)	(4)	(5)
	Semiparametric Diff-in-Diff			Number of obs.	
	Participation	Expenditure	Income	Treated	Control
Panel A: Unconditional effects					
Treatment T10					
ATET		24.7913	509.0406	2634	6088
		(21.1647)	(18.0720)		
Treatment T21					
ATET		31.5589	509.593	522	948
		(21.8446)	(113.9126)		
Panel AB: Conditional on Positive effects					
Treatment T10					
ATET	0.0256	46.0967	502.7877	1102	2174
	(0.0115)	(47.9714)	(32.7197)		
Treatment T21	0.0069				
ATET	(0.0759)	38.5273	672.8269	213	333
		(43.9780)	(112.4061)		

Notes: Treatment T10: all households have one unemployed members in the first period and treated households have no unemployed member in the second period. Treatment T21: all households have two unemployed member in the first period and treated households have one unemployed members in the second period. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.7: RESULTS FOR CINEMA AND PERFORMING ARTS (COICOP 09421)

	(1) Semiparametric Diff in Diff	(2) Matching	(3) Trimmed Matching
Panel A: Treatment T01			
Participation	-0.0085	-0.0053	-0.0049
	(0.0092)	(0.100)	(0.0102)
ATET on Cultural Expenditure	-40.3331	-45.1856	-39.5688
	(21.3923)	(23.2220)	(23.6703)
Implied Elasticity	0.6727	0.7667	0.6657
Number of Treated	1,024	1,024	1,024
Number of Controls	13,367	13,367	2,595
Panel B: Treatment T12			
Participation	-0.0525	-0.0417	-0.0485
	(0.0276)	(0.0238)	(0.0243)
ATET on Cultural Expenditure	-97.8634	-57.6318	-71.6404
	(38.7464)	(36.4599)	(37.6890)
Implied Elasticity	1.5260	1.3966	1.6287
Number of Treated	188	188	188
Number of Controls	1,859	1,859	458

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.8: RESULTS FOR MUSEUMS AND OTHER EXHIBITS (COICOP 09422)

	(1) Semiparametric Diff in Diff	(2) Matching	(3) Trimmed Matching
Panel A: Treatment T01			
Participation	-0.0084	-0.0007	-0.0007
	(0.0071)	(0.0074)	(0.0075)
Cultural Expenditure	-16.7516	-12.9107	-18.9326
	(9.2986)	(13.0463)	(14.9207)
Implied Elasticity	1.6378	1.2688	1.7388
Treated	424	424	424
Controls	5,484	5,484	1,066
Panel B: Treatment T12			
Participation	-0.0186	-0.0020	-0.0011
	(0.0194)	(0.0179)	(0.0181)
Cultural Expenditure	8.5548	20.6519	21.3561
	(21.1059)	(25.3699)	(25.1468)
Implied Elasticity	0.9087	2.0583	2.1961
Treated	84	84	84
Controls	779	779	210

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.9: RESULTS FOR BOOKS (COICOP 0951)

	(1) Semiparametric Diff in Diff	(2) Matching	(3) Trimmed Matching
Panel A: Treatment T01			
Participation	-0.0045	0.0097	0.0104
	(0.0089)	(0.0097)	(0.0098)
Cultural Expenditure	-27.4434	-22.3028	-19.8587
	(15.5100)	(17.6038)	(18.3067)
Implied Elasticity	0.7106	0.5756	0.5192
Treated	908	908	908
Controls	12,122	12,122	2,281
Panel B: Treatment T12			
Participation	-0.0127	0.0038	-0.0041
	(0.0234)	(0.0224)	(0.0228)
Cultural Expenditure	-13.1709	8.1372	8.4317
	(25.6156)	(27.8241)	(29.8151)
Implied Elasticity	0.3708	0.2070	0.2098
Treated	175	175	175
Controls	1,685	1,685	426

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.10: TREATMENT EFFECTS BY GENDER AND EDUCATIONAL ATTAINMENT

	(1)	(2)	(3)	(4)
	Gender		Less than	Tertiary
	Female	Male	Tertiary	Education
Panel A: Unconditional SDID estimates				
Expenditure	-19.5593	-28.7341	-27.9588	-10.8837
	(13.6035)	(12.0461)	(9.6796)	(29.3237)
Income	-410.7074	-578.6057	-460.783	-818.9822
	(19.8771)	(30.7791)	(20.9064)	(147.875)
Treated	1,409	1,891	2,324	476
Controls	45,719	45,719	45,719	45,719
Panel B: Conditional SDID estimates				
Participation	0.0083	-0.0151	-0.0157	-0.0014
	(0.0128)	(0.0130)	(0.0104)	(0.0252)
Expenditure	-44.3235	-63.1160	-60.1175	-27.6342
	(28.5479)	(25.6552)	(21.0079)	(51.7558)
Income	-412.1394	-578.2103	-457.6792	-735.1726
	(30.6900)	(38.4354)	(30.9209)	(91.6317)
Treated	621	577	955	243
Controls	16332	16332	16332	16332

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. All covariates are used for bias adjustment. Standard errors in parentheses.

Table 2.11: TREATMENT EFFECTS OVER THE BUSINESS CYCLE

	(1)	(2)	(3)	(4)	(5)
				Number of obs.	
	Participation	Expenditure	Income	Treated	Control
Panel A: Treatment T01					
Positive growth	-0.0042	-38.0753	-528.7371	380	7135
	(0.0027)	(34.0234)	(44.2247)		
Negative growth	-0.0068	-60.0460	-475.4436	819	9197
	(0.0024)	(24.6810)	(37.8096)		
Panel B: Treatment T12					
Positive growth	-0.0159	-32.3673	-448.2591	58	837
	(0.0210)	(52.4672)	(82.9171)		
Negative growth	-0.0267	-143.2761	-519.5031	156	1337
	(0.0213)	(52.5230)	(103.9967)		
Panel C: Treatment T10					
Positive growth	0.0051	22.6242	561.2798	505	856
	(0.0087)	(68.8307)	(50.9550)		
Negative growth	0.0012	51.0126	522.0832	593	1422
	(0.0036)	(74.1058)	(40.7181)		
Panel D: Treatment T21					
Positive growth	-0.0160	3.6913	740.0343	83	116
	(0.1265)	(68.5405)	(115.6749)		
Negative growth	0.0058	63.5319	603.0085	115	251
	(0.0444)	(54.8530)	(165.7362)		

Notes: Treatment T01: all households have no unemployed members in the first period and treated households have one unemployed member in the second period. Treatment T12: all households have one unemployed member in the first period and treated households have two unemployed members in the second period. The matching estimates are obtained forcing exact matching in region and year for the T01 treatment, and only in year for the T12 treatment. Treatment T10: all households have one unemployed member in the first period and treated households have no unemployed member in the second period. Treatment T21: all households have two unemployed members in the first period and treated households have one unemployed members in the second period. Standard errors in parentheses. The positive growth period includes years 2006-2009 and 2013-2015, and the negative growth period years 2009-2012. Participation for T10 and Positive growth period estimated using a linear probability model in the SDID.

3

THE CONSEQUENCES OF LABOR MARKET SHOCKS ON MULTIPLE HEALTH MEASURES

3.1 Introduction

In a seminal paper, Grossman (1972) introduces the idea of health as capital stock. According to this view, health stock increases with health investment and decreases with health depreciation, as is the case for other kinds of capital stocks. In this setting, inputs for health investment are own time and market goods, and they are combined in a household production function to obtain health as output. Therefore, it comes as no surprise that there is extensive literature on the relationship between labor market outcomes and health status, given that labor markets may determine the availability of resources for health investment: own time and income. As unemployment affects time available and income, it serves as a good example of an economic shock that affects the amount of time available and capacity to acquire market goods due to the loss of labor income. Layoffs can be less frequent during economic growth periods, which should positively impact the capacity to purchase market goods, time availability can be lower, and individuals may experience lower health investments and lower health outputs. Hence, based on the fact that the income-health gradient is well established (see Deaton, 2006 and Chetty et al., 2016), one should expect that a negative income shock impairs health status. However, when the negative income shock comes from job displacement, the amount of time available is affected as well. This trade off is arguably one of the reasons the available empirical evidence finds inconclusive results for the relationship between health and economic conditions.

In this chapter, I test the causal relationship between labor market shocks and health investment inputs and outputs. More precisely, I look at how a micro-level shock, such as unemployment, and a macro-level shock, such as an economic downturn, affect a set of health indicators. To do so, this research utilizes different approaches. First, the analysis focuses on the effect of unemployment on self-reported standardized health status measurements. Then, it shifts to study how the impact of unemployment varies with the business cycle, especially after the financial crisis of 2007-2009. Finally, it focuses on the evolution of self-reported health trends by workers after an economic downturn, regardless of their employment status. Throughout, the long-run impact of the recession is assessed on multiple measures of health inputs and outcomes, focusing on differences between unemployed and employed individuals, and how the Great Recession affected these potential differences. Finally, I discuss how the effects of unemployment on self-assessed health are related to the effects on inputs for health production.

According to their nature, health inputs can be classified into two categories: active and reactive inputs. Active inputs are those related to improving health status. They can be adopted even when there are no health problems, and are oriented to increasing health capital stock. For instance, individuals may decide to start exercising to increase their health stock. Alternatively, reactive behaviors are those that individuals do in response to potential health problems. They are usually adopted after receiving a health depreciation signal (for instance, illness symptoms), so they may be understood as investments to compensate for health depreciation. Examples of reactive behaviors are visits to emergency rooms, hospitals, and medicine consumption. Nevertheless, some health inputs can be fuzzy. Active behaviors such as exercising can be considered within the reactive category as well (an individual may start exercising as a consequence of a health problem). Moreover, a visit to a doctor may be preventive, even when there are no health problems. Finally, I am able to measure the health output from this health investment using data from blood pressure and cholesterol levels.

The maintained hypothesis is that if unemployment actually affects health status, we should observe some impacts not only on self-evaluated health status, but also in objective health outcomes. Alternatively, potential differences in self-assessed health may be driven by a biased evaluation of health. The present study uses the case of Spain, one of the hardest-hit countries during the Great Recession, to estimate the effect of unemployment on a wide range of health indicators. The research strategy exploits the variation in labor market status at the individual level to estimate the causal effect of unemployment on self-reported health using a difference-in-differences design. It also exploits the vast increase in the unemployment rate to compare health trends during an economic boom and economic downturn in an event type regression.

The data used comes from Spanish and European health surveys. I use two types of variables, those that can be considered inputs for health investments, and those that can be considered outputs from those health investments. The first type includes risky health behaviors, such as smoking, alcohol consumption, and exercising. The second group focuses on medical care, such as visits to a doctor, hospitals, emergency rooms, specialists, and if medication is taken. Finally, the third group includes objective health measures such as cholesterol level and blood pressure.

Even though unemployed individuals report poorer health than employed individuals before losing their job, the evidence indicates that unemployment negatively affects the self-evaluation of health. The preferred specification shows a 0.025 standard deviation worsening in self-evaluated health, which accounts for around a 34 percent of differences in self-reported health between employed and unemployed individuals. Results are robust to different specifications. In addition, the evidence suggests that the impact may be more considerable after the recession period, but estimates are not precise enough. The results also indicate a general improvement of self-reported health during the economic expansion period and a worsening after the end of the Great Recession.

Further findings are related to inputs and outputs for health investment. Regarding disparities between employed and unemployed individuals, I find suggestive evidence of larger gaps in health indicators regarding the probability of going to emergency rooms, and having blood pressure problems. In addition, when I turn to analyze changes in health investment 2009 onward, results from an event study might suggest a larger probability of visiting a specialist, and reporting blood pressure and cholesterol problems for both employed and unemployed individuals. Therefore, the cause of the observed deterioration of self-reported health after the abrupt raise in the unemployment rate barely can be attributed to lower health investments.

The chapter makes several contributions to the literature regarding labor markets and health status. A number of papers have studied the relationship between the business cycle and health outcomes with mixed evidence. An example of this mixed evidence is the relationship between the business cycle and mortality. Some studies, as Ruhm (2000), van den Berg et al. (2017), or Tapia Granados & Ionides (2017), present evidence of the pro-cyclical trend of mortality in Europe during the Great Recession. However, analyzing a more extended period, Ruhm (2015) finds that mortality shifted from being pro-cyclical to being uncorrelated with macroeconomic conditions. Other authors find a negative relationship between the unemployment rate and health indicators. Results from Browning & Heinesen (2012) indicate a negative effect of unemployment on a broad set of health outcomes, including mortality, amongst other health indicators such as suicide rates or alcohol-related diseases. Sullivan & von Wachter (2009) also find that those males displaced from their jobs present higher mortality rates. These previous contributions provide dissenting evidence regarding the relationship between economic downturn and mortality. Other authors focused on self-evaluated health. Cylus et al. (2015) find that unemployed men are more prone to report poorer health, whereas Schaller & Stevens (2015) find that job displacement has adverse effects on self-reported health in the short run. Schwandt (2018) indicates that adverse wealth shocks affect self-reported health, amongst other health outcomes.

For the Spanish case, the evidence is fuzzy. According to OECD (2020b), life expectancy in Spain is the second-highest within reported countries, and avoidable mortality is relatively low. However, the share of people reporting poor health status was only slightly below the average of the OECD countries. Gonzalez & Rodriguez-Gonzalez (2018) find a lack of relationship between mortality rates in Spain and economic conditions, in line with OECD indicators. Nevertheless, Fornell et al. (2018) Urbanos-Garrido & Lopez-Valcarcel (2015) find a negative relationship between unemployment and self-evaluated health, albeit the latter do not find a negative impact of the recession on self-reported health. Finally, Calzon Fernandez et al. (2017) find that between 2007 and 2011, the share of unemployed individuals reporting good health increased. I extend the analysis using data from 2004 to 2016. According to Ruhm (2015), using a long period seems relevant, which indicates the potential flaws that may arise from using a short sample period to estimate the unemployment effect and how this effect changes during the recession period. Second, whereas some papers focus on self-reported health or mortality in Spain during the recession, less is known about the scarring effects of the Great Recession on health production inputs. I estimate the unemployment gap in these health determinants and objective health measures. The unemployment gap is defined as the overall difference between employed and unemployed individuals in a given outcome. Additionally, I estimate whether the economic crisis increases the gap. This chapter is the first to analyze risky health behaviors in Spain after the financial crisis of 2007-2009, to the best of my knowledge.

This chapter also relates to studies regarding labor market shocks and health investment. The empirical evidence regarding the relationship between economic fluctuations and risky health behaviors is inconclusive. Ruhm (1995) finds pro-cyclical alcohol consumption, whereas Mullahy & Sindelar (1996) find that alcohol consumption might increase the unemployment rate. Also, Ruhm (2005) finds a reduction in unhealthy behavior during economic downturns and an increase in time-intensive activities such as physical activity. In line with these results, Ásgeirsdóttir et al. (2014) obtain that the economic crisis reduced smoking behavior and heavy drinking in Iceland. Nonetheless, Deb et al. (2011) find that unemployment may increase the unhealthy behaviors (for instance, alcohol consumption) of those individuals showing unhealthy behavior before becoming unemployed. In line with these results, Marcus (2014) and Everding & Marcus (2020) find that unemployment increases the probability of smoking by 2 to 4 percentage points for both the unemployed and the spouse. Regarding the contribution to the literature of healthcare demand and health outputs, the results are also in line with those in Black et al. (2015), who find that males do not show changes in cholesterol or blood pressure after a job displacement, whereas for females unemployment affects cholesterol positively and blood pressure negatively. Finally, Schaller & Stevens (2015) document no effect of job displacement on these health indicators.

The rest of the chapter proceeds as follows. Section 2 introduces the Spanish Great Recession. A description of the data follows in section 3, with the empirical strategy detailed in section 4.

Section 5 provides the results, with an analysis of the heterogeneity of the results in section 6. Conclusions follow in section 7.

3.2 Background

To understand the consequences of economic shocks, we need them to be large enough to potentially affect the outcomes of interest. Therefore, using data from an economic downturn seems appropriate. Thus, in this chapter, I use the relevant case of the Spanish Great Recession. According to the International Monetary Fund (2006), before the Great Recession, in 2006 Spanish GDP was growing at 3.4 percent, while The Euro Area grew at 2.4 percent. However, the unemployment rate in Spain was 8.6 percent, above the 7.9 percent unemployment rate for the Euro area, and remarkably high for a country that was experiencing a period of expansion. This is proof that one of the macroeconomic features of the Spanish economy is a large structural unemployment rate. What is more, the nonlinearity of Okun's law in Spain is remarkable. According to Nebot et al. (2019), when the Spanish GDP variation per capita is below -0.4 percent, the estimated Okun's coefficient is -1.04. However, when variations in the GDP per capita is above that threshold, the coefficient is -0.29. The Spanish economy destroys employment very quickly during the recessions, taking long periods to recover previous employment levels, and the Great Recession was not an exception. Figure 3.1 shows the relationship between the GDP growth per capita and the growth of the unemployment rate for the period 2004-2016. Figure 3.2 shows the evolution of the Spanish GDP per capita growth rate, and the unemployment growth rate for the same period. Spanish unemployment grew at very high rates in the early stages of the recession, and it was growing until 2014 when Spain experienced the first negative unemployment growth since 2007. However, GDP per capita was less volatile after the first sharp drop happened during the period 2006-2009. Thus, in 2016, with the GDP per capita almost at pre-crisis levels, the unemployment rate was still slightly below 20 percent. Therefore, the Spanish Great Recession seems to be an optimal framework to learn about the consequences of unemployment and economic downturn.

The economic downturn had consequences that potentially may affect health. At the aggregated level, according to OECD, public expenditure on health experienced a reduction during the period 2009-2012. The reduction might seem negligible, less than 1 percent, but it is huge when compared with the 22 percent average increase during 2004-2009. Regarding health services, Spain provides universal coverage, which means that all workers, regardless of their labor market status, are entitled to be treated for any health problem by the public health service. This feature of Spanish health policy dismisses the lack of health coverage after job displacement as a potential explanation driving health impairment.

At the individual level, both the increase in the unemployment rate and the drop in the GDP per capita are expected to affect the amount of time available and the capacity to acquire market goods. The increase of own time is straightforward since unemployed individuals do not have to fit into a determined schedule. Regarding how market goods can be affected, unemployment benefits do not fully repair the labor income drop. After involuntary job displacement, workers in Spain are eligible for unemployment insurance, provided that they were employed for at least one year during the six years before the displacement. Unemployed individuals are entitled to four months of benefits for each worked year, with a maximum length of two years receiving the unemployment benefits. Regarding the amount received by unemployed individuals, they receive 70 percent of the average salary received during the last six months before the job displacement. After the first six months, this amount was reduced to 60 percent before 2012. After a reform, forced by the economic situation, the replacement rate was reduced to 50 percent.

Consequently, displaced individuals face a reduction in their capacity to purchase market goods as a consequence of the income reduction that follows a spell of unemployment (see the second chapter of this thesis or Ganong & Noel, 2019). Thus, if some of these goods are inputs used to produce health, job displacements might negatively affect health production. Nevertheless, the impact of this variation in time and income levels on health can be fuzzy. For instance, the extra spare time can be used to exercise but also to engage in risky health behaviors more frequently. Regarding market goods, individuals may decide to reduce expenditure on some dangerous health goods, such as tobacco or alcohol, but it can also affect the expenditure on health production inputs, like healthy food. For instance, in Gardeazabal & Polo-Muro (2020), we find that tobacco, alcohol, and drugs are the only consumption items not negatively affected by job displacements.

In Figure 3.3, I show the amount received by unemployment insurance recipients in the sample. The amount was increasing until 2012 and then started to decrease. Moreover, the share of beneficiaries also starts to decrease during the recession. Finally, the public per capita expenditure on health services was increasing until 2009. Then, it decreased, with the lowest expenditure registered in 2013. Nonetheless, and despite the economic downturn, overall, self-evaluated health improved during the period 2004-2017. However, in 2011, self-evaluated health status started worsening. In Figure 3.4, I show the evolution of self-reported health by employment status.

Therefore, as Ridley et al. (2020a) remark, a good policy design to mitigate externalities caused by joblessness requires evidence regarding such an event's consequences. Examples of a policy that could mitigate the potential effects of recession are given in Schwartz (2013), and Moyen & Stähler (2014), who propose a business cycle dependent unemployment insurance. This policy seems appropriate considering the results in Kuka (2020), who shows that unemployment benefits improve self-evaluated health during recession periods. Dow et al. (2020) evaluated minimum wage policies and Earned Income Tax Credit (EITC), finding that they may reduce non-drug suicides.

3.3 Data and Descriptive Statistics

I use data from three different sources: The Spanish Survey of Income and Living Conditions (SILC), National Health Surveys (NHS) and European Health Surveys (EHS).

3.3.1 Survey of Income and Living Conditions

To examining the causal effect of unemployment on self-evaluated health status, I use The Spanish Survey of Income and Living Conditions (hence SILC). This survey covers the pre-recession years, the Great Recession, and the post-recession period (2004-2017). Each yearly wave consists of a sample of around 16,000 households, each household remains in the survey for four consecutive years, and all household members are interviewed. SILC contains detailed information about monthly labor market status and socioeconomic characteristics such as age, gender, educational attainment, marital status, income, and occupation according to the International Standard Classification of Occupations (ISCO). Furthermore, respondents report their self-evaluated health on a scale from 1 to 5 (being 1 the best possible health status, and 5 the worst), as well as their labor market status. The empirical analysis is carried out for a subsample of employed (regardless of the type of contract) and the unemployed living in a household with no other member unemployed.

3.3.2 Spanish and European Health surveys

The Spanish Health Survey (SHS) is conducted by the Spanish Health Ministry every five years. This survey is a nationally representative sample of around 20,000 men and women. The analysis uses waves for 2003, 2006, 2011-2012 (2011, henceforth), and 2016-2017 (2016, henceforth). European Health Survey (EHS) is designed by Eurostat and conducted for Spain by the Spanish Statistical Bureau (INE). I use the 2009 and 2014 waves. The EHS includes less information than the SHS. For instance, data for the year 2009 does not specify the unemployment length for unemployed individuals.

Health surveys consist of a two-part questionnaire: one focused on a randomly selected individual in each household, and the other questionnaire covering the characteristics of other household members. The former reports rich information on age, gender, education, marital status, labor market status, duration of this status, occupation, type of relationship with the employer, region, size of the town of residence, and household size. The second questionnaire covers the other household members and provides detailed information about each member's age, education, and labor market status. The empirical evidence reported below focuses on those household members who report being employed, and also on those who report being unemployed for less than two years. The survey also focuses on a large set of health issues. The outcomes of interest are classified into three categories: First, risky health behaviors, including the extensive margin of smoking, alcohol consumption, and exercising. Second, medical care, focusing on the extensive margin of visiting doctors, specialists, hospitals, and the emergency room. Finally, objective health measures, covering questions and measures about blood pressure and cholesterol levels.

3.3.3 Descriptive Statistics

Table 3.1 contains the summary statistics for the main characteristics: age, gender, and education by labor market status for SILC in Panel A. In addition, I present in Panel B the summary statistics for the SHS and EHS after pooling it (Health Surveys henceforth). For employed individuals, the average person is 42-43 years old for both data sources. The share of males is around 0.55 in both cases as well. However, the share of individuals with at least a college degree is 0.4 for the SILC and 0.2 for health surveys. Unemployed individuals are, on average, 39 years old, and the share of males ranges from 0.48 to 0.54. The share of individuals with at least a university degree in the SILC and the health surveys is 0.23 and 0.12, respectively. Table 3.2 shows descriptive statistics for the health outcomes and income by labor market status. Employed individuals report better health status and higher income. Panel B shows that employed individuals are less likely to smoke and more likely to consume alcohol and exercise. Panel C displays the descriptives for medical attendance. Outcomes for employed individuals are always lower, but for the likelihood of visiting a specialist. Finally, Panel D shows that the objective health measures are slightly higher for employed than for unemployed individuals.

3.4 Empirical Strategy

3.4.1 Identification Strategy and Models for the Effect on Self-Evaluated Health

My primary analysis focuses on the causal relationship between unemployment and self-perceived health. First, I analyze the response of the outcomes of interest when workers face changes in their employment status. In a canonical difference-in-differences setting of two periods and two groups, I compare the changes in self-reported health of those who transition from employment to unemployment with the changes in the self-reported health of those who remain employed. Despite the fact that I observe four periods for almost all individuals, I do not exploit this particular feature of longitudinal data in some cases. An example is when we observe an individual employed in the first wave who transits to unemployment in the second one, and remains unemployed for the rest of the periods (waves three and four). In this case, I am only interested in waves one and two, when the transition is observed. Nonetheless, in subsection 4.6.2, I show the results using only those individuals that are displaced in period three and those that are displaced in period four, allowing for exploitation of the longitudinal nature of the data but facing a considerable reduction in observed number of unique individuals due to the further restrictions imposed.

To estimate the effects of unemployment, it is necessary to identify the treatment status. I propose different restrictions to define unemployment status. The first definition selects as treated individuals those who, from the first to the second period, transit from employment to unemployment (T0). In a second definition (T1, the preferred one), I impose further restrictions. Before the first period, all the individuals have to report being employed in the previous twelve months. This restriction ensures that the pre-treatment health status, when all the observations report being employed, is not contaminated by some unemployed months during the previous year. The third definition (T2) restricts the treatment group to those individuals who lose their jobs in the last six months before the treatment period. As the unemployment effect might be heterogeneous across time, this restriction allows me to catch the short-time effect. In addition, I analyze whether the impact of unemployment is exacerbated when job displacements happen during a recession.

I estimate the effects of unemployment using a difference-in-differences design, which exploits the individual variation in labor market status over time. The identifying assumption behind this strategy is that reported health by unaffected workers provides a good counterfactual for the self-assessment of the health status that unemployed members would report in the absence of treatment. The main problem posed to estimate unemployment's causal effect is that individuals are not randomly selected to lose their jobs. Even if I adjust for observable characteristics, observed differences in the outcome could exist before the treatment. To reduce the potential bias from pre-recession differences, I control for auto-evaluated health differences before the treatment. Thus, I estimate the differences in reported health status between unemployed and employed individuals. Thus, in the main specification, I estimate the following equation

$$Y_{iprt} = \alpha_0 + \alpha_1 U_{iprt} + \alpha_2 P_{iprt} + \alpha_3 U_{iprt} P_{iprt} + \eta X_{iprt} + \zeta_r + \lambda_t + u_{iprt} \tag{3.1}$$

where Y_{iprt} represents the outcome of interest for individual i in period p whose residence regions is r, and is interviewed in year t . U_{iprt} indicates if individual i belongs to the group of individuals displaced from their jobs in the post-treatment period or not. When self-reported health is the outcome of interest, it is a group indicator that takes value equal to one if individual i belongs to the treatment group, and zero otherwise. When income is the analyzed outcome, U_{iprt} is a variable that measures the number of months that each treated unit was unemployed. P_{iprt} is a dummy variable that takes value one if the period is the post-treatment period, and zero otherwise. X_{iprt} represents a set of covariates that includes controls for age, educational level, age, occupation, marital status, and household composition. ζ_r and λ_t are region and year fixed effects. The parameter of interest is α_3, which captures the effect of unemployment on self-reported health or income.

In addition, I am interested in analyzing the long-term effects of the recession on self-reported health. To analyze how the effect changes after 2009, I estimate the heterogeneity of the estimated effect using a triple difference model given by equation

$$Y_{iprt} = \alpha_0 + \alpha_1 U_{iprt} + \alpha_2 P_{iprt} + \alpha_3 R_{iprt} + \alpha_4 U_{iprt} P_{iprt} + \alpha_5 R_{iprt} U_{iprt} + \alpha_6 R_{iprt} P_{iprt} + \alpha_7 R_{iprt} U_{iprt} P_{iprt} + \eta X_{iprt} + \zeta_r + \lambda_t + u_{iprt} \quad (3.2)$$

where R_{iprt} takes value one if individual i in period p was surveyed after 2009 and 0 otherwise. In this setup, α_7 measures how much the impact of unemployment changed when the treatment happened after 2009. Finally, I run an event study type regression to estimate how the self-evaluated health of workers changed after 2009. Using the whole sample of employed and unemployed individuals, I run the following regression

$$y_{iprt} = \delta_0 + \alpha_1 U_{iprt} + \sum_{j=2004, j\neq 2009}^{j=2016} \beta_j U_{iprt} 1(t=j) + \eta X'_{iprt} + \zeta_r + \lambda_t + \upsilon_{iprt} \quad (3.3)$$

where β_j captures the differential effect of unemployment with respect to the reference year 2009.

3.4.2 Identification Strategy and Models for the Effect on Health Investments

The cross-section nature of Spanish Health Surveys makes it impossible to use equation 3.1 and 3.2 to estimate the impact of unemployment on the set of health measures. Therefore, I estimate a repeated cross-section difference-in-differences model to compute if the differences between employed and unemployed individuals before and after 2009 remain constant or they change.. In particular, I estimate the following equation

$$y_{irt} = \delta_0 + \delta_1 U_{irt} + \delta_2 R_{irt} + \delta_3 U_{irt} R_{irt} + \delta_4 X_{irt} + \zeta_r + \lambda_t + \mu_{irt} \quad (3.4)$$

where the parameter of interest, δ_3, recovers the effect of the economic downturn on the gap between employed and unemployed individuals regarding the outcome of interest. To adjust for potential differences captured by the parameter of interest but arising from differences in trends before the treatment period, we extend this model adding specific linear trends. In addition, I estimate equation 3.3 using data from health surveys (thus, j now goes now -2 to 3).

3.5 Results

3.5.1 Results for Self-Reported Health and Income

3.5.1.1 Self-Reported Health

Table 3.3 presents the main results for self-evaluated health analysis. Reported coefficients show the unemployment effect on self-reported health and cross-differences between employed and unemployed individuals for the same outcome. I report two different sets of estimates for each treatment definition. Columns 1 and 2 report the results from the difference-in-differences model, while columns 3 and 4 display the estimated coefficients from the triple differences design. For each model, results are reported using covariates at the individual level and then adding covariates at the household level, respectively.

Panel A of Table 3.3 gives the estimates for the treatment T0. Results in columns 1 and 2 indicate that unemployment causes a worsening of the reported health slightly below 0.02 standard deviations. Differences between employed and unemployed individuals are 0.07 standard deviation, which means that the impact size is around 28 percent. According to results in columns 3 and 4, job displacement after the abrupt deterioration of the labor market affects self-reported health to a greater extent than before the economic downturn (0.03 standard deviations), but this estimate turns out to be insignificant. In Panel B, the preferred specification is presented. Baselines estimates in columns 1 and 2 suggest that unemployment impairs health evaluation by 0.025 standard deviations, in line with results from Panel A. This effect accounts for approximately 33 percent of the differences between employed and unemployed individuals. After the sudden increase in the unemployment rate during the period 2007-2009, the impact is around twice as bigger as the estimated impact in columns 1 and 2, around 0.05 standard deviation, albeit the coefficient is imprecisely estimated. According to this model, differences between employed and unemployed individuals are up to 0.10 standard deviations. Thus, the estimated effect accounts for around 50 percent of the cross-section differences. Panel C looks at the effect of unemployment in the short-term. Results in columns 1 and 2 are in line with the results in Panel A and Panel B, but they present larger standard errors. Differences between employed and unemployed individuals are also slightly smaller, around 0.06 standard deviation lower.

Having provided evidence of the impact of unemployment on self-evaluated health, I now present estimates of how the own evaluation of self-evaluation of health changes after an economic decline. Figure 3.5 reports estimates from the event study design. Findings suggest a negative trend for self-reported health over the period 2004-2017, which is associated with improved self-evaluated health. However, after 2012, self-reported health presents an impairment, and in 2017 is around 2010 levels. Finally, even though unemployed individuals present lower health assessment, the gap between employed and unemployed individuals remains constant during almost the whole period. Nevertheless, after 2012, the gap is slightly higher than in previous years. These results are in line with the visual evidence from Figure 3.4, where we can see that self-evaluated health follows a “v” shape path during the analyzed period. If we consider these results as a consequence of the economic decline, it might be possible that there is a lag between the beginning of the recession and the observable effects on health status.

3.5.1.2 Income

The estimates in Panel A from Table 3.4 show that unemployment (T1) reduces income by around 400€ for each unemployed month, in line with findings in Gardeazabal & Polo-Muro, 2020. Interestingly, unemployed individuals report lower yearly income even before the job loss (around 1,700€ less). A potential explanation for these results is that, if health nudges productivity as Jack (2012) indicates, individuals with poorer health would be less productive, and this fact would be reflected in their labor income. Results in Panel B show that after 2009 income was around 250 € lower as a consequence of unemployment in comparison with the period 2004-2010. However, also wages were, on average, around 600€ lower after 2009. As the unemployment benefits are a fraction of the wage during the last six months before the job displacement, the higher the earnings before the layoff, the larger the drop. Thus, higher earnings during the expansion period lead to larger income drops after job displacements. Finally, according to the data, the average number of months that treated individuals were unemployed during the period 2004-2017 was almost three months, and up to five months after the recession period.

3.5.2 Results for Health Determinants

In the following subsections, I provide evidence of the relationship between unemployment, recession, and a set of health inputs and outputs.

3.5.2.1 Risky Health Behaviors

According to the results in Panel A of Table 3.5, unemployed individuals are more likely to smoke and exercise, but less to consume alcohol. When we turn to results in Panel B, adding specific linear trends to capture pre-recession trends indicates that the economic downturn reduces the smoking of unemployed individuals by around 4 percentage points. In contrast, the effect on exercising is up to 7.3 percentage points, which means that, after the 2009 period, unemployment positively affects physical activity.

Figure 3.6 presents event study estimates. Panel A shows the differences between employed and unemployed individuals. Regarding the probability of smoking, we find no difference before and after 2009. This evidence goes against that obtained from the difference-in-differences model, and relies on how data from 2009 is managed. When I examine alcohol consumption and physical activity trends, the coefficients measuring the differences grow during the pre-treatment period. Then, the time path of the differences becomes flatter after the rapid increase in the unemployment during the 2007-2009 period. Using the information given by pre-trends as an estimate of what would have happened in the absence of the recession, the trends pattern indicate an improvement of these outcomes from 2009 onwards. Panel B plots the trends for the likelihood of engagement in risky health behaviors. I find a decline in smoking, probably driven by the pre-recession trend. Regarding alcohol consumption and exercising, findings suggest an increasing probability before 2009, that becomes constant after 2009. These changes in trends can be interpreted as positive for health in the case of alcohol consumption and negative in the case of physical activity.

3.5.2.2 Medical Care

I present the result from the difference-in-differences model and event study design for medical outcomes in Table 3.6. For the standard difference-in-differences design presented in Panel A, I find that unemployed individuals are more prone to consulting a doctor, going to hospital, being involved in emergencies, and taking medicines. By contrast, the only outcome that seems to be affected by unemployment after 2009 is the probability of taking medicines, lowering it by 2 percentage points. I report results for the difference-in-differences with specific linear trends in Panel B. I find that unemployed individuals are more prone to hospitalizations and take medicine (2.4 and 4.2 percentage points, respectively), which is in line with lower health quality. However, after the financial crisis, job displacements seem to reduce the probability of emergency room visits by almost 6 percentage points, while the remaining outcomes are unaffected. Exploring the results from the event study design, in Panel A of Figure 3.7 I show how the differences between employed and unemployed individuals change over time. Results suggest an increase in consulting a doctor and a specialist after 2009 compared to the period before 2009. Thus, we observe a change in the downward trend. Nonetheless, as regarding consulting a doctor, the increase is only observed in 2011, and then the gap vanishes. On the other hand, we observe how the increasing gap in hospital visits becomes flat after 2009. Regarding medicine intake, the downward trend observed before 2009 continues in 2011 and 2014. Finally, panel B suggests a drop in the likelihood of consulting a doctor. I also find a flattening trend in the probability of consulting a specialist and taking medicines after the sharp increase in the unemployment from 2007-2009, quite opposite of the increasing likelihood observed before 2009.

3.5.2.3 Objective Health Measures

While the outcomes analyzed so far are inputs for health production, objective health measures are an indicator of the actual health status of individuals. Consequently, they can be considered as outputs from health production. Cholesterol and blood pressure are considered as objective health measures, as is explained in Arni et al. (2020), and are potentially correlated with socioeconomic status as Cutler et al. (2012) indicate. I use three measures for each of these two outcomes. First, a binary indicator for any cholesterol or blood pressure problems. Second, a binary indicator for any problem in the previous 12 months, and finally, if a doctor diagnosed the above mentioned health problems. One potential caveat is that for 2003 I only have data for the first measure in each type of outcome, while I do not have data for cholesterol problems in the 2009 wave.

The results in Panel A of Table 3.7 suggest no difference in cholesterol and blood pressure problems between employed and unemployed individuals. The only difference I observe is that unemployed individuals have a 0.62 percentage point higher probability of being diagnosed with blood pressure problems, which is negligible in magnitude. Adding treatment specific linear trends leads to finding that unemployed individuals are more prone to report blood pressure problems (between 1.9 and 2.4 percentage points of difference). Results from the event study design are shown in Figure 3.8. As the survey conducted in 2009 does not provide information for cholesterol problems, we use 2006 as the reference year for this outcome. Panel A shows that the differences in both blood pressure and cholesterol problems barely change during the analyzed period. Nonetheless, both outcomes exhibit a negative trend before 2009, which indicates that the differences are smaller. Moreover, blood pressure problems increase in 2011, but they start to vanish again in 2014. On the other hand, cholesterol problems might be showing a change in the trend after 2011. Finally, according to Panel B results, both health measures show a concave trend, compatible with a positive change after 2009, bending the growing trend observed before the recession.

3.5.3 Robustness Checks and Placebo Tests

3.5.3.1 Robustness for self-reported health

As discussed earlier, the main analysis relies on a canonical difference-in-differences design with two periods and two groups. In this section, I exploit the longitudinal nature of the data, using different approaches. The identification strategies still rely on comparing the changes in self-evaluated health between those individuals that are displaced from their jobs with those who remain employed. Therefore, I isolate a subsample with those observations appearing four times in the survey and whose labor market status is always either employed or unemployed. The first identification relies on selecting as a control group the observations employed over the four periods, while those who transit to unemployment in the last period compose the treated group. This identification also allows me to run a pre-treatment parallel trends test. The second identification strategy compares individuals who transit from employment to unemployment (or the other way around) with those whose labor market status does not vary during the four periods. The last identification exploits the different timing in transition to unemployment, but using as treated units those who do not exit that status after being unemployed. In addition, I use the same identification strategies to estimate the effect on income. As in the primary analysis, the number of unemployed months substitutes the binary indicators for labor market status. These three subsamples are used to run the next fixed effect model

$$y_{iwrt} = \alpha_i + \alpha_1 U_{iwrt} + \alpha_2 W_{iwrt} + \alpha_3 U_{iwrt} W_{iwrt} + \eta X_{iwrt} + \lambda_t + \zeta_r + \varphi_w + e_{iwrt} \quad (3.5)$$

where W_{iwrt} adjust for the survey wave of individual i in region r and calendar year t, α_i and φ_p control for individual and period fixed effects, respectively. Estimated coefficients for effect on self-perceived health are presented in Column 1 of Table 3.8. These results range from 0.02 to 0.05 standard deviation, in line with the results obtained from the canonical difference-in-differences design. Column 2 shows the income losses for each month unemployed. Results range from 276€ to 412€, slightly below the results obtained in the main analysis. In Panel C, I present the pre-treatment trends test, using the first identification strategy. The test for self-evaluated health shows a negligible and statistically insignificant effect, while the test for income trends shows a large (between 30 and 43 percent of the estimated effect) but statistically not significant result.

3.5.3.2 Pre-Treatment Tests

Identifying the causal effect of a treatment using the difference-in-differences design relies on the parallel trends assumption, which assumes that outcomes from both employed and unemployed individuals would present common trends in the absence of the treatment. A violation of this assumption would mean that the difference-in-differences design is capturing something else apart from the treatment effect, and the results would be biased. Even when I cannot test the parallel trends assumption, I can test if both employed and unemployed exhibit the same trends for the period before the treatment. Observing parallel trends before the treatment would lend credibility to the identifying assumption. To test it, the general equation that I estimate is

$$y_{iprt} = \pi + \pi_1 T + \pi_2 U_{iprt} + \pi_3 U_{iprt} T + \eta X_{iprt} + \lambda_t + \zeta_r + e_{iprt} \tag{3.6}$$

where T is a linear trend. Parameter π_3 captures if unemployed individuals show a different trend in a given outcome. The estimation of this equation over the pre-treatment periods allows checking pre-treatment parallel trends. To run this regression, I select from the main analysis those observations that appear four times. Then I define as treated observation those employed in the first three periods and then transit to unemployment in the fourth period. The control group is composed of those observations that are employed during the four periods. Therefore, this procedure allows me to estimate if control and treated units exhibit parallel trends over the first three periods.

In a second approach, I select those observations that appear three times, which leads to an increase in the number of disposable units. The control group is defined in the same way as before, while the treated group is defined as those observations employed in the first two periods and unemployed in the last one. Using this subsample, I run equation 3.1, but defining a pseudo treatment period occurring from period one to two instead of the actual treatment period, which occurs from period two to three.

Next, I verify if differences in health determinants between unemployed and employed individuals after 2009 come from divergent paths arising before the economic downturn. Specifically, I run equation 3.6 for the period 2003-2009, which tests if employed and unemployed individuals exhibit parallel trends in each outcome before the Great Recession.

Table 3.9 presents the results for self-reported health. As I trimmed the original sample, I first show the effect of unemployment on the new subsample. Findings are in line with those obtained in the primary analysis, even when the new estimations exhibit large standard errors. The pre-treatment placebo test results are close to zero (0.003 and 0.006), which indicates that differences in self-reported health before the treatment were negligible. When I look at results for common trends in the pre-treatment period, I find a small and negative coefficient, indicating that both groups exhibit almost parallel trends. Moreover, the differences between employed and unemployed individuals might be narrowing before the spell of unemployment, which indicates that I am not capturing some pre-trends in the principal analysis.

Table 3.10 shows the results for the pre-recession common trends test. I find a violation of pre-treatment trends test for some outcomes, such as the probability of smoking, carrying out physical activity, or having an appointment with a specialist. Nevertheless, the main results are presented both with and without specific linear trends to reduce the potential bias arising from the violation of the parallel-trends assumption.

3.6 Heterogeneity of the Results

After showing the estimated treatment effects from the baseline models, I next examine the heterogeneity of the results. The impact of unemployment and the recession may be different for males and females. To empirically test if treatments effects vary with gender, I include an interaction of the treatment variable with a gender indicator in equations 3.1 and 3.4. These interactions allow the unemployment effect on self-reported health, and the recession effect on health indicators to garner different responses conditional on gender. To account for potential variation on the effect depending on the level of educational, I create a binary indicator that takes value one if an individual reports a college degree or higher educational attainment, and zero otherwise.

I show the results of comparing the effect of unemployment in males versus females and individuals with tertiary education versus individuals without it. Results for unemployed males versus unemployed females show negligible and statistically insignificant differences, while individuals with tertiary educational attainment report better health, but are more adversely affected by unemployment, albeit these estimations are not significant.

In Table 3.12, I show how each health indicator was affected by the economic downturn depending on gender and educational level. Panel A provides results for risky health behaviors. When I examine the effect on smoking behavior, results are negligible for males (a reduction of less than 1 percentage point), whereas, for individuals with tertiary education, findings indicate a reduction of one percentage point. Nonetheless, both results are statistically insignificant. Males and individuals with tertiary education are also less likely to exercise when facing job displacement, but results are only significant for the latter group, showing a reduction of 3 percentage points. These two subgroups also display a higher probability of alcohol consumption as a consequence of the economic downturn (around 2 percentage points in both cases), but none of these results is significant. In Panel B, I present the results for medical attendance. For the group of individuals with at least a university degree, estimated coefficients are always positive, but only significant in the case of probability of seeing a specialist (6 percentage points higher than their counterpart).

For males, results indicate a negative impact on the probability of seeing a doctor, going to an emergency room, or taking medicine, but only the result for being attended into an emergency room outcome is significant (3 percentage points). I also find a positive effect on hospital attendance (2 percentage points) and specialists, but the latter is not statistically significant. Finally, Panel C shows no differences in cholesterol problems between males and females, but those with tertiary education are more likely to report higher cholesterol problems (3 percentage points). In addition, I also find that these two groups are more likely to report high blood pressure (4 percentage points for both cases).

3.7 Conclusions and Discussion

This chapter estimates the effects of labor market shocks on a set of health indicators. I consider three distinct health indicators: self-reported health status, health investment inputs, and objective health measures. Regarding labor market shocks, I focus on a micro-level shock, such as job displacement, and a macro level shock, such as a period of economic deterioration. While income, one of the inputs for health production, is negatively affected by job displacements, the other resource, own time, increases after a layoff. However, depending on how they are combined, these variations in resources may improve health or impair it. For instance, income can be used to improve health by purchasing healthy food or better medical care, but it can also be spent on goods that impair health, such as tobacco or alcohol. Using data from the Survey of Living Conditions and Health Surveys, I find some evidence of the relationship between labor market shocks and health indicators. An interesting aspect of this chapter is that I am able to extend the analysis of labor market shocks beyond the effect on self-reported health and analyze a set of health indicators.

Results suggest that the self-evaluation of health status is negatively affected by unemployment. Moreover, those who suffered job displacement were already reporting worse health even before the transition to unemployment. Regarding health investments, unemployed individuals are more likely to smoke and do physical activity. They also display a higher propensity to take medicines, and hospital stays. Individuals who face job displacements might be also more likely to suffer high blood pressure. These results are a reflection of the worse health reported by unemployed individuals. Unfortunately, the data do not allow me to assess how much of the difference in health inputs and outputs between employed and unemployed individuals are caused by unemployment. I also find that, even before the layoff, those displaced from their jobs earn lower salaries than those not displaced, and the income gap is even larger after an unemployment spell. Consequently, income can be one of the mechanisms behind the results

The effect of unemployment on self-reported health appears to be even larger after the sharp increase in the unemployment rate between 2007-2009, although the estimations are statistically insignificant. The results from an event study design suggest that the effect of the great recession might appear after some periods later on. However, when I compare the differences between employed and unemployed individuals before and after the recession, I see a decline in terms of the difference in smoking behavior, and also an increase in the difference of exercising, both effects in favor of unemployed individuals doing positive health investments. Results from medical care outcomes might suggest that only the difference of emergency room visits is affected. The recession seems to exacerbate differences in blood pressure problems. Thus, some indicators for health inputs are deteriorated after the abrupt deterioration of the labor market over the period 2007-2009 for unemployed individuals, while others remained unaffected or even improved, but I do not find suggestive results driving a larger unemployment effect on self-reported health from 2009 onwards.

The evidence is also mixed when I analyze the effect of the recession on health indicators for the employed and the unemployed both together. The results indicate that self-evaluated health worsens after the sharp decline of the labor market, regardless of their employment status. The event study results for health indicators suggest a higher probability of having to see a specialist, and reporting blood pressure and cholesterol problems. On the other hand, I also find evidence in favor of a decline in the smoking probability, likely driven by pre-recession trends. In addition, the results also point to the same positive effect on the probability of doctor visits, and medicament intake.

Therefore, I find a relationship between self-reported health, and health inputs and outputs for unemployed individuals, but I do not find suggestive evidence to support that impairment in self-evaluated health from 2009 onwards was related to more flawed health investments. A potential mechanism explaining the former finding is the income channel. Unemployed individuals report lower wages, even before job displacements. Thus, they may be constrained to make health investments. Unobserved differences in socioeconomic status (for instance, wealth) may also explain some of these differences (see Cutler et al., 2012). A second potential mechanism might be that less healthy individuals face more often job displacement. Health is known to boost productivity (see Jack, 2012), so individuals with poorer health may be less productive and, consequently, more prone to face job displacement.

Regarding the latter, I consider the potential mechanism behind the results. First, if I consider the results from the triple differences model as a lack of a larger effect of unemployment from 2009 onwards, I could argue that observed improvements in health indicators were not enough to nudge changes in self-evaluated health status. Thus, other factors beyond health investment might be affecting health, such as public policies or gene related issues. Second, if I consider that the unemployment effect was probably larger, even though the results are not significant, this finding would not be aligned with the results from health investment and outputs. This might be caused by some bias in self-perception of health status. For instance, it could be the case that before the recession, individuals tended to overestimate their health status. In that case, the impairment that I observe from 2009 onwards might be upward biased. An alternative option is that individuals underestimate their health status after the financial crisis. Arni et al. (2020) provide empirical evidence of health perception bias. According to their results, they find that some individuals tend to overestimate their health status. Considering that disadvantageous economic conditions affect mental health (see Ridley et al., 2020a), health perception bias and a higher level of depression and anxiety might be affecting health status evaluation.

The last potential explanation is linked with the results for all workers. A lag between health investments and health outputs might exist . Ruhm (2000) indicates that the amount of available time can be lower during expansion periods, which may lead to lower health investment and lower health outputs. Thus, the observed improvement in self-perceived health during the period

2004-2012 may be caused by early investment. If individuals perceive that they have robust health, they can relax their investment efforts. Therefore, the deterioration in self-reported health observed after 2012 may be a consequence of lower health investments in previous years in addition to the deteriorated economic situation. Likewise, the improvement in health inputs that I observe after the Great Recession may be reflected in the next few periods.

The results add evidence to the literature in economics that documents labor market shocks, health inputs, and health outcomes. In terms of policy implications, findings suggest that, if lower health quality is one of the causes of unemployment, encouraging individuals with lower health quality to to invest in health may reduce their probabilities of facing job displacements. In terms of policy implications, public health policymakers in cooperation with workplaces can promote regular health screenings, or promote programs and offer incentives to reduce risky health behaviors or increase exercise. Second, if individuals are biased regarding their health status, periodic checks followed by provision of precise information about the results would improve their self-health evaluation accuracy.

3.8 Figures

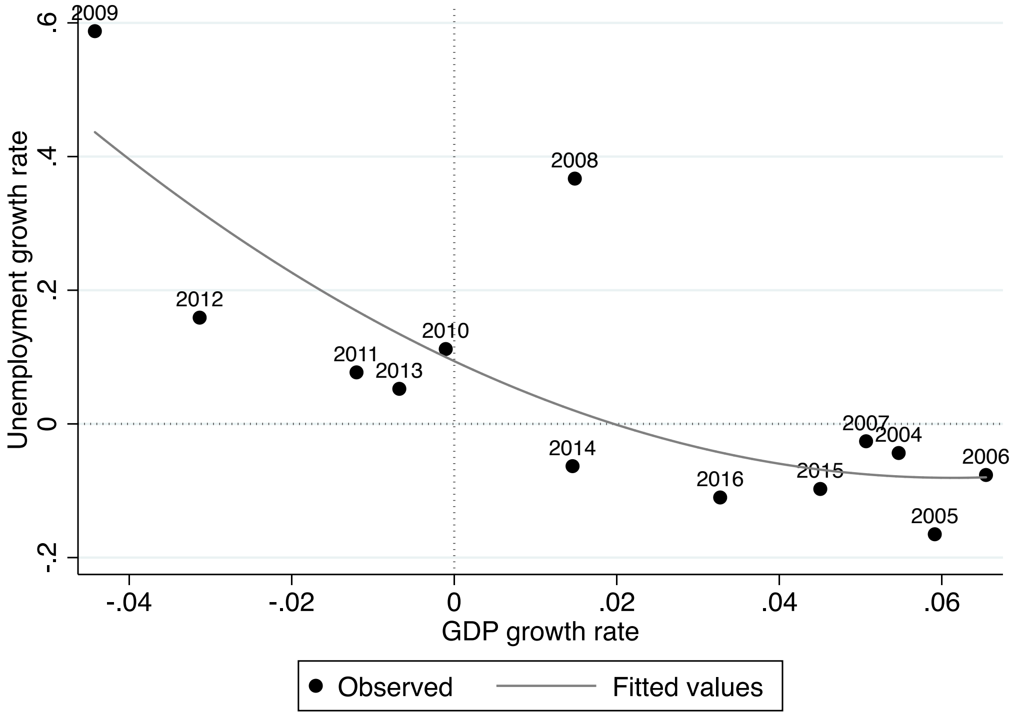

Figure 3.1: GROWTH RATE OF SPANISH GDP PER CAPITA AND UNEMPLOYMENT RATE (2004-2016)

Source: Survey of active population and National accounts from Spanish National Bureau of Statistics
Notes: Figure shows the growth rate of the Spanish unemployment rate and GDP per capita during the period 2004-2016. Each dot refers to the growth between the previous year and the labeled year. Quadratic fit is included.

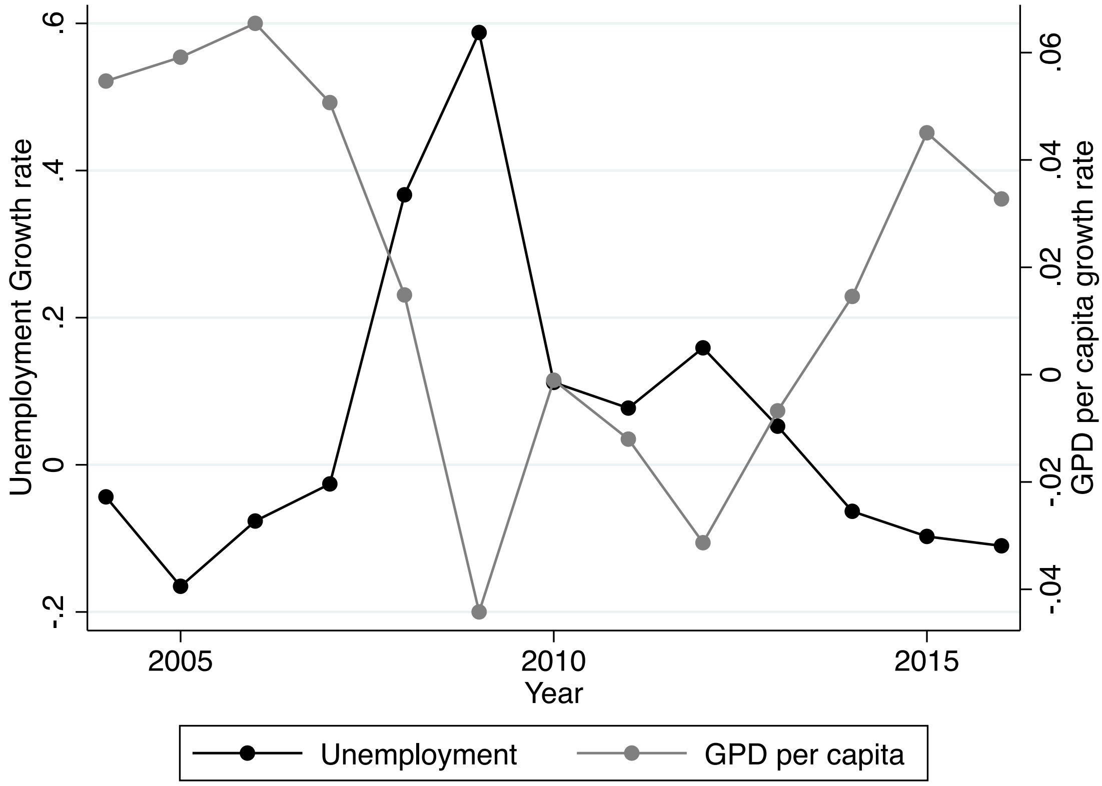

Figure 3.2: Spanish GDP per Capita and Unemployment Rate (2004-2016)

Source: Survey of active population and National accounts from Spanish National Bureau of Statistics
Notes: Figure shows the evolution of the growth rates of the Spanish unemployment rate and GDP per capita during the period 2004-2016.

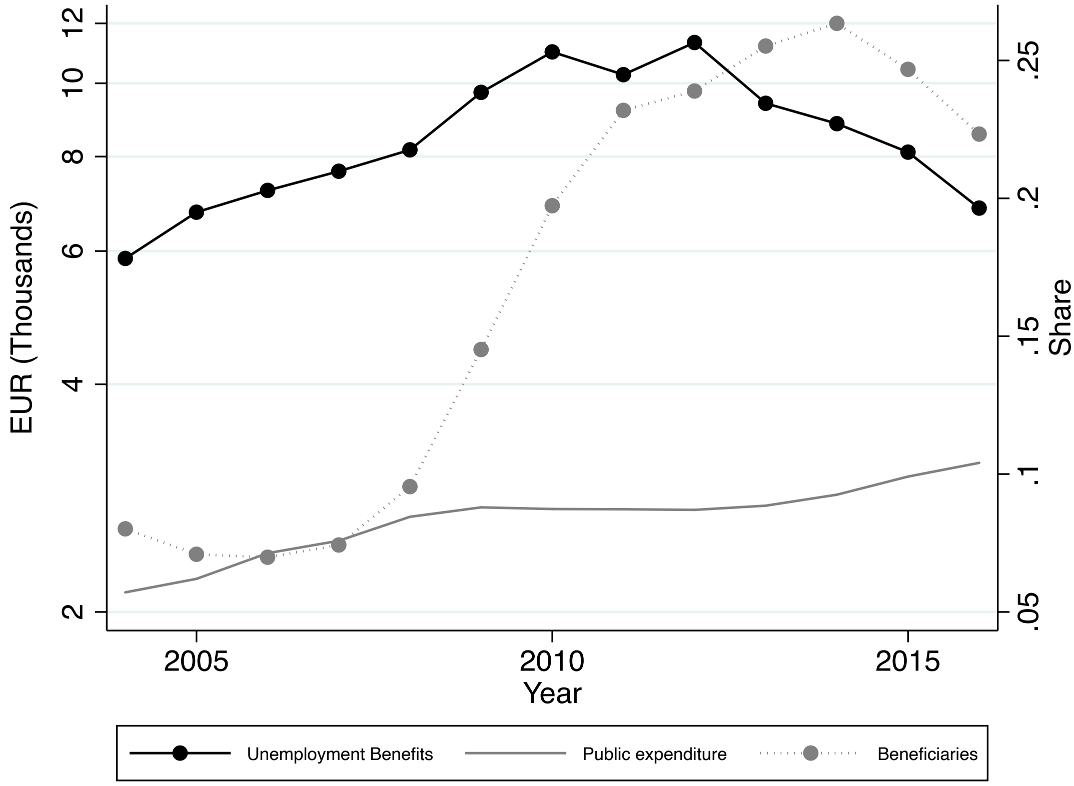

Figure 3.3: UNEMPLOYMENT BENEFITS AND HEALTH EXPENDITURE (2004-2016)

Sources: Survey of income and living conditions and OECD Health spending report
Notes: Figure shows the average unemployment benefit payment from our sample, the share of unemployed individuals receiving unemployment benefits and the average public expenditure on health.

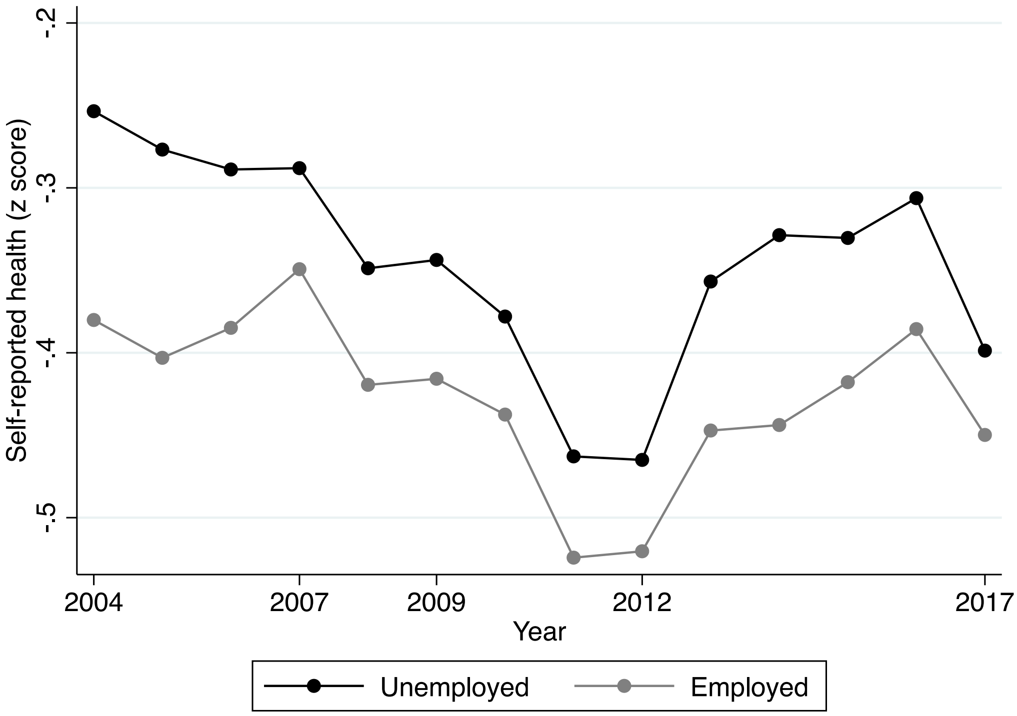

Figure 3.4: ESTIMATES SELF-ASSESSED HEALTH BY LABOR MARKET STATUS (2004-2016)

Notes: Figure shows the evolution of the standardized self-assessed health for employed and unemployed individuals.

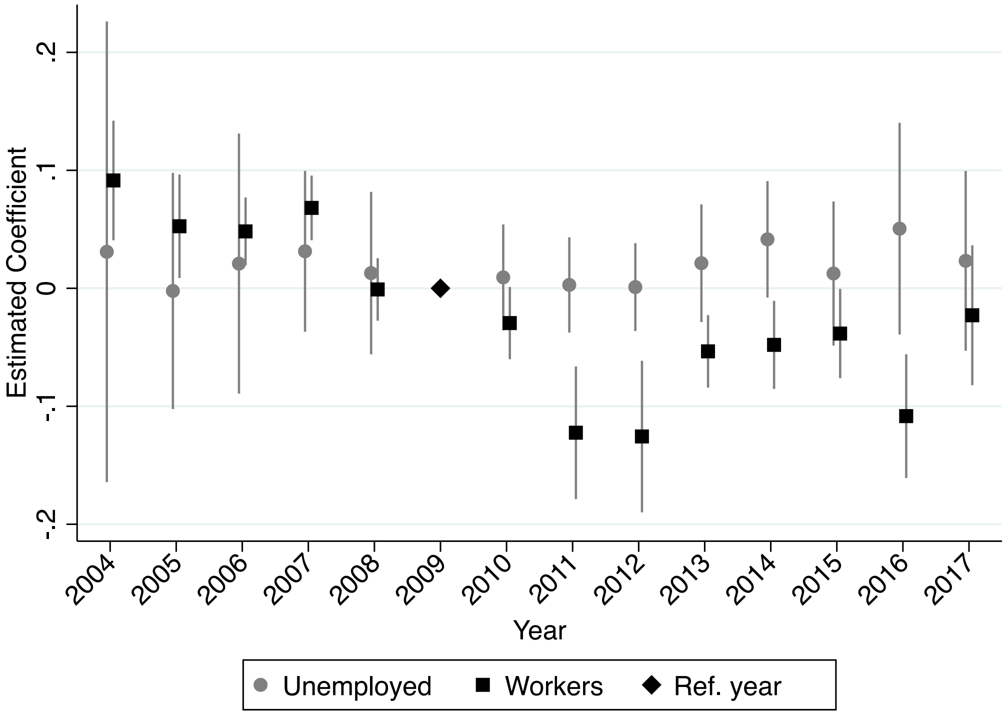

Figure 3.5: EVENT STUDY DESIGN: SELF-REPORTED HEALTH

Notes: Confidence intervals at 5%. All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

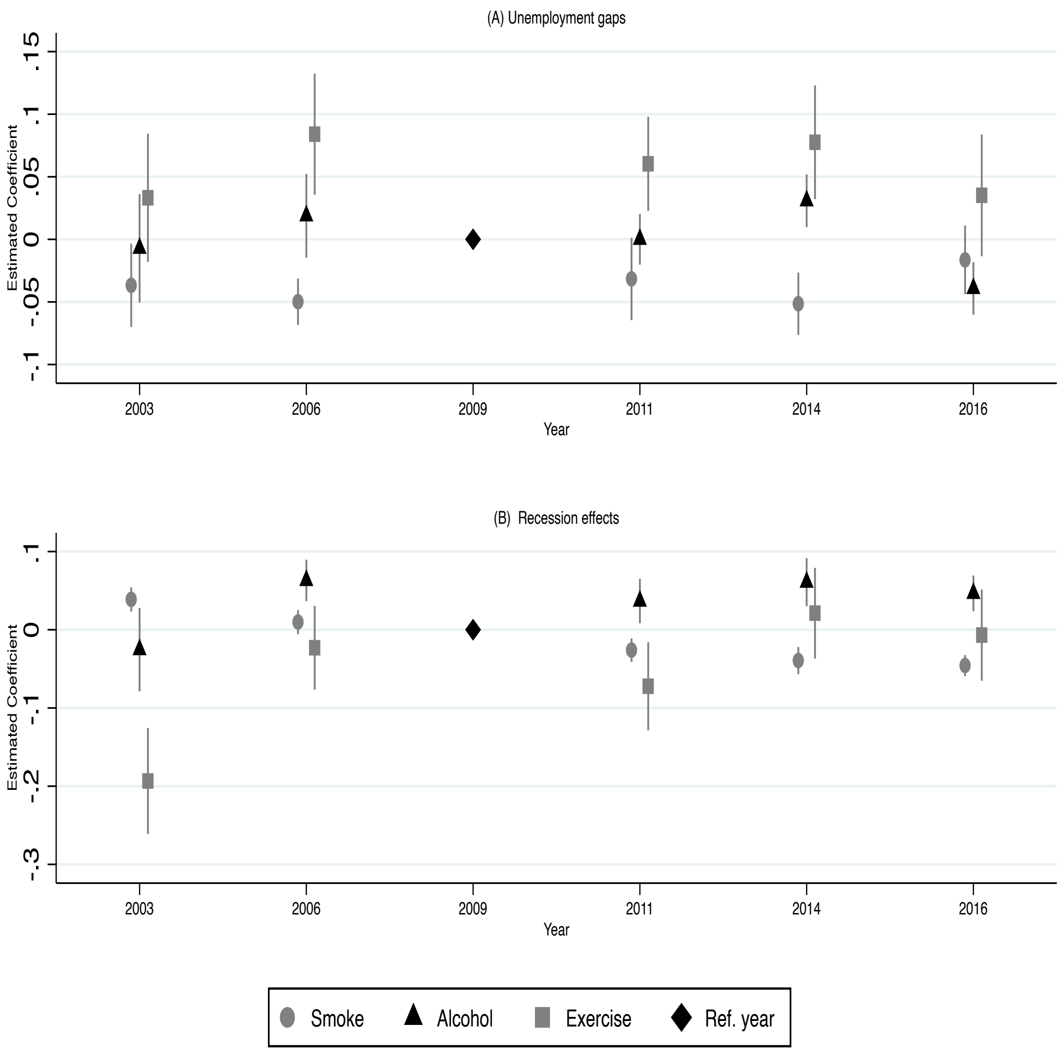

Figure 3.6: EVENT STUDY DESIGN: RISKY HEALTH BEHAVIOR

Notes: Confidence intervals at 5%. All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2009 does not specify the length of time unemployed. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

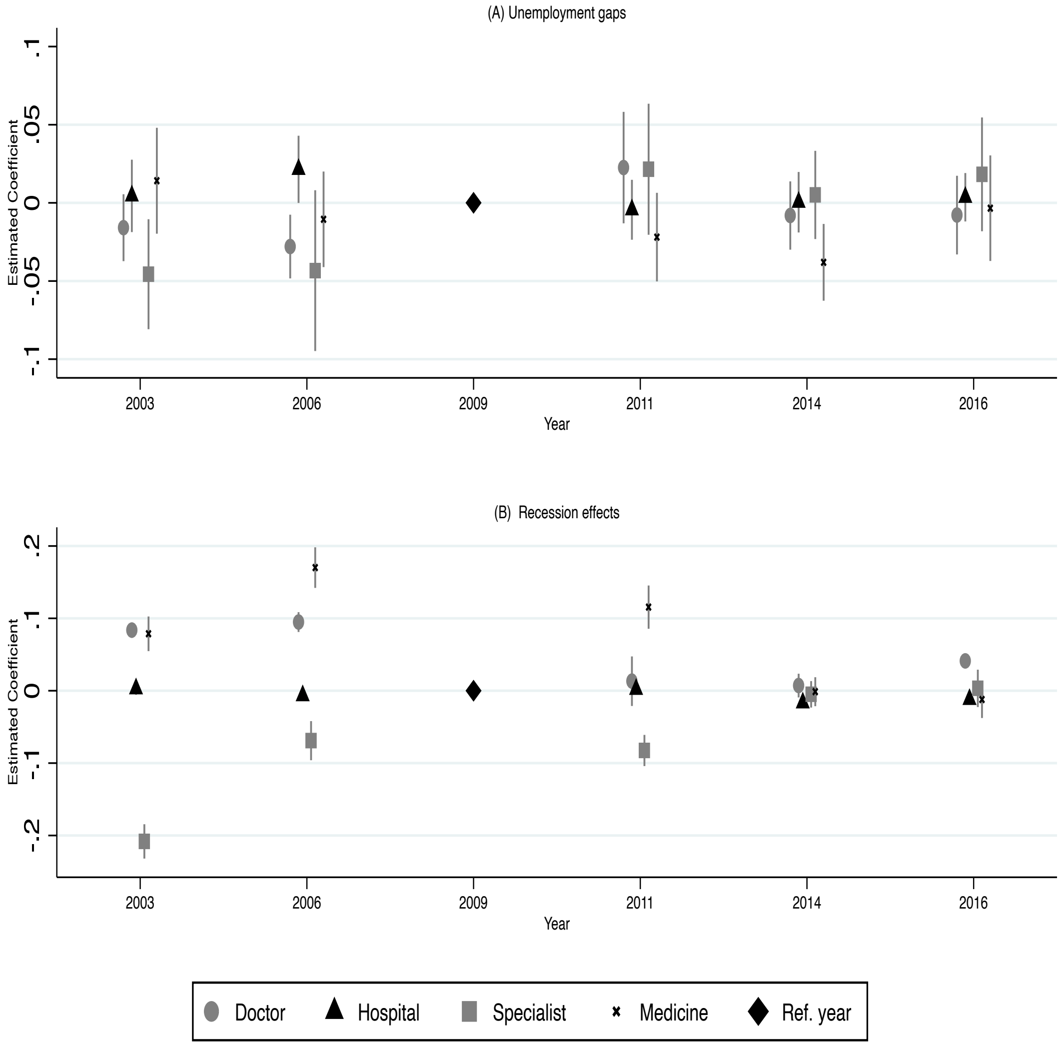

Figure 3.7: EVENT STUDY DESIGN: MEDICAL CARE

Notes: Confidence intervals at 5%. All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2009 does not specify the length of time unemployed. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

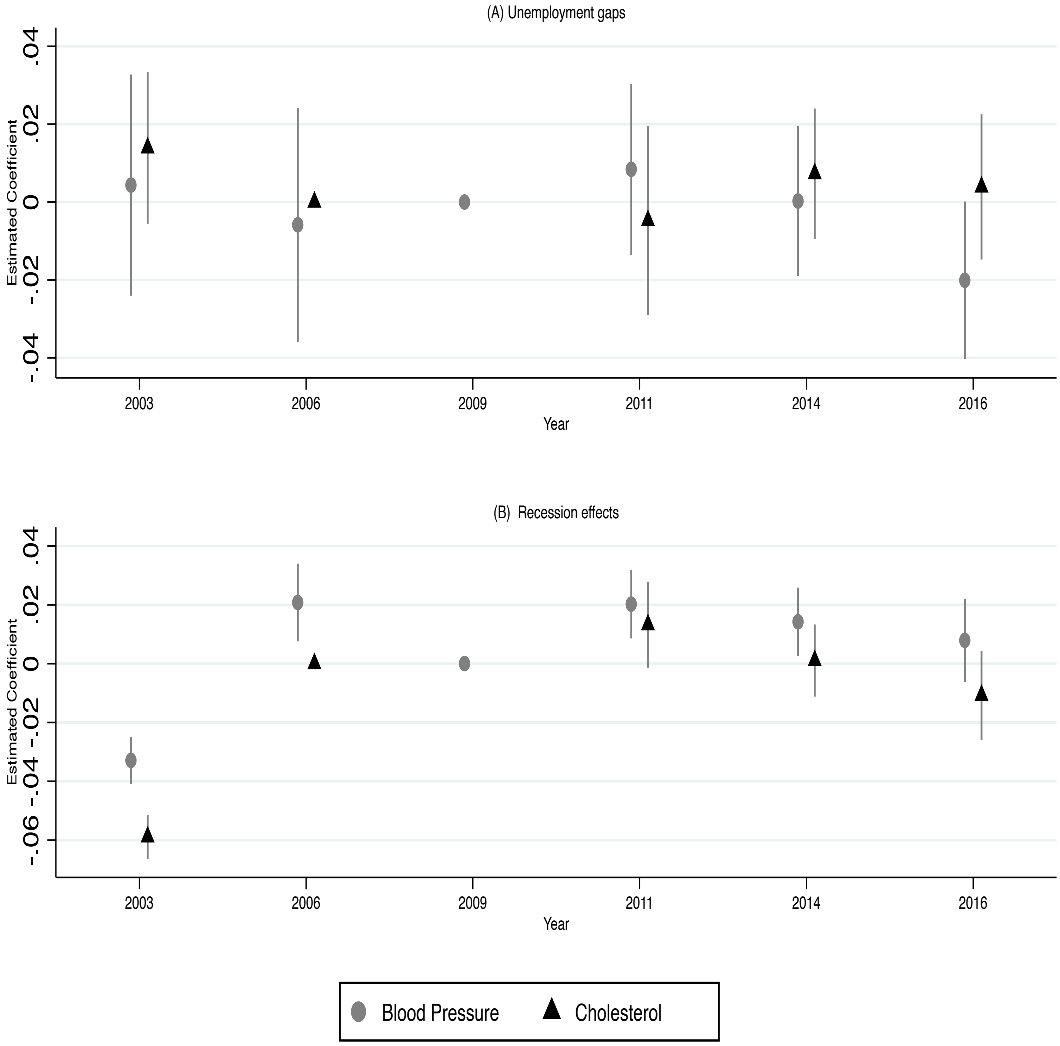

Figure 3.8: EVENT STUDY DESIGN: HEALTH OBJECTIVE MEASURES

Notes: Confidence intervals at 5%. All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2009 does not specify the length of time unemployed, and does not include an item for cholesterol problems. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

3.9 Tables

Table 3.1: DESCRIPTIVE STATISTICS (COVARIATES)

	(1)	(2)	(3)	(4)
	Employed		Unemployed	
	Mean	Std.Dev.	Mean	Std.Dev.
Panel A: SILC				
Age	43.33	10.53	39.65	11.71
Gender	0.556	0.497	0.543	0.498
University degree	0.402	0.490	0.237	0.425
Observations	270,125		20,057	
Panel B: Health surveys				
Age	42.21	10.78	39.75	11.48
Gender	0.540	0.498	0.483	0.500
University degree.	0.261	0.439	0.125	0.331
Observations	61,835		9,047	

Notes: In Panel A I define as unemployed those individuals classified as treated in T1 according to our identification strategy. Gender refers to the share of males, while university degree refers to the share of individuals with tertiary education or above.

Table 3.2: DESCRIPTIVE STATISTICS (OUTCOMES)

	(1)	(2)	(3)	(4)
	Employed		Unemployed	
	Mean	Std.Dev.	Mean	Std.Dev.
Panel A: Reported health and income (SILC)				
Self-reported health	-0.434	0.662	-0.363	0.741
Income	17,971	12,078	12,022	13,085
Panel B: Risky health behaviors				
Smoke (intensive)	0.344	0.475	0.435	0.496
Smoke (extensive)	13.82	9.502	14.29	9.749
Alcohol	0.737	0.440	0.685	0.464
Exercise	0.400	0.490	0.312	0.464
Panel C: Medical care				
Doctor	0.752	0.432	0.777	0.416
Hospital	0.0666	0.249	0.0855	0.280
Emergency	0.260	0.438	0.308	0.462
Specialist	0.398	0.490	0.394	0.489
Medicine	0.486	0.500	0.506	0.500
Panel D: Objective health measures				
Cholesterol (1)	0.105	0.307	0.0999	0.300
Cholesterol (2)	0.0939	0.292	0.0862	0.281
Cholesterol (3)	0.105	0.307	0.103	0.304
Blood Pressure (1)	0.120	0.325	0.119	0.324
Blood Pressure (2)	0.102	0.302	0.0985	0.298
Blood Pressure (3)	0.120	0.325	0.118	0.323

Notes: In Panel A I define as unemployed those individuals classified as treated in T1 according to our identification strategy. Cholesterol (1) and blood pressure (1) refer to having a problem in each outcome. Cholesterol (2) and blood pressure (2) refer to having a problem in each outcome during the last twelve months. Cholesterol (3) and blood pressure (3) refer to being diagnosed as having each problem.

Table 3.3: ESTIMATES OF THE EFFECT OF UNEMPLOYMENT ON SELF-ASSESSED HEALTH

	(1)	(2)	(3)	(4)
	Difference-in-differences		Triple Differences	
Panel A: Treatment T0				
Unemployment*Post	0.0198**	0.0196**	0.0330	0.0321
	(0.0087)	(0.0087)	(0.0317)	(0.0318)
Unemployed	0.0757***	0.0721***	0.1018***	0.0987***
	(0.0099)	(0.0100)	(0.0153)	(0.0155)
Observations	284,996	284,859	284,996	284,859
Panel B: Treatment T1				
Unemployment*Post	0.0257**	0.0256**	0.0544	0.0538
	(0.0103)	(0.0104)	(0.0399)	(0.0399)
Unemployment	0.0758***	0.0735***	0.1023***	0.0998***
	(0.0115)	(0.0115)	(0.0154)	(0.0155)
Observations	262,915	262,832	262,915	262,832
Panel C: Treatment T2				
Unemployment*Post	0.0302	0.0302	0.0389	0.0378
	(0.0194)	(0.0195)	(0.0492)	(0.0490)
Unemployed	0.0695***	0.0666***	0.1176***	0.1140***
	(0.0186)	(0.0184)	(0.0208)	(0.0207)
Observations	251,533	251,450	251,533	251,450
Household covariates .	No	Yes	No	Yes

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Household covariates adjust for the educational level of household members, the share of members in each age profile, labor market status, and the average health status of the rest of the members. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.4: ESTIMATES OF THE EFFECT OF UNEMPLOYMENT ON INCOME

	(1)	(2)
Panel A: Difference-in-differences design		
Unemployed*Post	-401.9653***	-398.3496***
	(48.4526)	(48.0014)
Unemployed	-1,749.6133***	-1,704.8906***
	(140.6525)	(137.1413)
Observations	262,969	262,886
Panel B: Triple differences design		
Unemployed*Post	258.4219**	256.58**
	(102.6407)	(100.5737)
Unemployed	-1,870.1885***	-1,826.2293***
	(151.8411)	(150.4699)
After 2009	-646.0871***	-631.8159***
	(147.4955)	(148.7715)
Observations	262,969	262,886
Household covariates	No	Yes

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Household covariates adjust for the educational level of household members, the share of members in each age profile, labor market status, and the average health status of the rest of the members. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.5: ESTIMATES OF THE EFFECT OF RECESSION ON RISKY HEALTH BEHAVIOR

	(1)	(2)	(3)	(4)
	Smoking		Alcohol	Exercise
	Extensive	Intensive		
Panel A: Difference-in-differences				
Unemp*Post	-0.0094	-0.9732**	-0.0041	0.0256
	(0.0116)	(0.3555)	(0.0107)	(0.0156)
Unemployed	0.0561***	1.0865***	-0.0212***	0.0271***
	(0.0078)	(0.2855)	(0.0054)	(0.0090)
Panel B: Difference-in-differences with linear trends				
Unemp*Post	-0.0483*	-0.2736	0.0137	0.0730***
	(0.0233)	(0.5689)	(0.0140)	(0.0250)
Unemployed	0.0255*	1.6409***	-0.0071	0.0647***
	(0.0134)	(0.3721)	(0.0164)	(0.0618)
Observations	69,050	22,827	69,725	69,061

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent. They also include marital status and occupational dummies. Smoking intensity conditional on smoking behavior. Data from 2009 does not specify the length of time unemployed length. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.6: ESTIMATES OF THE EFFECT OF RECESSION ON MEDICAL CARE

	(1) Doctor	(2) Hospital	(3) Specialist	(4) Emergency	(5) Medicine
Panel A: Difference-in-differences design					
Unemp*Post	0.0090	-0.0076	0.0138	-0.0125	-0.0222**
	(0.0078)	(0.0072)	(0.0150)	(0.0135)	(0.0082)
Unemployed	0.0162**	0.0239***	-0.0080	0.0302**	0.0447***
	(0.0066)	(0.0048)	(0.0081)	(0.0108)	(0.0080)
Panel B: Difference-in-differences with linear trends					
Unemp*Post	0.0005	-0.0076	0.0206	-0.0593*	-0.0254
	(0.0193)	(0.0157)	(0.0225)	(0.0301)	(0.0169)
Unemployed	0.0101	0.0239**	-0.0030	0.0095	0.0422**
	(0.0143)	(0.0111)	(0.0121)	(0.0183)	(0.0153)
Observations	62,112	69,263	52,146	57,294	58,803

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2009 does not specify the length of time unemployed, and does not include an item for emergency rooms visits. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.7: ESTIMATES OF THE EFFECT OF RECESSION ON HEALTH OBJECTIVE MEASURES

	(1)	(2)	(3)	(4)	(5)	(6)
	Blood pressure			Cholesterol		
	Reported	12m	Medical	Reported	12m	Medical
Panel A: Difference-in-differences						
Unemp*Post	-0.0023	0.0020	0.0003	-0.0049	-0.0060	0.0004
	(0.0054)	(0.0053)	(0.0054)	(0.0074)	(0.0090)	(0.0104)
Unemployed	0.0079	0.0035	0.0062*	0.0042	0.0035	0.0033
	(0.0047)	(0.0034)	(0.0034)	(0.0050)	(0.0074)	(0.0078)
Panel B: Difference-in-differences with linear trends						
Unemp*Post	0.0181	0.0171	0.0241	-0.0080	-0.0241	-0.0081
	(0.0143)	(0.0173)	(0.0173)	(0.0173)	(0.0206)	(0.0177)
Unemployed	0.0241*	0.0139*	0.0227**	0.0023	-0.0058	-0.0011
	(0.0133)	(0.0076)	(0.0087)	(0.0090)	(0.0096)	(0.0072)
Observations	69,167	58,806		57,185	46,837	

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2003 does not include items for problems in the last twelve months and diagnoses. Data from 2009 does not specify the unemployment length and does not include an item for cholesterol problems. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.8: ESTIMATES OF THE ROBUSTNESS CHECKS

	(1)	(2)
Panel A: First identification strategy		
Unemployment*Post	0.0459*	-276.6613***
	(0.0243)	(24.8374)
Observations	81,282	80,578
Panel B: Second identification strategy		
Unemployment*Post	0.0265**	-412.0212***
	(0.0119)	(13.4949)
Observations	106,286	104,926
Panel C: Third identification strategy		
Unemployment*Post	0.0341*	-406.4511***
	(0.0179)	(23.1081)
Observations	85,746	84,942
Panel D: Parallel trends test		
Unemployment*Trend	-0.0059	-120.1930
	(0.0127)	(137.0441)
Observations	58,786	58,795

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Number of unemployed individuals in the household are included. Robust standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.9: ESTIMATES OF THE PRE-TREATMENT TEST ON SELF-REPORTED HEALTH

	(1)	(2)	(3)	(4)
	Placebo Test		Common Trends	
	T0	T1	T0	T1
Unemployment*Post	0.0213	0.0232	0.0331	0.0396
	(0.0145)	(0.0140)	(0.0355)	(0.0347)
Pre-treatment test	0.0057	0.0032	-0.0061	-0.0122
	(0.0209)	(0.881)	(0.0156)	(0.0165)
Observations	202,038	197,213	62,338	60,703

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Household covariates adjust for the educational level of household members, the share of members in each age profile, labor market status, and the average health status of the rest of the members. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.10: PRE-TRENDS TEST FOR CROSS-SECTION DIFFERENCE-IN-DIFFERENCES

	(1)	(2)	(3)	(4)	(5)
Panel A: Risky health behavior					
	Smoking		Alcohol	Exercise	
	Extensive	Intensive			
Pre-Trends	0.0208**	-0.2350	-0.0031	-0.0291*	
	(0.0091)	(0.2441)	(0.0140)	(0.0152)	
Observations	37,259	13,727	37,907	37,264	
Panel B: Medical outcomes					
	Doctor	Hospital	Specialist	Emergency	Medicine
Pre-Trends	0.0072	-0.0026	-0.0138*	0.0195	-0.0139
	(0.0060)	(0.0068)	(0.0071)	(0.0209)	(0.0109)
Observations	37,445	37,445	25,476	27,501	37,904
Panel C: Objective health measures					
	Blood pressure			Cholesterol	
	Reported	12m	Medical	Reported	
Pre-Trends	-0.0038	-0.0027	0.0047	-0.0133	-
	(0.0084)	(0.0155)	(0.0182)	(0.0114)	
Observations	37,393	26,988		25,435	

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2003 does not include items for problems in the last twelve months and diagnoses. Data from 2009 does not specify the length of time unemployed, and does not include an item for cholesterol problems and for emergency rooms visits. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.11: ESTIMATES OF THE HETEROGENEOUS EFFECT OF UNEMPLOYMENT ON SELF-ASSESSED HEALTH

	(1)	(2)	(3)	(4)
	Gender		Educational level	
Unemployment*Post	0.0008	0.0006	0.0134	0.0134
	(0.0156)	(0.0156)	(0.0477)	(0.0478)
Unemployment	-0.0058	-0.0068	-0.0259	-0.0241
	(0.0158)	(0.0158)	(0.0278)	(0.0280)
Observations	262,915	262,832	262,915	262,832
Household covariates .	No	Yes	No	Yes

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Household covariates adjust for the educational level of household members, the share of members in each age profile, labor market status, and the average health status of the rest of the members. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

Table 3.12: ESTIMATES OF THE HETEROGENEITY EFFECT OF THE RECESSION ON HEALTH

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Risky health behavior						
	Smoke	Alcohol	Exercise			
Unemp*Post	-0.0053	0.0259	-0.0512***		-	
*Male	(0.0197)	(0.0192)	(0.0159)			
Unemp*Post	-0.0134	0.0271	-0.0330		-	
*Tertiary	(0.0340)	(0.0266)	(0.0292)			
Panel B: Medical outcomes						
	Doctor	Hospital	Emergency	Specialist	Medicine	
Unemp*Post	-0.0037	0.0244**	-0.0395*	0.0212	-0.0285	-
*Male	(0.0235)	(0.0089)	(0.0188)	(0.0286)	(0.0184)	
Unemp*Post	0.0304	0.0164	0.0241	0.0624**	0.0597	-
*Tertiary	(0.0266)	(0.0162)	(0.0262)	(0.0227)	(0.0471)	
Panel C: Objective health measures						
	Cholesterol			Blood pressure		
	Reported	12m	Medical	Reported	12m	Medical
Unemp*Post	0.0092	-0.0031	0.0003	0.0358**	0.0239	0.0064
*Male	(0.0165)	(0.0177)	(0.0185)	(0.0156)	(0.0175)	(0.0138)
Unemp*Post	0.0304*	0.0312	0.0242	0.0443*	0.03960	0.0372
*Tertiary	(0.0162)	(0.0213)	(0.0196)	(0.0254)	(0.0346)	(0.0332)
Observations	69,167	58,806		57,194	46,837	

Notes: All regressions include region and year fixed effects. All regressions include interactions for age, educational level, and gender. They also include marital status and occupational dummies. Data from 2003 does not include items for problems in the last twelve months and diagnoses. Data from 2009 does not specify the length of time unemployed, and does not include an item for cholesterol problems and for emergency rooms visits. At region level clustered standard errors in parentheses. One, two, and three stars indicate significance at ten, five, and one percent.

4

THE EFFECT OF ECONOMIC SHOCKS ON MENTAL HEALTH OUTCOMES

4.1 Introduction

Mental health disorders constitute around 13 percent of the global burden of disease, surpassing cardiovascular disease, and cancer (e.g. Collins et al., 2011). It is estimated that, in 2017, 3.76 percent of the population had an anxiety disorder, and 3.44 percent suffered from depression Ritchie & Roser (e.g. 2018). There exists extensive literature that analyzes the relationship between labor market status and intangible outcomes, such as mental health, well-being, or happiness. Mental well-being is relevant in economics for two reasons. First, economic status influences mental well-being through different channels, such as income and labor market status. Second, mental health may affect economic status if individuals with poor mental health suffer from these negatives labor market experiences, including lower wages, higher temporality, or longer unemployment spells. These bad labor market outcomes may impair mental health, creating a mental health trap. Therefore, a deep understanding of the relationship between labor market status and mental health becomes crucial for an optimal policy design focused on mitigating potential problems that may arise from that relationship.

A principal concern when individual life evaluation is studied relies on Prospect Theory, e.g. Tversky & Kahneman (1981), according to which individuals tend to evaluate their outcomes depending on the deviation from a reference point. Estimating the income gradient in reported well-being might be straightforward if only the income level matters. However, it may be more accurate to assume that individuals evaluate their status concerning their reference point, as in Luttmer (2005). Therefore, comparing different individuals with different income levels might not be the optimal strategy to estimate the causal relationship between income and subjective mental health. I identify two potential reference points that individuals may consider when making their self-assessment: inherent standard reference points and social reference points. As regards the inherent standard reference point, is supposed that individuals accustom themselves to certain living conditions. Thus, they compare their current status with a former status that acts as a baseline, and individuals create expectations using available information. Therefore, they evaluate their actual position using their beliefs as to what what they deserve as a benchmark. Moreover, they might assess the probability of reaching a desirable position in the future. As concerns social reference points, when individuals evaluate something they are experiencing (income or grades, for instance), they may compare their own position with the position of the members of their reference group. For instance, one individual can evaluate how much is she earning by comparing her wage with a colleague's wage. Another example could be a student that evaluates her grades, comparing them with those obtained by classmates. These reference groups could be their colleagues, relatives, or former fellow students, for instance.

In this chapter, I analyze the differences in mental health between unemployed and employed individuals, and the long-run consequences of an economic downturn on the those differences. An unemployment spell is a shock that removes affected individuals from their reference points. First, unemployment might affect the self perceived mental health through the violation of a social norm. If individuals perceive that their social category implies being employed, unemployment means a deviation from the expected situation (see Akerlof & Kranton 2000). Unemployment might then reduce life valuation of unemployed individuals. Moreover, it reduces disposable income. Thus, individuals affected by job losses are also removed from their reference points regarding earnings as well. Henceforth, unemployment may affect both inherent standard reference points and social reference points. Furthermore, a recession period also affects individuals reference points. A higher unemployment rate may be linked to worse future perspectives, but can change social norms as well. For instance, those individuals who belong to high unemployment groups may experience a weaker violation of the social norm when they are unemployed.

The empirical analysis begins by analyzing the impact of unemployment on subjective mental health using Spanish and European Health Surveys from 2006 to 2017. I divide the sample into two groups: those reporting unemployed status composes the treatment group, and the control group is composed of those who report being employed. I compare results from the different selection on observables methods. Results from regression, Inverse Probability Weighting, and Matching methods do not show remarkable differences. Then adjustments for past mental health and past labor market status are included. Results show how the inclusion of an adjustment for these confounders can reduce the bias.

Having provided evidence of the differences in self-evaluated mental health between employed and unemployed individuals, the second analysis focuses on the scarring effect of a recession on the subjective mental health of unemployed individuals. I exploit the labor market deterioration that affected Spain as a consequence of the Great Recession to perform a repeated cross-section difference-in-differences exercise. The design focuses on a period after a massive increase of the unemployment rate and decreasing GDP per capita, which provides an opportunity to analyze the variation in the differences between employed and unemployed individuals on subjective mental health after an economic downturn compared with the period before. Thus, I estimate the long-term effect of the Great Recession on the mental health gap between workers according to their labor market status. I also add to the analysis two medical outcomes related to mental health: the probability of taking or being prescribed medicines for mental health problems, and the likelihood of visiting a mental health specialist.

Another interesting feature of this chapter is that the test for the dominance of both inherent standard points and social reference points. To do so, I look at individuals working in occupations heavily affected by the Great Recession, and then compare the results with those obtained from individuals working in occupations barely affected by the increase in the unemployment rate. If inherent standard points dominate the effects, during periods when the unemployment rate is high, the prospect of finding a job might be weakened, thus, negatively affecting mental health.

However, if the social norm dominates the horizon, during periods when the unemployment rate is high, individuals may feel that unemployment is a generalized phenomenon, and therefore it might affect mental health to a lesser extent.

Therefore, this chapter adds evidence to the literature focused on social behavior. For instance, Clark (2003) finds that the effect of unemployment on well-being is smaller when there are more unemployed in the reference group. Luttmer (2005) provides some evidence regarding the dependence that utility has from the individual's relative position regarding their social surroundings. He shows that a given individual's subjective well-being is negatively correlated with the income of those members located in the same area. Perez-Truglia (2020) uses an income transparency law (which allows checking the income level of any citizen) to test if income differences drive differences in reported happiness. His findings indicate that self-evaluated happiness and life satisfaction gaps increase when individuals are aware of their real income distribution position. Ferrer-i Carbonell (2005) finds that increases in household income do not improve evaluations of well-being when the reference group experiences the same income improvement. Kahneman & Deaton (2010) notice that the relationship between income and well-being is not linear. Above a certain income threshold, income increases may not increase happiness.

Results from the preferred specification indicate that the deterioration of the economic situation increases the mental health gap by 0.07 - 0.09 standard deviations, which corresponds to around 45 percent of the cross-differences between unemployed and employed individuals. The impact of the economic deterioration on mental illness in the short run is around 0.02 standard deviations, but this coefficient is not statistically significant. Further, I find evidence that during a recession, those who lost their jobs show a decrease of around two percentage points in terms of the likelihood of being prescribed medicine for mental problems compared to the period before the recession. Moreover, I do not find evidence of changes in the likelihood of visiting a mental health specialist for those who become unemployed. Placebo tests show that the effect of the recession on medical attendance is underestimated for unemployed individuals.

In addition, when I explore the heterogeneity of the results, I find fuzzy evidence when focus on the effect on males, high educated individuals, and those employed in low unemployment rate sectors. First, findings suggest that those unemployed individuals with a college degree report a higher impact of the recession on mental health (a difference of 0.11 standard deviations). I also find a higher probability of using drugs for mental health (a difference of 0.03 standard deviation). However, I do not find significant differences in the probability of visiting a mental health differences as regards the likelihood of visiting a mental specialist. Exploring the heterogeneity caused by gender or occupation, I find that the differences are statistically indistinguishable from zero. However, results from the pre-trends test show evidence in favor of underestimating the impact based on these two characteristics. Had I considered pre-recession differences, the causal effect of unemployment during the recession would have been larger for males and those workers less affected by the recession. Finally, effects on drug use or medical attendance are not found.

This chapter's findings are significant for several reasons: First, whereas the literature usually focuses on the effect of unemployment on mental health, I look at how the deterioration of the economic situation affects the differences between employed and unemployed individuals. Second, I add medical outcomes related to mental health to analyze the response to the unemployment shock. The results indicate that even when unemployed individuals experience impairment in their mental health, they do not report visiting a mental health specialist or having mental medicine prescribed. I provide two potential explanations: First, individuals could actually visit a doctor or take mental health medicine, but hide it in the interviews. Second, individuals do not visit a doctor or take mental medicine due to the stigma surrounding mental health problems. Third, I look at the heterogeneity of the effects to understand the mechanism behind the results. Furthermore, subsamples based on unemployment rates or social norms allow me to test the impact of the violation of social norms.

Results are in line with those who analyzed the relationship between unemployment and mental health in Spain. Urbanos-Garrido & Lopez-Valcarcel (2015) find This relationship was also analyzed for the case of workers heavily affected by the financial downturn. Farré et al. (2018) find that unemployment negatively affects the mental health of those workers that suffer job losses in the construction sector. Bartoll et al. (2018) look at the causal effect of temporary employment on work-related stress and mental health before and during the Great Recession, finding a positive link between temporary employment and deterioration of mental health. Furthermore, the results regarding the demand for mental health care are in line with those in Martínez-Jiménez & Vall Castelló (2020), who find no significant changes in the use of drugs for depression and anxiety in Catalonia during the 2010-2015 period. However, Gili et al. (2012) find that an increase in the frequency of mental health disorders among primary care attendees in Spain during 2010 compared to 2006

Nonetheless, I present some remarkable insights in this chapter in contrast with these previous ones. First, I consider different survey waves. Covering a more extended period, I are able to check whether the results vary with the business cycle. Second, some survey items related to past mental health are used to adjust for pre-recession differences. Finally, I include different outcomes linked with mental health, such as whether individuals take drugs for depression or anxiety, and whether individuals visited a mental health specialist.

The rest of the chapter is structured as follows. In Section 4.2 I provide some background information on unexpected unemployment shocks and the Great Recession in Spain, while Sections 3 and 4 describe the data and the empirical strategy. Section 5 contains the main findings. Section 6 presents the robustness checks, and Section 7 includes heterogeneity analysis. Finally, Section 8 concludes.

4.2 Mental Health and Labor Markets

4.2.1 An Overview on the Relationship Between Income and Well-Being

The relationship between economic status and happiness was first established by Easterlin (1974), who found that those individuals in the upper part of income distribution were happier than those in the bottom part. However, his work does not find differences between high income and low-income countries. Easterlin suggested that individuals tend to evaluate their happiness in relationship to other individuals, who act as a reference point. Instead, Sacks et al. (2010) found that average life satisfaction is higher in those countries with higher GDP. In this sense, some papers propose using income shocks to establish the relationship between income and well-being. Accordingly, if income is one of the well-being determinants, income changes and income sources should also be relevant. Regarding income changes, Akee et al. (2018) examine the impact of an unconditional income transfer on child personality traits. Using a difference-in-differences strategy, they conclude that the increase in unconditional household income improves the personality traits of those children living in treated households. Baird et al. (2013) use a randomized cash transfer to assess the causal effect of income shock on self-assessed mental health, concluding that a positive income shock improves psychological distress. Lachowska (2017) contributes to the income and subjective well-being literature studying the effect of an unexpected lump-sum tax rebate. She finds a positive effect of 0.6 standard deviations on well-being. To estimate the impact of income on subjective well-being, Lindqvist et al. (2018) use lottery prizes as income shock. Schwandt (2018) studies the effects of wealth shock caused by stock market fluctuations on a set of health outcomes. Results show that a ten percent wealth shock leads to an improvement in mental health.

A second approach consists of analyzing the differences in the labor market status, given that for a fair share of individuals, it represents the primary income source. For instance, Bell & Blanchflower (2018) find that underemployed individuals display a higher prevalence of mental illness. Tefft (2011) finds a positive relationship between the unemployment rate and Google search for depression and anxiety. Moreover, unemployment can also affect the spouse's mental health, as in Marcus (2013). Bangham (2019) finds that household income and labor market status are both related to subjective well-being. Stutzer & Odermatt (2018) study the impact of a set of life events, including unemployment. The main findings indicate that unemployment harms life satisfaction. Ridley et al. (2020b) extend the analysis to the relationship between poverty and mental health.

4.2.2 The Future of Employment and the Welfare State

During the last century, large unemployment rate periods were linked to the business cycle, and more precisely, to recession periods. Nonetheless, threats to employment stability may stem from an unexpected event. For instance, the debate relating to the increasing automatization of production and improvements on Artificial Intelligence (AI) and their threat to labor markets is a contention topic. These changes in production processes could lead to, at least, partial replacement of human work. Examples of this are Frey & Osborne (2017), Acemoglu & Restrepo (2017), Acemoglu & Restrepo (2020), or Autor & Salomons (2018). Another example comes from the pandemic caused by Covid-19. This virus forced governments to close economies, only allowing for essential activities, which caused a huge increase in unemployment. According to Cowan (2020), more than 38 million unemployment claims were filed in less than two months after the US lockdown. Conde-Ruiz et al. (2020) estimate that affiliation to social security fell by more than a million people as a consequence of the pandemic in Spain.

Given the uncertainty regarding the future of employment, and the relationship between unemployment and mental illness, understanding the relationship between these two variables seems a critical issue. Therefore, these side effects caused by the technological change might be mitigated by an optimal welfare state design. There is evidence in favor of a relationship between mental health and welfare policies such as Earned Income Tax Credit (EITC) (see (Gangopadhyaya et al., 2019)). In this sense, Hoynes & Rothstein (2019) start to evaluate the Universal Basic Income (UBI) as an alternative to mitigate the negative externalities that arise from automatization. Preliminary results from the UBI experiment in Finland show a positive effect of the unconditional income transfer on self-reported well-being. Other authors, as Andersen & Svarer (2010), propose unemployment insurance that depends on the business cycle. Kuka (2020) find that UI is an important tool for reducing the negative impact of unemployment on health outcomes. Furthermore, the results suggest that the effect of unemployment shock on mental illness is heterogeneous. Therefore, as Ridley et al. (2020b) remark, effective policy design to mitigate externalities caused by layoffs requires evidence about the consequences that these job displacements may cause.

4.2.3 Case of Study: The Great Recession in Spain

The Great Recession was incredibly hard in Spain. In Figure 4.1, I show the evolution of Spanish GDP per capita and the unemployment rate. In 2008, when the unemployment rate was already increasing, GDP per capita was at its peak. Five years later, in 2013, GDP per capita hit its lowest point in this period, whereas the unemployment rate was over 25 percent. By the end of 2016, GDP per capita had almost recovered, but the unemployment rate was still around 20 percent. In figure 4.2, I present further evidence of the evolution of the Spanish GDP per capita and the unemployment rate. The above statistics make the Spanish case a very interesting one. Furthermore, it is interesting to notice that the unemployment shock affected a fairly heterogeneous population. Figure 4.3 shows that unemployment affected males and females, individuals at all educational attainment levels, and in all working age groups. Therefore, the treated group is heterogeneous and not a selected subsample. Finally, the unemployment shock had a remarkable impact on income. Kawano & LaLumia (2017) find that household income declined by about 17 percent using data from the USA. Gardeazabal & Polo-Muro (2020) find that unemployment could reduce the monthly family income by around 400€.

4.3 Data and Descriptive Statistics

4.3.1 Spanish Health Surveys

I use data from the Spanish Health Survey (SHS) and the European Health Survey (EHS). These surveys consist of a two-part questionnaire: one focused on a randomly selected individual in each household, and another questionnaire covering the characteristics of the other household members. The former reports rich socioeconomic information including age, gender, education, marital status region, size of the town of residence, and household size. Labor market covariates are also included, such as status, occupation, and contract type. The health questionnaire includes an instrument to measure psychological well-being. Questions about past health status are included as well. The inhabitant's questionnaire covers detailed information about age, education, and labor market status. The SHS is conducted by the Spanish Health Ministry every five years. This survey is a nationally representative sample of around 20,000 men and women. I use the available waves for 2006, 2011/12 (2011 henceforth), and 2016/17 (2016, henceforth). Eurostat conducts the EHS, and the actual data collection for Spain is carried out by the Spanish Statistical Bureau (INE). Comparing both surveys, the SHS covers a wider range of data than the EHS. For instance, data for the year 2009 does not specify the length of time unemployed for those who are unemployed. I use waves 2009 and 2014 of the EHS.

4.3.2 Mental Health Instruments

As I use different waves from different providers, I have different instruments to measure self-evaluated mental health. For 2006, 2011, and 2016, the SHS uses the General Health Questionnaire (GHQ). The GHQ was developed to detect individuals with a mental disorder (see Goldberg & Hillier (1979); Goldberg & Williams (1988)). The original questionnaire had 60 items (GHQ 60) from which shorter versions of 12 items (GHQ 12) have been constructed. The GHQ-12 has been used in several studies as in Apouey & E.Clark (2014), Lindqvist et al. (2018), Baird et al. (2013), Urbanos-Garrido & Lopez-Valcarcel (2015) or Farré et al. (2018), for instance. However, this instrument is not trouble-free. The problem with the self-reported mental health instruments might be the misreporting or the reporting bias. Individuals tend to overestimate their mental health, as shown by Brown et al. (2018). Their results suggest that GHQ-12 might underestimate the effects of improved economic status. Bond & Lang (2018) also show the difficulties of dealing with this instrument. The European Health Interview Survey (2009) covers nine questions regarding the mental health dimension, the SF36 mental health score developed by RAND Corporation. For the 2014 wave, they use the Patient Health Questionnaire (PHQ-8).

Regarding the measured outcome, and to create a mental health index, I aggregate questions from the instrument for each wave. Then those observations with at least one question unanswered are dropped, which means that only those observations with non-missing information from the mental health measurement instrument are included. The score of the questionnaire is then standardized. In addition, these surveys cover various questions regarding the use of medicine. I create a binary outcome that takes value equal to one of the individual reports being prescribed or is taking medicines for mental illness (I refer to this outcome as a prescription or use of mental illness medicine indistinctly), and a binary outcome if the individual reports at least one visit in the last 12 months to any mental health specialist.

4.3.3 Descriptive Statistics

In order to use comparable individuals, I restrict the analysis to respondents who surveys cover various questions regarding the use of medicine. Table 4.1 displays summary statistics for the restricted sample using the five waves included in the analysis. Health Survey respondents are between 16 and 86 years old, with a mean age of 42 years. Half of the restricted sample are males, and the most reported educational level is elementary education, the level attained by more than a third of surveyed individuals. The share of Spanish citizens is equal to 88 percent. Single or married individuals are nearly 0.9 of the restricted sample. Panel C shows that the share of paid workers or company owners is almost 90 percent. Low skill services account for 41 percent of the sample. Panel E to H report the mean number of other household members in each category. Six out of ten other household members are employed. On average, there is slightly more than one individual within the two lowest educational attainment levels. Finally, households with two or three members represent 51 percent of the sample.

4.4 Empirical Strategy

This section describes the empirical strategy. The identification of the unemployment consequences on mental health is complicated in the absence of longitudinal data. Some differences in mental health between the employed and the unemployed might have existed even before the transition to unemployment. If mental health problems are correlated with job losses, those who suffer a job loss may have lower mental health quality even before the job loss than those who remain employed. For instance, Biasi et al. (2018) find that those individuals with mental health diagnoses have a higher probability of reporting zero earnings. Bubonya et al. (2018) find a bilateral relationship between poor mental health and job losses. Precarious employment could also affect the probability of developing mental health problems, according to Moscone et al. (2016). Therefore, differences in mental health evaluation between unemployed individuals and those who keep their job could be determined, at least partly, previous to the treatment. These pre-treatment conditions might generate a selection bias. Those individuals entering unemployment are those individuals with a higher mental health index before the treatment and have less stable jobs.

First, I perform a selection on observables analysis to compute the differences between employed and unemployed individuals. Using information about past labor market status and past mental health, I control for mental problems before entering the job displacement. In addition, while a large number of papers analyze the effect of unemployment on mental health, I focus the analysis on the long-term effect of the Great Recession on the mental health gap between employed and unemployed individuals. I use a pooled cross-section difference-in-differences model to compare the differences in the gap after 2009 in comparison with the period before. If I assume that observed differences in mental health between employed and unemployed individuals come from differences before the job displacement in addition to the effect of unemployment. Therefore, I capture the differences between employed and unemployed individuals before and after the recession period as the effect of unemployment after the recession.

4.4.1 Selection on Observables (SOO)

To implement the empirical approach, I consider that mental health status depends on a vector of individual characteristics. The primary variables of interest to control for potential endogeneity are indicators for the past labor market status and past mental health. First, I create an indicator that takes value one if the individual reports depression symptoms, anxiety, or other mental health problems in the past 12 months, and zero otherwise. Thus, to deal with mental health differences before treatment, this binary indicator as a proxy of mental health previous to the job loss is included. For past labor market status, ideally, I would like to observe the exact number of months since the job displacement for treated individuals, or employment trajectories as in Mousteri et al. (2018). My best strategy uses as controls those observations that report being employed for at least six months, and as treated those observations that report unemployment status for six months or less. Thus, these restrictions implies that six months before recording the data, all the individuals were employed, while I control for mental health problems in the last twelve months.

4.4.2 Regression Methods

As first approach to the differences in mental health between the employed and unemployed, a regression analysis is used. Specifically, I estimate by OLS the following equation

$$y_i = \delta_0 + \delta_1 D_i + \delta_2 X_i + \delta_3 W_i + u_i \tag{4.1}$$

where y_i is one of the mental health outcomes of individual i. X_i is a vector of covariates for individual i, that include age, gender, education, marital status, a set of labor market controls (occupation and type of contract) and regional dummies. A vector capturing characteristics of household members living with individual i, W_i, includes information about the number of rooms and size of the household, population size of the town of residence, and the number of adults and children in the household. I also include information concerning the educational level, age and labor market status of the members living in the household. The explanatory variable of interest D_i measures if individual i was unemployed or employed. In addition, the causal effect of unemployment on subjective mental health is estimated using the Inverse Probability Weighting (IPW) method (see Hirano et al. (2003)). With IPWs, untreated individuals with a low probability of being treated are weighted up. These weights, estimated using a logistic model, reduce covariate imbalance that arises when treatment assignment is not random. Nonetheless, misspecification of the propensity score can lead to biased estimation of the ATET. A robustness check to potential misspecification is done using a matching method (see Abadie & Imbens (2011)) . I match each treated observation to the n-nearest neighbor according to Mahalanobis's metric and use bias adjustment correction. A problem that arises with matching using Mahalanobis distance is that I have a large set of covariates. Thus, improving the balance of a subset of covariates could led to a worsening of the balance of the rest of covariates. To deal with this curse of dimensionality, I only match on individual characteristics, X_i.

4.4.3 Difference-in-Differences Estimate

While the relationship between unemployment and mental health is well establish in the economic literature, less attention has been paid to the long-term effect of an economic decline in terms of the subjective mental health gap between employed and unemployed individuals. To deal with this question I estimate the causal effect of the economic downturn on the unemployed's subjective mental health. That is, how the business cycle phase moderates the effect of unemployment on mental health. To do so, I pool together all waves of the two surveys and use a repeated cross-section difference-in-differences strategy that exploits the large unemployment rate and low GDP per capita in Spain during the Great Recession.

$$y_{itr} = \gamma + \gamma_1 * unemp_{itr} + \gamma_2 * post_i + \gamma_3 * (unemp_{itr} * post_{itr}) + \gamma_4 * X'_{itr} + \lambda_t + \zeta_r + \epsilon_{itr} \tag{4.2}$$

where $unemp_{itr}$ is a dummy variable for whether individual i in region r and year t is unemployed, $post_{itr}$ indicates whether the survey was completed during an economic downturn (years 2011, 2014 and 2016). Parameter γ_3 is the coefficient of interest, which captures the effect of unemployment on mental health outcomes during an economic downturn with respect to an expansionary period. As data comes from different sources, there are changes between which covariates are included in each wave. Thus, I decided to include only the set of homogeneous covariates featured in all waves. Nonetheless, results are robust to the addition of other covariates. I also include a set of regional dummies and year fixed effects.

This study focus in the period after the 2007-2009 recession compared to the period before 2009. Even when 2009 is considered a recession period, I include it as part of the pre-treatment period because the purpose of this study is to analyze the long-run effects of the Great Recession. Thus, the scars of the 2007-2009 Great Recession might never be observed before 2010.

4.4.4 Counterfactual Distribution

In this section I present the model used to estimate the effect of unemployment on self-evaluated mental health using re-centered influence functions (RIF), as in Firpo et al. (2009). The RIF is a transformation of the dependent variable that allows the coefficient on the dummy variable to recover the effect of changes in density on some aggregate statistics of the outcome distribution. In this case, I use the RIF of the mental health index. I look at the effect of changes in the share of unemployed people on each decile. I present the effect of unemployment shock density for each decile. To do so, I run equation 4.2 using the RIF of the mental health measure as an outcome.

4.5 Results

4.5.1 Selection on Observables

I begin the analysis by estimating the effect of unemployment on the subjective mental health index. Table 4.2 presents the estimated coefficients for the impact of losing a job on mental health. In Panel A I present the cross-section results for the three estimation methods. Results in Panel B include a binary adjustment for past mental health problems for the Inverse Probability Weighting and matching estimates. Panel C also adjusts for past labor market status using the same procedures as in Panel B.

Results in the first row of Panel A indicate that differences between treatment and control group range from 0.15 to 0.34 standard deviations. Result in rows 2 and 3 in Panel A use the same covariates but a different estimation method. However, the results do not show remarkable differences. In order to control for potential confounders, in Panel B I show the results when I include in the model a covariate that adjusts for past mental health. Results now range from 0.11 to 0.28 standard deviations when I use the IPW scheme. Matching results range from 0.17 to 0.34. To put these effects in perspective, job displacement explains around 72 percent of the differences between unemployed and employed individuals in 2006 when I use the IPW. The largest explained percentage corresponds to the 2011 estimation, where I explain 82 percent of the differences. When I use the matching method, I explain between 79 and 92 percent of the differences. Finally, Panel C presents the results adjusting for past employment status. Estimated coefficients are slightly lower than in previous specifications, ranging from 0.11 to 0.26. These results suggest that (i) there exists a clear relationship between labor market status and reported mental health, (ii) results are robust to different specifications and estimation procedure used, (iii) including an adjustment for past mental health status and labor market status reduces the bias from the unadjusted estimation, and (iv) the estimates are slightly larger than those reported in the literature. For instance, Boyce et al. (2018) find that during the period 2009-2010, unemployment caused a reduction of 0.17 standard deviations in well-being. Mousteri et al. (2018) find that unemployment is associated with a 0.017 standard deviations reduction in quality of life, and 0.014 standard deviations decrease in life satisfaction. Ridley et al. (2020b) present a meta-analysis analyzing the effects of cash transfer programs. They find an overall improvement of 0.13 standard deviations on reported mental health.

As European and Spanish surveys use different instruments to measure the mental health, a potential concern is comparability across surveys. Appendix B provides some evidence in favor of comparability between different waves.

4.5.2 Difference-in-Differences Estimates

Table 4.3 presents the results for the repeated cross-section difference-in-differences model. In columns 1 I present the estimated coefficients from equation 4.2, whereas in column 2 I add a set of dummies indicating the occupation and household level covariates. In column 3 I focus on the effect on short-run unemployment. Results in columns 3 and 4 indicate that facing a job displacement during the recession increases the subjective mental health gap by 0.07 - 0.09 standard deviations compared to the pre-recession period. These results range from 38 percent to 50 percent of the cross-section differences between individuals displaced from their jobs and those who remained employed. Results in column 3 indicate that I do not appreciate an increase in the mental health gap between employed and unemployed individuals in the short run.

Turning to medical outcomes, unemployed individuals are three percentage points more prone to taking drugs to deal with mental problems. Nonetheless, the economic crisis reduced the differences in the probability of taking or being prescribed drugs for mental problems by almost two percentage points in comparison with employed individuals. These results are surprising, considering that (i) those who lost their jobs report worse mental health, and (ii) The Great Recession increases mental health differences between treated and untreated individuals. Therefore, I might expect that an increase in the differences of mental health would lead to an increase in the probability of being medicated for mental illness.

Finally, even though those who lose their jobs are more prone to visiting a mental health specialist (the likelihood is one percentage point higher), results indicate that a recession does not increase the likelihood of visiting these above specialists. I entertain two potential explanations for these results. First, individuals might under-report their medical care for mental health problems. These results aligned with those in Bharadwaj et al. (2017), who find that individuals tend to under-report mental diagnosis and prescription drug use regarding mental health. A second potential explanation is that individuals effectively do not ask for diagnoses more often, even when they are facing an impairment of mental health. Both explanations could be related to mental health stigma. Individuals might avoid asking for a diagnosis or even do not recognize that they asked for a diagnosis, in order to avoid being stigmatized.

4.6 Robustness Checks

4.6.1 Robustness Analysis

For the main analysis I perform robustness checks on the model adding household covariates and two different timespans for unemployment. In addition, I perform various different robustness tests to analyze the sensitivity of the results to model specifications. Therefore, I first estimate the model with a minimum set of covariates (age, educational attainment, gender and their interactions). Second, for each pair of region and year, I collapse the data into a unique age-education-gender cell, as in Barbaresco et al. (2015). Then I run the analysis at the cell level. Finally, I provide a leave-one-out analysis. I drop each one of the post-recession periods and run the main specification. Panel A and Panel B in Table 4.3 summarizes findings from this exercise, which are consistent with results in Tables 4.3 in showing that unemployment is associated with a worsening of mental health. For example, in the main setting, I estimate an effect of around 0.07 standard deviations. Results in Table 4.3 ranged from 0.04 to 0.10 standard deviations, in line with previous results. In addition, I find a similar fall in the probability of prescription (around 2 percentage points), and also a lack of effect on the visits to a doctor.

4.6.2 Pre-Recession Trends and Placebo Tests

It might be the case that results are reflecting pre-recession differences in trends between those who lose their jobs and those who do not. A key identification assumption of the Differences-in-difference analysis is that both treatment and control group would have the same trend in the absence of treatment. In this section I provide a pre-recession trends test and a placebo test. First, I consider a pre-recession trends test for the period 2006-2009, before the Great Recession. Ideally, I would like to cover more waves before the Great Recession. However, the 2006 wave was the first to including an instrument to measure subjective mental health. Second, the placebo test consists of using a group of individuals that should not have been affected by labor market conditions. I use retired individuals as the placebo group. Even though they might be affected by some recession consequences, such as reduction of retirement benefits, or stress caused by seeing relatives having to go through hard times, those who are actively working might also be affected by these indirect consequences. Finally, I look at the trends of retired individuals and the group classified as others[1]. The reason for examining trends is that I expect that those groups less affected by The Great Recession should have a trend in common. Therefore, groups actually affected should deviate from those trends. I consider this as evidence of the effect of unemployment on mental health. Nonetheless, it could be the case that some unobserved factors had been affecting only those who kept their job. Under these circumstances, I will not be capturing the effect of unemployment, but rather any other confounder acting over the control group.

Table 4.5 presents the results for these three tests. Columns 1 shows the results for the pre-recession test. Before the recession, the differences were narrowing (estimated effect of -0.06 standard deviations, but large standard errors). These results suggest that, the results are not driven by a pre-existing divergence between groups before the Great Recession. I consider these results

1 those who report that they are homeworkers or in other activities excluding students and the disabled

as evidence in favor of the causal effect during The Great Recession. Columns 2 and 3 report results for the placebo group. I find no effect of the recession on retired people's mental health. I consider that both employed and retired individuals can be affected by economic downturns (for instance, lower wages and retirement benefits) but there are no specific factors affecting one group more than the other, as is the case of unemployed individuals. Finally, figure 4.4 shows the average outcome level for four different groups for period 2009-2016. Note that employed individuals, retired group, and the others display a negative trend in reported mental health, ranging from -0.03 to -0.004. This means that the self-evaluation of mental health was improving for these groups during the period 2009-2016. Unemployed show a positive trend, compatible with an impairment of self-evaluated mental health, although it is not statistically significant. I consider this result as further evidence of how mental health was affected by labor market conditions.

I extend the analysis to the other two outcomes. Results in column 1 in Table 4.5 for mental health medicine use show that the previous results were not driven by differences arising before the recession. When I turn to mental health specialist visits, the results indicates a positive coefficient for the pre-trends test, which indicates that the results from Table 4.3 overestimate the effect. According to the pre-trends analysis, unemployment could reduce both the probability of being prescribed with mental health drugs and visiting a mental specialist. In columns 2 and 3 I report results for the placebo group. I find no effect for the two measures of response to mental health problems. Finally, all the groups show a negative trend in drugs prescription and positive trends for specialist attendance during the period 2009-2016. In summary, the findings of this subsection reinforce the evidence in favor of the negative effect of unemployment on mental illness during The Great Recession, even though I find no evidence of a higher probability of medical attendance related to the impairment of mental health.

4.7 Heterogeneity and Mechanism

4.7.1 Theory and Evidence

The main results assume a homogeneous effect of unemployment on self-evaluated mental health. To account for potentially heterogeneous effects, I carry on three different analyses. First, I divide the sample using whether the individual was in an occupation which suffered from a significant rise in the unemployment rate as a criterium during during the Great Recession. I use the International Standard Classification of Occupations (ISCO) at 1 digit level. Figure 4.7 shows the evolution of the unemployment rate for each occupation. Three occupations illustrated by a dashed line showed a stable path in unemployment rates, whereas the other seven where affected by the Great Recession. This allows my to identify the effect of being in a high unemployment rate occupation. Second, I divide the sample by gender, and finally I differentiate it by educational level. Table 4.6 presents the unemployment rate by gender and educational level. I observe that highly educated individuals experience lower unemployment rates. I do not observe that gap in unemployment conditional on gender. However, males are traditionally seen as breadwinners. Henceforth, unemployment amongst men might mean a remarkable deviation from the social norm.

Specifically, I test the impact of the economic downturn on a set of outcomes by running a triple difference (DDD) model

$$\begin{aligned} y_{itr} = \beta + \beta_1 * unemp_{itr} + \beta_2 * post_{itr} + \beta_3 * (unemp_{itr} * post_{itr}) + \\ \beta_4(unemp_{itr} * group_{itr}) + \beta_5(post_{itr} * group_{itr}) + \\ \beta_6(post_{itr} * group_{itr} * unemp_{itr}) + \beta_7 * X'_{itr} + \lambda_t + \zeta_r + \omega_{itr} \end{aligned} \tag{4.3}$$

where a triple interaction between the unemployment indicator, the Great Recession indicator and the group indicator is included. In this setting, in comparison with equation (4.2), I include the variable $\mathtt{group}_{\mathtt{itr}}$, which takes value equal to 1 for males when I look at gender, for the highly educated when I look at educational attainment, and for occupations with a high rate of unemployment when I look at occupation classification, and 0 otherwise. I next provide a series of placebo tests to evaluate if the previous results are caused by unemployment or if I are just capturing some differential trends. I also estimate equation (4.3) for pre-recession period (2006-2009) as in subsection (4.6.2).

Table (4.6) reports the results for each subsample. Columns (1), (3) and (5) present the estimated coefficients from the whole sample, whereas columns (2), (4) and (6) present the results for the pre-recession period. Those unemployed people that were working in occupations that were less affected by the Great Recession present a larger effect (0.07 standard deviations), albeit estimates are insignificant. Highly educated individuals, a group that might be less affected by recessions, present a large effect in comparison with individuals with a low level of education (0.11 standard deviations). In line with these results, I might expect that males would report a higher effect than females. Results in column 5 indicate that the impact is larger for females, although the difference is not statistically significant. However, if I consider that pre-trends indicate what would happen in the absence of treatment, I should take into account these results when I interpret the estimated coefficients. Results for the pre-trends test by gender indicates that before the recession, differences in the effect on males were 0.12 standard deviations lower. If after the recession the difference is reduced to around 0.03, it can be consider as evidence of a higher effect on males, leading to a reduction in differences, which is in line with the results for the rest of the subgroups. Turning to pre-recession tests regarding the probability of being prescribed with drugs for mental illness, and visiting an specialist for mental health problems, all of the test are relatively small and imprecisely estimated. Therefore, I reject the idea of results being driven by pre-treatment trends.

The mechanism behind these results relies on how individuals weight the evaluation of their own perspectives and the deviation from social position. For instance, individuals that belong to a group with a low unemployment rate should have better perspectives for the future. However, they present a larger deviation from the social norm. Thus, if the latter dominates the former, being in a group with a low unemployment rate may increase the impact of losing a job on mental self-evaluation, because individuals give less importance to their own perspectives for the future in comparison with the social norm. Following Akerlof & Kranton (2000), those individuals that differ the most from social norms might experience higher utility losses. In that sense, being unemployed in those groups with the lowest unemployment rate might be understood as a deviation from the social norms, which can affect their own life evaluation.

4.7.2 Distributional Rank

I check another potential source of heterogeneity: the position in distribution of mental health. Those individuals located in the bottom part of distribution, representing the highest quality of mental health, might be affected differently than those individuals with lower levels of mental health. Therefore, a deeper insight into the causal relationship between unemployment and mental well-being requires an additional analysis. In this section, for each year of the data, I estimate the effect of a spell of unemployment on unconditional mental health distribution, accounting for the changing position of unemployed members in counterfactual workers mental health distribution. Estimating counterfactual distribution, I can compare the differences in mental health index for each decile. To do so, I run equation 4.2 again, where the dependent variable is the recentered influence function of the mental health index.

Figure 4.9 plots the unemployment effect across mental health distribution. I find that for those individuals located below the median in distribution, the effect is ambiguous: the effect is lower than the average affect, and sometimes indistinguishable from zero. Those individuals in the median display an effect close to the average treatment effect, around 0.09 standard deviations. Finally, for those individuals in the highest decile, the effect is around 0.12 standard deviations, slightly higher than the average treatment effect. In general, figure 4.9 shows that the higher the mental illness index, the larger the effect. A feasible explanation of these results is that those with worst mental health are those who enter unemployment, and those who suffer the largest effect when they are unemployed. This might affect their employability, thus creating then an unemployment trap.

4.8 Conclusions and Discussion

This chapter examines the relationship between labor market status and mental health during the Great Recession. First, I provide evidence on how an unemployment shock impairs self-assessed mental health. Key finding is that unemployment is systematically associated with higher subjective mental health index scores, which means a lower quality of mental health. Furthermore, the results indicate that, after the 2007-2009 recession, the mental health gap between employed and unemployed individuals increased by around 0.10 standard deviations, which accounts for 38 percent of the cross-sectional differences between both groups.

I also show how the estimated effect is reduced when I move from selection on observables methods to a quasi-experimental design. Regarding unobservable variables leading the bias, I suggest that those individuals with lower mental health quality are more prone to losing their the job. Findings suggest that, even when mental health is affected, there is no response in terms of medical behavior. In contrast, unemployment may reduce the probability of being prescribed or taking medicines for mental illness and the odds of visiting a specialist for mental illness. A potential explanation for these results is that affected individuals may refuse to visit a doctor as a consequence of the existing stigma surrounding mental health.

Then I turn to the mechanisms behind worsening mental health. Theoretically, deviations from the social norm could affect life evaluation. In particular, being unemployed can be understood as a deviation from the social norm regarding labor market status. Individuals might feel that they are useless for society or feel that they are excluded. Moreover, the social surrounding might be related to the results. In a context with high unemployment rates, I present two hypotheses. The first hypothesis, if the social surrounding is more weighted than their situation, large unemployment rates might mitigate the unemployment effect on mental health. In this scenario, unemployment might be seen as a general problem, so the deviation from the social norm is lower, reducing the feelings of guilt. The second hypothesis is that individuals are more concerned about their own perspectives and they consider less relevant their position regarding the social norm. Under this hypothesis, larger unemployment rates are associated with worse foresight and has a considerably higher impact on mental health.

I test this hypothesis by running the analysis in different subsamples. I compare the unemployment effect (i) for those in occupations with a high and low unemployment rate, (ii) by gender, and (iii) by educational level. Individuals in occupations with a low unemployment rate, males, and the highly educated may have better future perspectives, but they also suffer a larger deviation from the social norm if they lose their jobs. Results suggest that the deviation from the social norm may matter more than the perspectives. Highly educated individuals and workers in low unemployment occupations show a larger unemployment effect than lowly-educated individuals and workers in low unemployment rate occupations. I also discuss the results for males, where the lack of clear evidence can also be interpreted taking into account existing pre-recession trends. These results might be considered as evidence supporting the second hypothesis. Finally, I derive the counterfactual distribution of the subjective mental health index under no treatment. I show that the estimated effect is larger for those individuals with the lowest mental health quality.

These results are reliable for policymakers. Empirical evidence favors the idea that those who have the worst mental health are the first to lose their job. This reduces mental health, affecting employability, which could lead to an unemployment trap. Mental health screening and early detection of psychological distress might be a potential policy oriented to reducing unemployment.

4.9 Figures

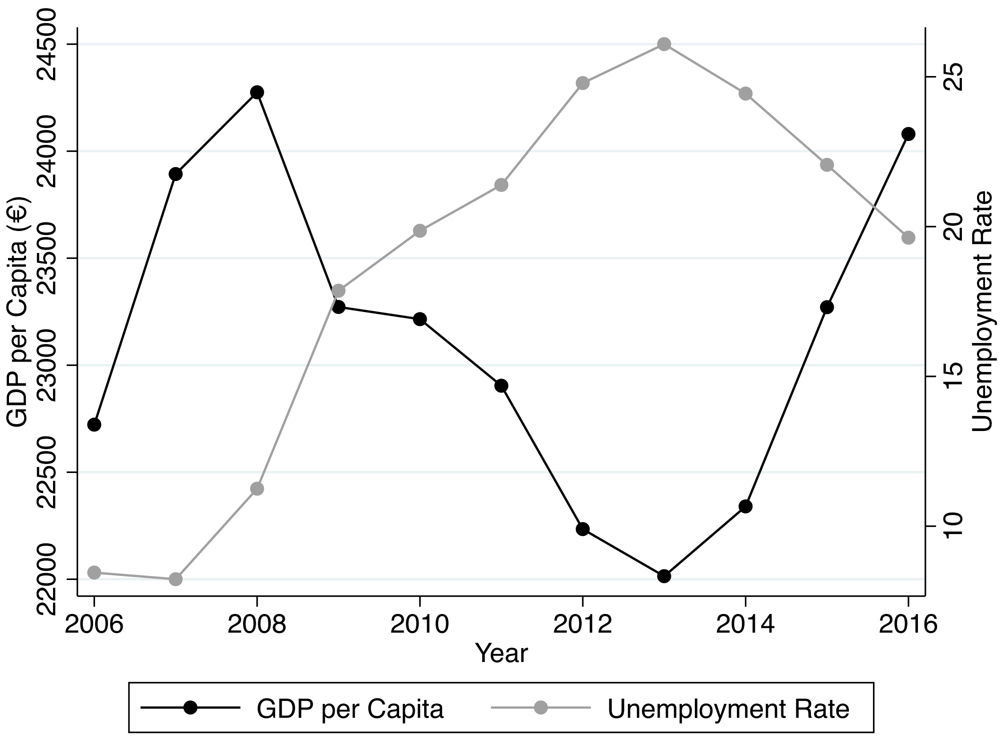

Figure 4.1: Spanish GDP per Capita and Unemployment Rate

Notes: Figure shows Spanish unemployment rate and GDP per capita during the period 2006-2016. *Source*: Survey of active population from Spanish National Bureau of Statistics.

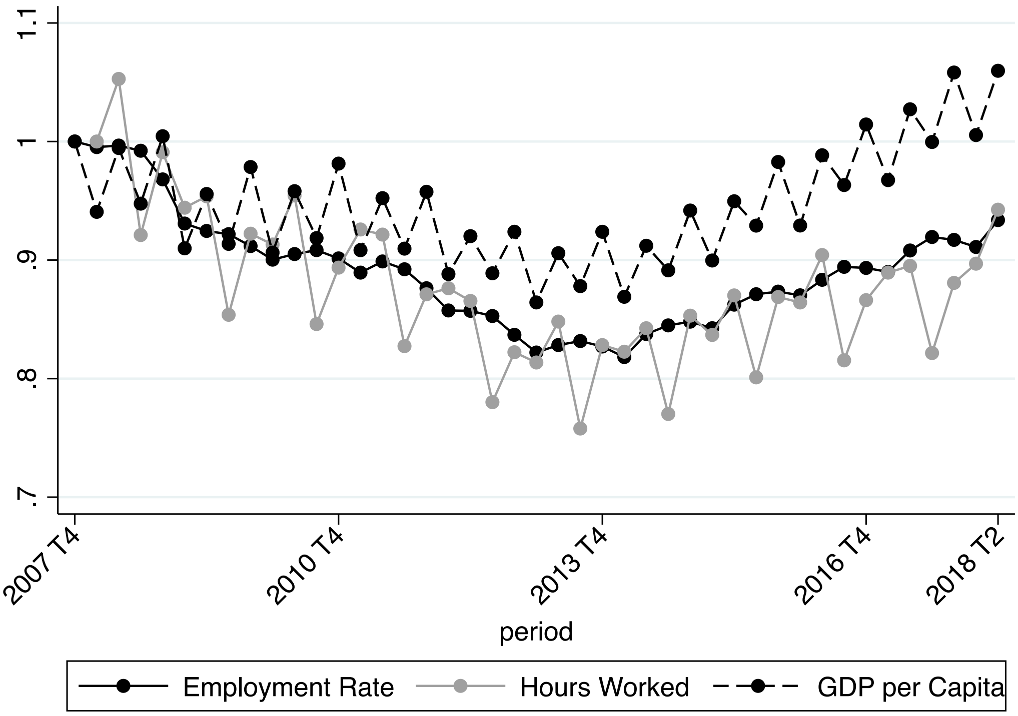

Figure 4.2: EVOLUTION OF EMPLOYMENT, WORKED HOURS AND GDP PER CAPITA

Notes: Figure shows the Spanish employment rate, hours worked and GDP per capita. All the values normalize to 1 in last trimester of 2007. *Source*: Survey of active population from Spanish National Bureau of Statistics.

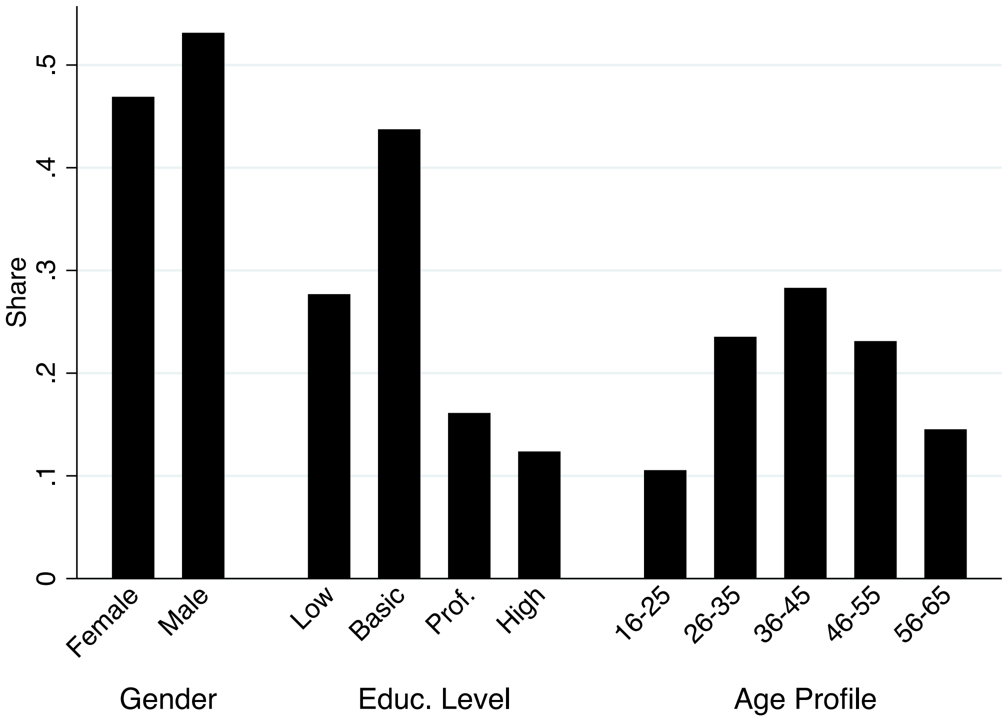

Figure 4.3: UNEMPLOYED PROFILE

Notes: Figure shows the share of unemployed individuals in each bin among each category.

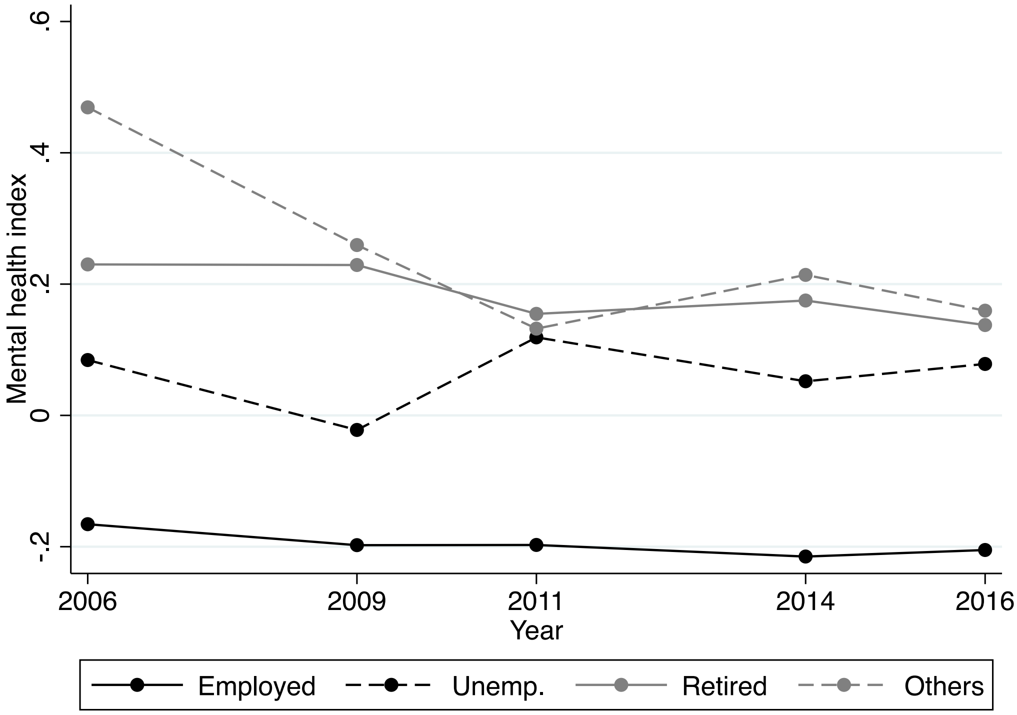

Figure 4.4: MENTAL HEALTH TRENDS

Notes: Figure shows trends in subjective mental health for four different groups: employees, unemployeds, retired people, and others. Subjective mental health is measured in standard deviations.

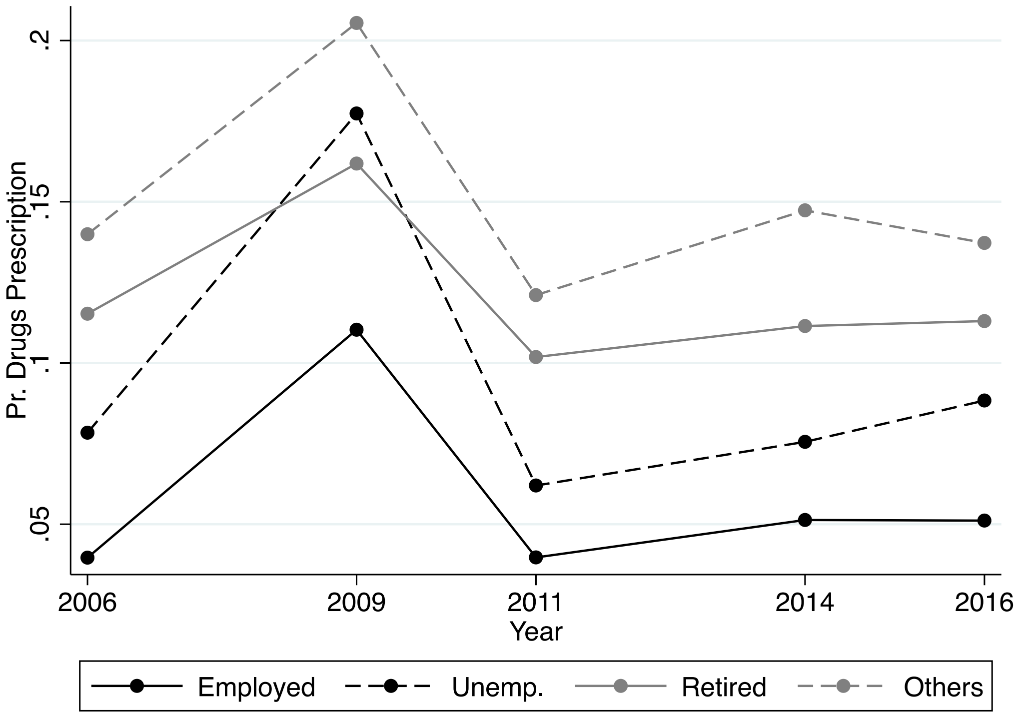

Figure 4.5: TRENDS IN PRESCRIPTION OF DRUGS FOR MENTAL HEALTH PROBLEMS

Notes: Figure shows the fraction of individuals in each category being prescribed or taking drugs for mental illness.

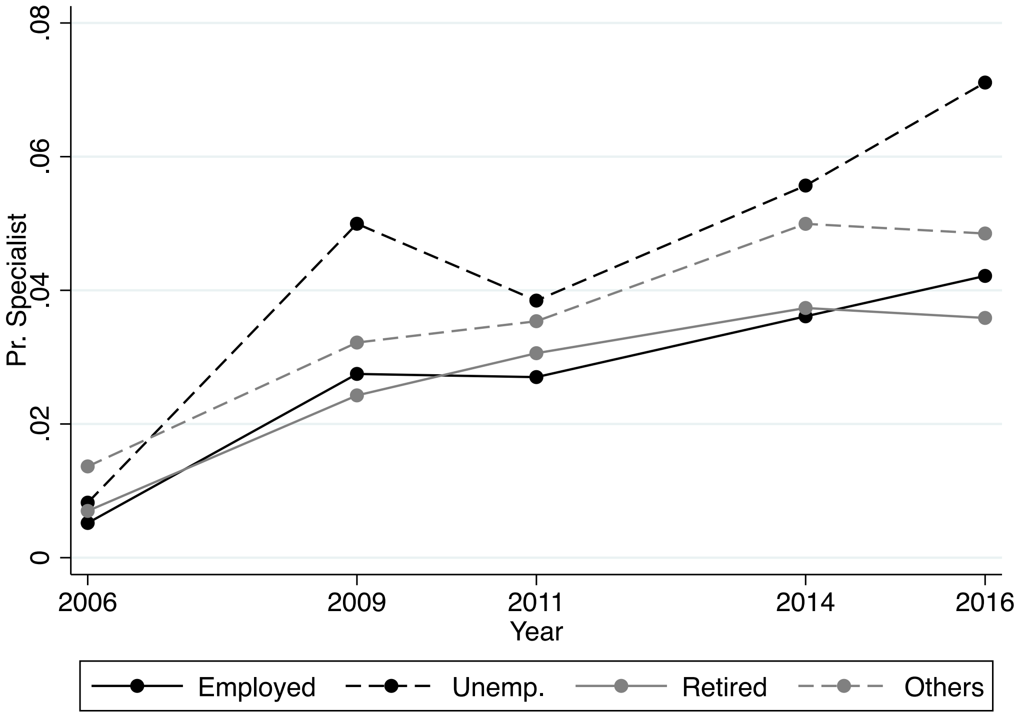

Figure 4.6: TRENDS IN SPECIALIST FOR MENTAL HEALTH PROBLEMS

Notes: Figure shows the fraction of individuals in each category visiting a mental health specialist.

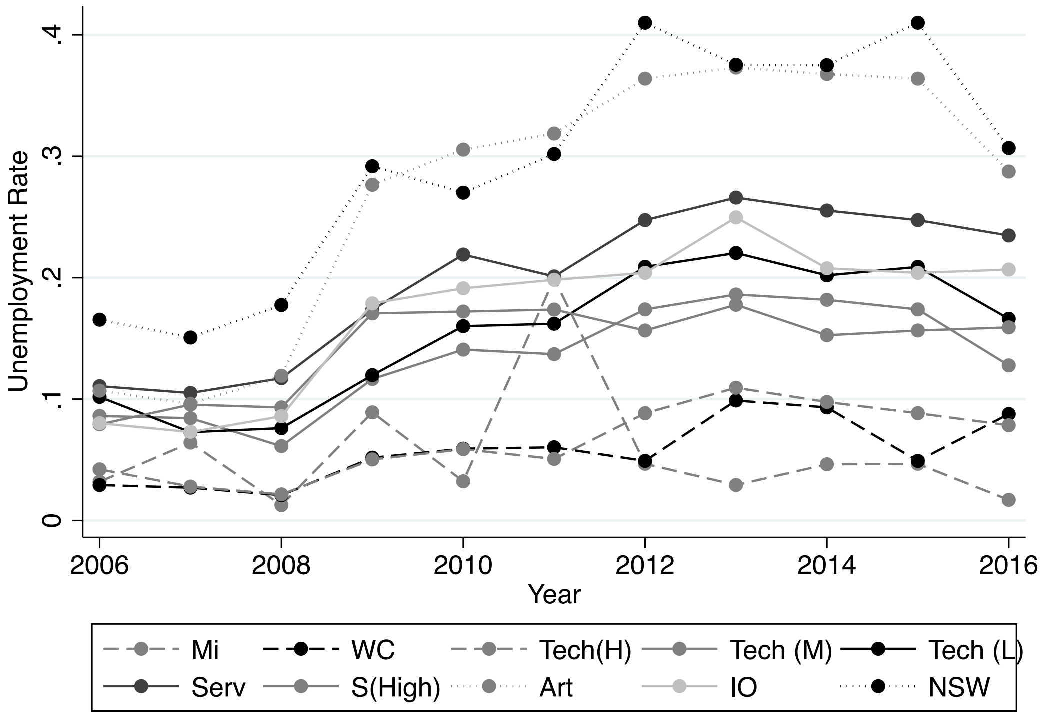

Figure 4.7: UNEMPLOYMENT RATE BY OCCUPATION

Notes: Table shows the evolution of the unemployment rate for 10 occupations. I follow the ISCO at 1 digit level. Dashed lines account for low affected occupations: military employees, withe collar workers, and technological sector employees in high skill positions. Straight lines plot medium affected occupations: Technological workers (medium and low skill), services, and industry workers. Dots represent highly affected occupations: manufacturers, artisans, and non skilled workers. Mi refers to militaries. WC refers to white collar workers. Tech(H, M or L) refers to individuals working in the technological sector in high, medium or low skill occupations. Serv. refers to individuals working in service sector. S(high) accounts for workers in high skill services. Art defines individuals in artistic occupations. Finally, IO and NSW refers to industrial operators and non skilled workers, respectively. *Source*: Survey of Income and Living conditions from Spanish National Bureau of Statistics

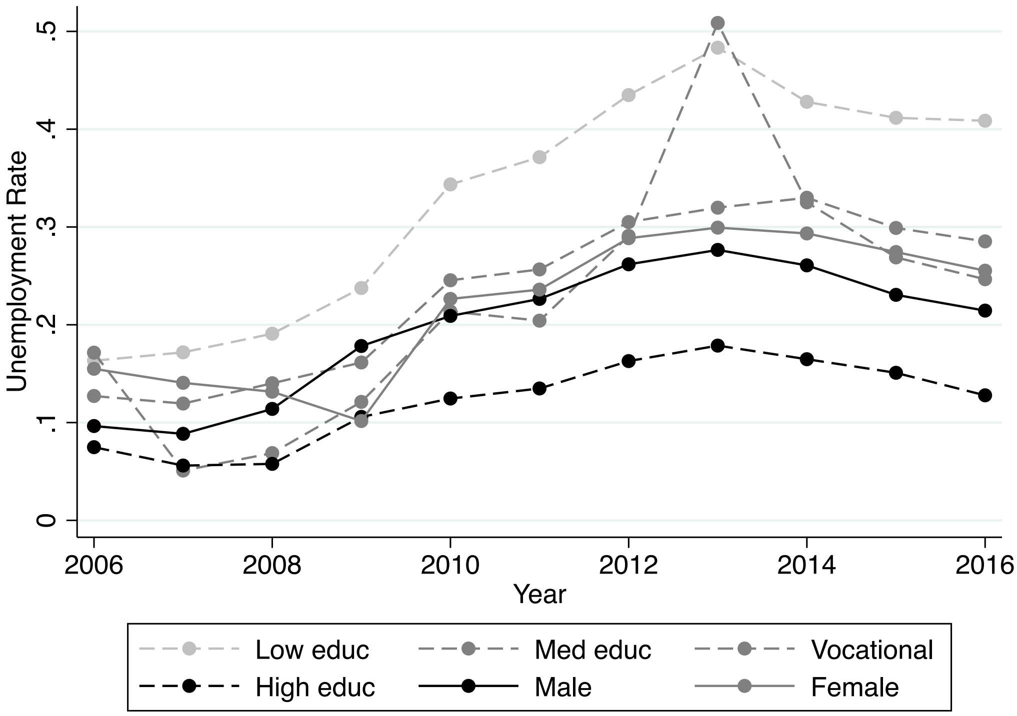

Figure 4.8: UNEMPLOYMENT RATE BY GENDER AND EDUCATIONAL ATTAINMENT

Notes: The figure shows the unemployment rate by educational level and by gender. *Source*: Survey of Income and Living conditions from Spanish National Bureau of Statistics

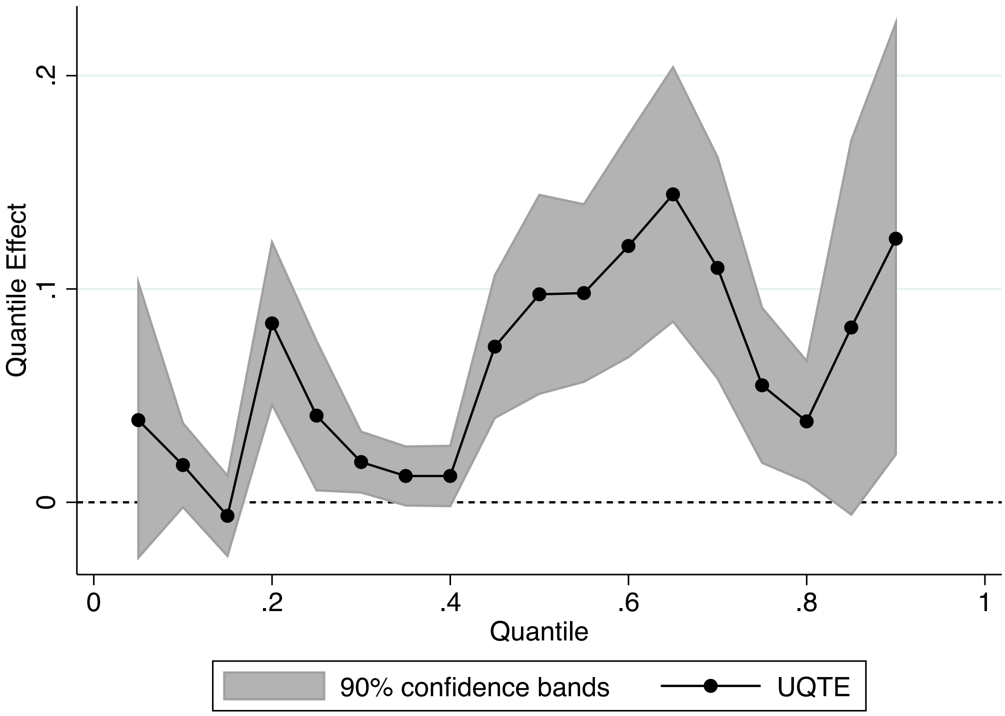

Figure 4.9: UNCONDITIONAL QUANTILE EFFECT

Notes: Figure shows estimated treatment effect for individuals at each point of the mental health index distribution. The dark shaded area represents 90% bootstrapped confidence intervals.

4.10 Tables

Table 4.1: DESCRIPTIVESTATISTICS

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.
Panel A: Basic Covariates				
Age	42.4049	10.9713	16	86
Gender	0.5128	0.4998	0	1
Education (low)	0.1975	0.3981	0	1
Education (medium)	0.3684	0.4823	0	1
Education (vocational)	0.1911	0.3931	0	1
Education (tertiary)	0.2408	0.4276	0	1
Spanish	0.8850	0.3189	0	1
Panel B: Marital Status				
Single	0.3152	0.4646	0	1
Married	0.5775	0.4939	0	1
Widow	0.0192	0.1375	0	1
Divorced	0.0879	0.2832	0	1
Panel C: Labor Relationship				
Paid worker	0.6693	0.4704	0	1
Owner	0.2660	0.4418	0	1
Cooperatives	0.0197	0.1392	0	1
Others	0.0411	0.1985	0	1
Panel D: Occupation				
Management	0.0557	0.2294	0	1
Science / techs	0.2399	0.4270	0	1
Security & Health	0.0532	0.2246	0	1
agriculture, Blue Collar	0.2306	0.4212	0	1
Services (low skills)	0.4173	0.4931	0	1

DESCRIPTIVE STATISTICS (continued)

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.
Panel E: Household Members Labor Market Status				
Employed	0.6102	0.6211	0	8
Unemployed	0.1575	0.4248	0	7
Student	0.4680	0.7814	0	7
Retired	0.0916	0.3180	0	3
Others	0.5015	0.8517	0	10
Panel F: Household Members Educ. category				
Not Categorized	0.3917	0.6930	0	8
Primary education or less	0.5520	0.8550	0	10
Secondary education	0.4862	0.7317	0	7
Vocational studies	0.2056	0.4493	0	5
Higher education	0.1892	0.4403	0	9
Panel G: Household Members Age				
From 0 to 25	0.8422	0.9465	0	8
From 26 to 35	0.2386	0.4709	0	6
From 36 to 45	0.2655	0.4620	0	4
From 46 to 55	0.2393	0.4703	0	4
From 56 to 65	0.1475	0.3993	0	4
65 or older	0.0956	0.3503	0	4
Panel H: Others				
Town size	2.9034	1.9057	0	6
M2	102.1613	49.7795	5	997
N Rooms	3.0053	0.8728	0	15
Adults	1.9860	1.0581	0	11
Minors	0.6316	0.8668	0	8

Notes: The sample consists of all employees and the unemployed in Health surveys for the periods 2006, 2009, 2011, 2014, and 2016.

Table 4.2: RELATIONSHIP BETWEEN UNEMPLOYMENT AND SUBJECTIVE MENTAL HEALTH

	(1) 2006	(2) 2009	(3) 2011	(4) 2014	(5) 2016
Panel A: Unadjusted model					
OLS	0.2214***	0.1551***	0.3435***	0.2436***	0.2571****
	(0.0362)	(0.0315)	(0.0415)	(0.0249)	(0.042)
IPW	0.2154***	0.1615***	0.3391***	0.2477***	0.2481***
	(0.0353)	(0.0240)	(0.0272)	(0.0323)	(0.0291)
Matching	0.2597***	0.1842***	0.3141***	0.2478***	0.2441***
	(0.0360)	(0.0259)	(0.0292)	(0.0297)	(0.0305)
Panel B: Adjusted model					
IPW	0.1603***	0.1144***	0.2862***	0.1855***	0.1989***
	(0.0326)	(0.0227)	(0.0258)	(0.0315)	(0.0274)
Matching	0.1789***	0.346***	0.2701***	0.1875***	0.2106***
	(0.0330)	(0.0244)	(0.0278)	(0.0287)	(0.0288)
Observations	14,108	11,833	10,129	10,955	10,894
Share T.	0.0845	0.1911	0.1592	0.1328	0.1136
Panel C: Short-term unemployment					
IPW	0.1452***	-	0.2655***	0.1140	0.1593***
	(0.0431)		(0.0521)	(0.0842)	(0.0502)
Matching	0.1435***	-	0.2644***	0.1219***	0.1559***
	(0.0402)		(0.0568)	(0.0467)	(0.0487)
Observations	3,188	-	1,410	1,565	1,356
Share T.	0.2311		0.5312	0.3948	0.4896

Notes: All regressions include regional fixed effects. Share T. accounts for the share of treated observations in each combination of year and specification. All regressions control for age, educational level, gender (and their interactions), marital status and occupation. At household level I adjust for age profile, educational levels and labor market composition, city size, number of rooms, and house size. 2009 estimations does not adjust for unemployment length, household educational level, city size, number of rooms and house size. 2014 does not adjust for number of rooms and house size due to lack of information. Matching includes bias adjustment for all the covariates. Standard errors for OLS model are clustered at regional level. One, two and three starts indicate significance at ten, five and one per cent.

Table 4.3: EFFECT OF THE RECESSION ON SUBJECTIVE MENTAL HEALTH

	(1) Standard	(2) Saturated	(3) short
Panel A: Mental health			
Treatment	0.0998***	0.0770***	0.0182
	(0.0256)	(0.0256)	(0.0383)
Unemployment	0.1832***	0.1810***	0.1568***
	(0.0211)	(0.0201)	(0.0225)
Panel B: Drugs			
Treatment	-0.0173***	-0.0190***	-0.0256**
	(0.0052)	(0.0051)	(0.0092)
Unemployment	0.0325***	0.0321***	0.0306***
	(0.0036)	(0.0038)	(0.0050)
Panel C: Medical attendance			
Treatment	0.0067	0.0049	-0.0031
	(0.0069)	(0.0067)	(0.0068)
Unemployment	0.0127***	0.0137***	0.0174***
	(0.0041)	(0.0040)	(0.0049)
Observations	58,262	58,262	19,315

Notes: All regressions include region and fixed effects. All regressions control for age, educational level, gender, their interactions, and marital status. The saturated model also includes household members profile and occupation at 1 digit level. 2009 estimations do not adjust for unemployment length due to lack of information. At region level clustered standard errors in parentheses. One, two and three starts indicate significance at ten, five and one per cent.

Table 4.4: ROBUSTNESS CHECKS OF UNEMPLOYMENT ON SUBJECTIVE MENTAL HEALTH

	(1)	(2)	(3)
Panel A: Alternative Design			
	Parsimonious	Collapsed	
Mental Health	0.1007***	0.1096***	
	(0.0338)	(0.0341)	
Drugs	-0.0170***	-0.0131**	
	(0.0054)	(0.0066)	
Medical	0.0069	0.0087	
	(0.0062)	(0.0062)	
.			
Observations	58,313	26,909	
Panel B: Leave one out design			
	2011	2014	2016
Mental Health	0.0441	0.1023**	0.0841**
	(0.0345)	(0.0402)	(0.0373)
Drugs	-0.0191***	-0.0162**	-0.0208***
	(0.0066)	(0.0061)	(0.0057)
Medical	0.0086	0.0051	0.0017
	(0.0075)	(0.0064)	(0.0063)
Observations	48,139	47,363	47,387

Notes: All regressions include the unemployed between 0 and 24 months and region-year fixed effects. 2009 estimations do not adjust for unemployment length due to lack of information. Parsimonious model only include age, gender and educational level and their interactions. The collapsed model includes data at educational level-age-gender-unemployment status cell. Leave-one-out analysis drops one wave in each estimation. At region-year level clustered standard errors in parentheses. One, two and three starts indicate significance at ten, five and one per cent.

Table 4.5: PLACEBO TEST

	(1)	(2)	(3)	(4)
Panel A: Pre-trends and Placebo Group				
	Pre-trends	Placebo Group		
	2006-2009	2009-2016	2006-2009	
Mental Health	-0.0639	-0.0461	0.0287	
	(0.0468)	(0.0314)	(0.0470)	
Drugs	0.0021	-0.0095	0.0227**	
	(0.081)	(0.0059)	(0.0091)	
Medical	0.0211***	0.0018	-0.0043	
	(0.0072)	(0.0024)	(0.0038)	
Observations	26,365	60,630	35,130	
Panel B: Trends (2009-2016)				
	Employed	Unemployed	Retired	Others
Mental Health	-0.0049	0.01011	-0.0124	-0.0370*
	(0.0134)	(0.0186)	(0.0179)	(0.0197)
Drugs	-0.0058***	-0.0132***	-0.0061**	-0.0120***
	(0.0012)	(0.0028)	(0.0029)	(0.0031)
Medical	0.0050***	0.0045	0.0048***	0.0072***
	(0.0008)	(0.0030)	(0.0011)	(0.0015)
Observations	37,159	6,559	23,534	9,375

Notes: Pre-trends are estimated using equation 4.2 for waves 2006 and 2009. Retired individuals are used as placebo group. I estimate 2009-2016 trends using a OLS regression. 2009 estimations do not adjust for unemployment length due to lack of information. At region-year level clustered standard errors in parentheses. One, two and three starts indicate significance at ten, five and one per cent.

Table 4.6: EFFECT OF RECESSION ON SUBJECTIVE MENTAL HEALTH, BY OCCUPATION, GENDER, AND AGE

	(1)	(2)	(3)	(4)	(5)	(6)
	Occupation		Educational level		Gender	
	All periods	2006-2009	All periods	2006-2009	All periods	2006-2009
Mental Health	0.0720	-0.0962	0.1165*	-0.0721	-0.0348	-0.1213*
	(0.0755)	(0.1043)	(0.0651)	(0.0997)	(0.0425)	(0.0689)
Drugs	-0.0101	-0.0168	0.0358*	-0.0021	-0.0017	-0.0169
	(0.0228)	(0.0423)	(0.0141)	(0.0202)	(0.0097)	(0.0157)
Medical	0.0028	-0.0007	0.0064	-0.0057	-0.0060	0.0002
	(0.0178)	(0.0253)	(0.0153)	(0.0204)	(0.0086)	(0.0112)
Observations.	58,082	26,152	58,313	26,152	58,313	26,152

Notes: I report the effect for each subgroup. Triple differences model reports the differences in those effects. All regressions include unemployed between 0 and 24 months and region-year fixed effects. 2009 estimations do not adjust for unemployment length due to lack of information. Leave-one-out analysis drops one wave in each estimation. At region-year level clustered standard errors in parentheses. One, two and three starts indicate significance at ten, five and one per cent.

Appendix A. Mental Health Distribution

In the event study analysis, I use different health surveys which provides information regarding a set of questions related to mental health. I use these questions to construct the SMH index. As preliminary evidence of differences in SMH index between employees and unemployed members, I present the outcome distribution for both control and treated members. I use a Kernel density estimation. Figure A1 shows that the shape of the SMH index distribution changes according to the instrument used to measure subjective mental health. Waves from 2006, 2011 and 2016 appear to be bimodal distribution and positive Skew. Wave from 2009 is similar to normal distribution with positive skew. However, the wave from 2014 is similar to Poisson distribution with $\lambda = 1$. The particularity of the 2014 dataset is that a lot of individuals report the lowest value for the SMH index (i.e. highest mental quality).

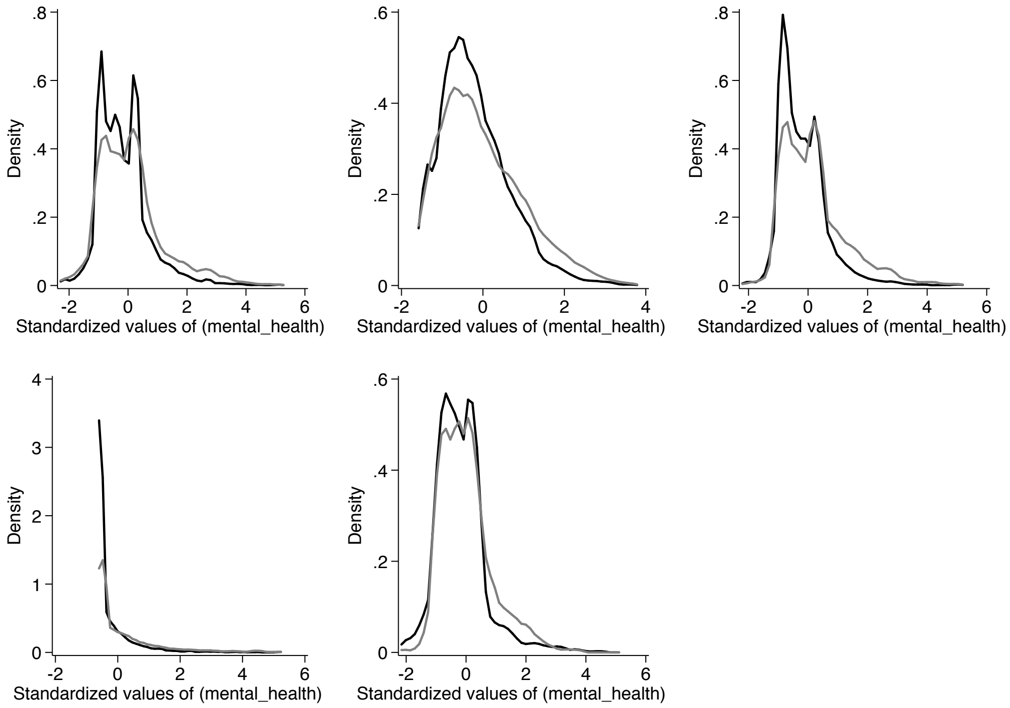

Figure A1: Mental Health Distribution

Appendix B. Longitudinal Analysis

This Appendix provides a comparison of the estimated effects for different waves. In previous analyses, I estimate the effect of unemployment measured in standard deviations were the outcome is standardizes using the mean value of the year. It might be the case that a standard deviation of the population varies from one wave to the next. To test this hypothesis, I focus on the 2006, 2011, and 2016 waves. These three waves use the GHQ-12 as an instrument to measure the SMH. This will allow me to compare the effect of unemployment at different points in the business cycle. Furthermore, these three waves are a reference to points before, during, and after the Great Recession. In more detail, I standardize the SMH index using the 2006 mean and standard deviation, even for the 2011 and the 2016 waves. By doing so, I measure the effect of each year on the standard deviations of 2006, which gives comparable results. Then I just estimate the IPW model. In columns 1 and 2 I report the mean value and standard deviation of the SMH index for both treated and control group. Wave 2006 does not fit to mean equal to 0 and standard deviation equal to 1 because I report the values for the restricted sample. Descriptive values for waves 2011 and 2016 indicate that, on average, employed mental health improves for each wave. Meanwhile, the unemployed members report a stable SMH in each wave. Results in column 3 of Table B1 indicate a negligible difference between using each wave's standard deviation or a the baseline year's standard deviation.

Table B1: Longitudinal Analysis of the Impact of Unemployment

	(1)	(2)	(3)
Year	Control	Treated	IPW
2006	-0.1509	0.0427	0.1569***
	(0.8823)	(1.0757)	(0.0434)
2011	-0.1824	0.0441	0.2997***
	(0.8608)	(1.0167)	(0.0522)
2016	-0.2276	-0.0406	0.1739***
	(0.8193)	(0.8587)	(0.0520)

Notes: Effects measured in standard deviations of year 2006. A logit model is estimated for the treatment probability. Standard errors in parentheses. One, two and three starts indicate significance at ten, five and one per cent.

Appendix C. Normalized differences

Assessing causality implies a comparison between what I observe under treatment, and what I would observe in the case of no treatment. As the latter case never happens, I use untreated observations to construct the counterfactual. The causal estimation is as good as the counterfactual case. Thus, the group used to construct the counterfactual should be as similar as possible to the treated group. To check this, I compare the normalized differences in covariates on both groups.

This appendix provides information regarding the size of the normalized differences for each year. I present the differences for both control and treatment group before applying any procedure, after matching and after weighting. The identification of the influence of unemployment in mental illness rests on the assumption that both control and treatment groups are exchangeable. This means that if I switch treatment statuses, the ATET would remain the same. Under this assumption, using the outcome from the control group as the counterfactual outcome for treated is fair. Unfortunately, this assumption is not testable. However, if control units provide a reasonable counterfactual, treatment and control groups should not differ considerably in their characteristics. As in Abadie & Imbens (2011) and Imbens (2015), I assess covariate overlap using the normalized mean differences between treated and control groups. Tables C1-C10 show normalized mean differences. Columns (1) to (3) report statistics for the individual covariates, and Columns (4) and (5) for the aggregated covariates. Columns (1) and (4) show the normalized differences for the full samples. Column (2) displays the normalized differences after trimming the sample, keeping the treated and matched untreated observations. Columns (3) and (5) present the normalized differences after weighting the covariates according to the IPW scheme. Blank spaces indicate dropped covariates from a set of dummy variables. Column (2) indicates that both groups have balanced individual characteristics after the matching. Only the dummy indicating if the observed individual is a paid worker is above the 0.3 threshold (0.31). Column 4 indicates that 3 covariates overcome the threshold. The normalized difference in the number of unemployed members is 0.31. When I compute the normalized differences after reweighting the sample, I find again that only the covariate for paid workers is above the 0.3 threshold (0.31). I also find that none of the household level covariates are above the threshold. Finally, 31 out of 33 covariates show a lower normalized difference after reweighting.

Table C1: NORMALIZED DIFFERENCES IN 2006 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Matched	(3) Weighted
Panel A: Basic Covariates			
Education	-0.1231	0.0550	0.0034
Gender	0.0101	0.0902	0.0097
Age	0.0921	0.0442	0.0105
Spanish	0.1804	-0.0619	-0.0020
Panel B: Marital status			
Single	0.0181	0.0320	0.0053
Married	-0.0525	-0.0767	-0.0115
Widow	0.0818	0.01424	0.0098
Divorced	-	-	-
Panel C: Labor relationship			
Paid worker	0.1384	0.0132	0.0052
Owner	0.1127	-0.0156	-0.0040
Cooperatives	-0.0552	0.0000	0.0012
Others	-	-	-
Panel D: Occupation			
Management	0.0950	0.0136	0.0183
Science / techs	-0.1797	-0.004	-0.0001
Security & Health	-0.0690	0.0069	-0.0027
Agriculture, Blue Collar	-0.0219	0.0728	0.0056
Services (low skills)	-	-	-

Table C2: NORMALIZED DIFFERENCES IN 2006 (HOUSEHOLD COVARIATES)

	(1) Raw	(2) Weighted
Panel A: Labor Market Status		
Employed	-0.1454	-0.0133
Unemployed	0.0674	-0.0052
Student	-0.0264	-0.0006
Retired	0.1467	0.0000
Other	-0.0689	-0.0103
Panel B: Educational level		
Low	0.03268755	-0.00927975
Basic	-0.08469893	-0.00870378
Professional	-0.08345594	0.00094084
High	-0.09268522	-0.005299
	-	-
Panel C: Age		
<25	-0.0936	-0.0088
25-35	-0.0430	-0.0064
35-45	-0.0423	-0.0090
45-55	-0.0421	-0.0058
55-65	0.0656	-0.0015
>65	-	-
Panel D: Others		
Size of city	0.0753	-0.0024
M2	-0.0960	-0.0066
Rooms	-0.0510	-0.0064
Adults	-0.0411	-0.0118
Minors	-	-

Table C3: NORMALIZED DIFFERENCES IN 2009 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Matched	(3) Weighted
Panel A: Basic Covariates			
Education	-0.4607	-.0225	-0.0079
Gender	-0.0781	0.0055	0.0170
aAge	-0.2195	-0.0342	0.0017
Spanish	-0.1567	-0.0260	0.0162
Panel B: Labor relationship			
Paid worker	0.4538	-0.0060	0.0035
Owner	-0.2679	0.0033	-0.0007
Cooperatives	-0.3212	0.0058	-0.0020
Others	-0.0857	-	-0.0000
Panel C: Occupation			
Management	-0.2557	0.0041	-0.0008
Science / Techs	-0.3985	0.0133	0.0006
Security & Health	0.03974	0.0169	-0.0044
Agriculture, Blue Collar	0.1482	0.0065	0.0007
Services (low skills)	-	-	-

Table C4: NORMALIZED DIFFERENCES IN 2009 (HOUSEHOLD COVARIATES)

	(1) Raw	(2) Weighted
Panel A: Labor Market Status		
Employed	-0.1910	-0.0029
Unemployed	0.3415	0.0050
Student	-0.0713	0.0100
Retired	0.0728	-0.0029
Other	0.03086	0.0051
Panel B: Age		
<25	-0.0046	0.0032
25-35	0.0151	0.0035
35-45	-0.0407	0.0007
45-55	-0.0031	0.0096
455-65	0.0564	0.0085
>65	-	-
Panel C: Others		
Size of city	-0.0518	-0.0117

Table C5: NORMALIZED DIFFERENCES IN 2011 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Matched	(3) Weighted
Panel A: Basic Covariates			
Education	-0.2915	-0.1006	-0.0098
Gender	0.1940	0.1882	0.0629
Age	0.1027	0.1269	0.0232
Spanish	0.0533	-0.1074	0.0054
Panel B: Marital status			
Single	-0.0482	-0.0667	0.0023
Married	0.04711	-0.0122	-0.01632
Widow	-0.0562	0.0172	-0.0029
Divorced	-	-	-
Panel C: Labor relationship			
Paid worker	-0.3137	-0.3137	-0.31289
Owner	0.2844	0.2844	0.2836
Cooperatives	-	-	-
Others			
Panel D: Occupation			
Management	0.1151	0.0520	0.0382
Science / techs	-0.3230	-0.0121	0.02687
Security & Health	-0.1031	0.0474	-0.0219
Agriculture, Blue Collar	0.2813	0.1492	0.0352
Services (low skills)	-	-	-

Table C6: NORMALIZED DIFFERENCES IN 2011 (HOUSEHOLD COVARIATES)

	(1) Raw	(2) Weighted
Panel A: Labor Market Status		
Employed	-0.1075	-0.0184
Unemployed	0.1327	-0.0176
Student	0.0752	0.0140
Retired	0.0585	0.0069
Other	0.0284	0.0118
Panel B: Educational level		
Low	0.1428	-0.0266
Basic	0.0921	0.0215
Professional	-0.1392	0.0138
High	-0.1171	0.0144
Panel C: Age		
<25	0.0429	-0.0043
25-35	0.0304	-0.0467
35-45	0.1138	-0.0271
45-55	-0.0885	-0.0038
55-65	0.0107	0.0173
>65	0.0835	0.0124
Panel D: Others		
Size of city	0.1963	-0.0149
M2	0.0322	0.0306
Rooms	-0.0348	0.0091
Adults	0.0224	-0.0070
Minors	0.0756	-0.0126

Table C7: NORMALIZED DIFFERENCES IN 2014 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Matched	(3) Weighted
Panel A: Basic Covariates			
Education	-0.1463	-0.0130	0.0300
Gender	0.1012	0.0302	-0.2638
Age	0.1387	0.0616	-0.2154
Spanish	0.1174	-0.0598	0.0141
Panel B: Marital status			
Single	-0.0073	0.0631	0.1502
Married	-0.0538	-0.1377	-0.0981
Widow	-0.0494	0	-0.0927
Divorced	0.1235	-	-0.0279
Panel C: Labor relationship			
Paid worker	-0.0016	-0.3025	-0.2994
Owner	0.3569	0.3569	0.3566
Cooperatives	0.0569	0.0569	0.0568
Others	-0.3085	-	-0.013
Panel D: Occupation			
Management	0.0154	0.01389	-0.0176
Science / techs	-0.2074	-0.0307	-0.0099
Security & Health	-0.0909	0.0891	0.0131
Agriculture, Blue Collar	0.1891	0.0521	-0.1444
Services (low skills)	0.0388	-	0.13848

Table C8: NORMALIZED DIFFERENCES IN 2014 (HOUSEHOLD COVARIATES)

	(1) Raw	(2) Weighted
Panel A: Labor Market Status		
Employed	-0.9748	-0.0102
Unemployed	0.0925	-0.0102
Student	0.0347	-0.0641
Retired	0.0496	0.1497
Other	-0.0198	-0.1227
Panel B: Educational level		
Low	0.0038	-0.0398
Basic	0.1132	0.0709
Professional	-0.0716	0.0112
High	-0.1965	0.0489
Panel C: Age		
<25	-0.0138	-0.1062
25-35	-0.0752	-0.0874
35-45	0.0678	0.1242
45-55	0.0114	0.0588
55-65	0.0247	-0.0124
>65	0.0212	0.1521
Panel D: Others		
Size of city	0.1398	-0.0639
Adults	-0.0464	0.0668
Minors	0.0489	-0.1387

Table C9: NORMALIZED DIFFERENCES IN 2016 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Matched	(3) Weighted
Panel A: Basic Covariates			
Education	-0.2637	-0.1191	-0.0436
Gender	0.0472	0.0804	0.0013
Age	0.0974	0.0987	0.0447
Spanish	0.1421	-0.0810	-0.0046
Panel B: Marital status			
Single	-0.0150	-0.0466	0.0150
Married	0.0089	-0.0266	-0.0270
Widow	-0.0543	0.0059	0.0090
Divorced	0.0292	-	0.0178
Panel C: Labor relationship			
Paid worker	-0.3129	-0.3129	-0.3127
Owner	0.2911	0.2911	0.2909
Cooperatives	-	-	-
Others	0.1106		0.1100
Panel D: Occupation			
Management	-0.0127	0.0183	0.0184
Science / techs	-0.3038	-0.0312	0.0273
Security & Health	-0.0050	0.0999	0.0349
Agriculture, Blue Collar	0.0256	0.0536	0.0108
Services (low skills)	0.2121	-	-0.0443

Table C5: NORMALIZED DIFFERENCES IN 2016 (INDIVIDUAL COVARIATES)

	(1) Raw	(2) Weighted
Panel A: Labor Market Status		
Employed	-0.1712	-0.0347
Unemployed	0.2554	0.0679
Student	-0.1266	.0410
Retired	0.0699	-0.0516
Other	0.0320	0.0117
Panel B: Educational level		
Low	0.0863	0.0372
Basic	-0.0877	-0.0117
Professional	-0.0665	0.02641
High	-0.0672	-0.0282
Panel C: Age		
<25	-0.0475	0.0574
25-35	-0.0904	-0.0157
35-45	-0.0234	-0.0313
45-55	-0.0323	0.0549
55-65	0.0703	-0.0185
>65	0.0359	-0.0589
Panel D: Others		
Size of city	0.2108	0.0169
M2	0.0719	0.0052
Rooms	0.0417	-0.0168
Adults	-0.0178	0.0083
Minors	-0.0568	0.0242

5

CONCLUSIONS

The first chapter examines the relationship between the employment status of household members and their cultural expenditure using the Spanish household budget survey. It provides an estimate of the effect of becoming unemployed on income and cultural expenditure and the corresponding income elasticity. On average, the first unemployed household member reduces cultural expenditure and income by EUR 24.90 and EUR 495.63, respectively, while the second unemployed household member has a negative impact on expenditure and income of EUR 30.16 and EUR 464.70. Combining these estimates, the income elasticity of cultural demand is around 0.62 for the first unemployed household member and 0.85 for the second. For those households that participate in cultural activities, an additional unemployed member barely affects household cultural participation. However, those households that do participate in cultural activities reduce their cultural expenditure by EUR 54.04 and EUR 122.41 for the first and second unemployed members respectively. On average, income falls EUR 477.99 and EUR 559.00 for the first and second unemployed member. Thus, the impact on income is similar for the unconditional and conditional analysis, whereas there are considerable differences in estimated effects on cultural expenditure.

The second and third chapters focus on health. The former analyzes the effects of labor market shocks on a set of health indicators. I consider three distinct health indicators: self-reported health status, health investment inputs, and objective health measures. Regarding labor market shocks, I focus on a micro-level shock, such as job displacement, and a macro level shock, such as a period of economic deterioration. Results suggest that the self-evaluation of health status is negatively affected by unemployment. Moreover, those who suffered job displacement were already reporting worse health even before the transition to unemployment. The effect of unemployment on self-reported health appears to be even larger after the Great Recession, although the estimations are statistically insignificant. Moreover, the recession seems to negatively affect the self-evaluated health of both employed and unemployed individuals.

However, results from health investments and health indicators might suggest an increase in the differences of likelihood of emergency room visits and blood pressure problems, while the rest of the outcomes are unaffected or even show a reduction in differences. Therefore, I do not find suggestive results driving a larger unemployment effect on self-reported health from 2009 onwards. The evidence is also mixed when I analyze the effect of the recession on health indicators for both employed and unemployed together. The results indicate that self-evaluated health worsens after the economic downturn, regardless of their employment status. The event study results for health indicators suggest a larger probability of having to see a specialist, along with reporting blood pressure and cholesterol problems. On the other hand, I also find evidence in favor of a decrease in the probability of smoking, likely driven by the pre-recession trend. In addition, the results also point to the same positive effect on the probability of doctor visits and medicament intake.

The third chapter extends the analysis regarding health and unemployment, focusing on mental health. More precisely, it analyzes the differences in mental health between unemployed and employed individuals and the consequences of an economic downturn on those differences. An unemployment spell is a shock that removes affected individuals from their reference points. First, this chapter shows evidence of the mental health differences between employed and unemployed individuals. Then, it analyzes how these differences change after the Great Recession. Results indicate that the mental health gap between employed and unemployed individuals increased by around 0.10 standard deviations, accounting for 38 percent of the cross-sectional differences between both groups. Further analysis shows that, even when mental health is affected, there is no response in medical behavior. In contrast, unemployment may reduce the probability of being prescribed or taking medicines for mental illness and the odds of visiting a specialist for mental illness. A potential explanation for these results is that affected individuals may refuse to visit a doctor as a consequence of the existing stigma surrounding mental health.

Then I turn to analyze the mechanisms behind worsening mental health. Theoretically, deviations from the social norm might affect life evaluation. Thus, I present two hypotheses. The first hypothesis is that, individuals might care not only about their own labor market situation, but also about their social surrounding. Should this hypothesis hold, large unemployment rates might mitigate the unemployment effect on mental health. Under this scenario, unemployment might be seen as a general problem, so the deviation from the social norm is minor, reducing feelings of guilt. The second hypothesis is that individuals are more concerned with their prospective labor market situation and care less about their social norm position. The evidence obtained suggest that it is the deviation from the social norm that may matter more than their perspectives.

Regarding directions for future research, first, the focus of the first chapter can be extended to a different set of goods to analyze how unemployment affects household consumption composition. Second, a deeper insight into health outcomes exploiting the timing and intensity of the Great Recession by region and occupation. The unequal distribution of the 2007-2009 macroeconomic shock across Spanish regions and occupations might be exploited to deepen analysis of the relationship between labor markets and health .

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