Three Essays on Public Policy Evaluation

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PhD Thesis

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Abstract

This thesis evaluates three key public policies relevant in contemporary contexts: the Minimum Income Scheme of the Basque Country, the significant increase in Spain's minimum wage in 2019, and an innovative Active Labor Market Policy named Training with Hiring Commitment. Chapter 1 assesses the impact of the Basque Country's Minimum Income Scheme on poverty reduction, utilizing a comprehensive set of poverty indicators and the Survey of Poverty and Social Inequalities (EPDS) for analysis. Chapter 2 examines the effects of the 2019 minimum wage hike in Spain on employment probabilities and work intensity, employing panel data from the Continuous Sample of Working Lives (CSWL) and comparing employment transitions between different wage groups. Chapter 3 evaluates the impact of the Training with Hiring Commitment program in the Basque Country, which combines job training with direct employment opportunities. Administrative records and matching techniques are used to estimate the program's effect on employment outcomes. Through rigorous analyses of these policies, this thesis aims to showcase the depth of understanding and the array of methodologies available for evaluating policy impacts, contributing to the broader academic literature in the field of public policy evaluation.

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Introduction

Public policies are the tools used by policy makers to address challenges, perceived problematics or unfair situations that require public intervention. In the contemporary economic landscape, notable challenges encompass issues such as income inequality, difficulties in accessing employment and deteriorating working conditions. Consequently, policies aimed at ensuring a minimum income level or promoting job access have taken center stage in public debates and translated into public institutions' agendas and frameworks for action.

Although the goal of public policies is usually well-defined, the effects of their implementation are not always obvious. Therefore, for the introduction of such measures, it is common to raise some questions. Has the policy objective been achieved? Is the impact equal for all affected individuals? Are there any unexpected or undesirable indirect effects resulting from its implementation? Answering these questions requires a combination of advanced and rigorous methodological tools with rich microdata in terms of size and number of variables. These are key ingredients in evaluating the impact of public policies, a process that has long been established in academic circles and is gradually spreading to other sectors of society. It is crucial for individuals tasked with developing and executing public policies to recognize the significance of assessing them. By adopting this approach, policymakers are able to make informed choices based on measurable outcomes, which eventually leads to better policy design and more effective policy implementation.

The work presented in this thesis focuses specifically on evaluating three public policies that, due to their typology, are particularly relevant today. The first policy analyzed is the Minimum Income Scheme of the Basque Country. The second policy under study is the minimum wage in Spain, specifically, the increase that took place in 2019 and has been the most significant to date (it represented a 22% increase compared to 2018). Finally, the third policy evaluated is an Active Labor Market Policy that combines training and hiring of unemployed individuals and is subject to an unmet hiring need by companies. Beyond understanding the impact of each policy, the ultimate goal is to demonstrate the extensive knowledge derived from this type of analysis as well as showcasing a wide range of tools and approaches available for conducting such evaluations.

Chapter 1 shows the importance of using several poverty indicators when assessing the impact of a minimum income scheme on poverty reduction, using the minimum income that operates in the Basque Country as a case study. To conduct the analysis, the Survey of Poverty and Social Inequalities (EPDS for its acronyms in Spanish) is used. This survey is specifically designed to analyze poverty in the Basque Country and provides information on monthly household income, including the amount of minimum income received by beneficiary households. To measure poverty, the analysis combines several indices and equivalence scales frequently used in the literature. These methodological aspects, combined with the dynamic nature of the analysis, allow for a thorough and robust research of the policy impact on poverty reduction.

Chapter 2 examines the effects of the 2019 minimum wage increase in Spain on individual probabilities of losing employment and on reducing work intensity. To estimate these effects, the analysis is based on panel data from the Continuous Sample of Working Lives (CSWL or MCVL for its acronym in Spanish). This administrative register records high-frequency data about wages and employment status of a random sample of workers from the Spanish Social Security. The empirical strategy relies on comparing employment transitions between a group of workers who earned less than the newly-established minimum wage (treatment group) and a group of workers who earned more than the minimum wage threshold and that should therefore be unaffected by the reform (control group). Compared to other analyses of minimum wage increases in Spain, the sample of workers used in this evaluation includes two groups that typically receive low wages: part-time workers and those who do not work every day of the month. In addition, the empirical approach compares employment transitions on a monthly-basis, therefore investigating the dynamic nature of employment adjustment and the possible differential effects of MW in the short- and the medium-term.

Chapter 3 contributes to the academic literature by evaluating the impact of an Active Labor Market Policy established in the Basque Country called *Training with Hiring Commitment*. This program aims to address the skills gap in the labor market by providing job seekers with training courses that align with companies' skill requirements. To qualify for participation, companies must demonstrate a shortage of these profiles in the labor market and commit to hiring a minimum number of individuals. This innovative policy combines training programs for unemployed individuals with direct access to employment opportunities within participating organizations. To conduct the analysis, a set of databases compiled from administrative records are utilized. By combining records of program participants and training activities with information from job seekers from the Basque Public Employment Service (PES) and work histories from the Social Security Administration, it is possible to develop a methodological approach that employs matching techniques and implements Difference-in-Differences estimators. To estimate the effect of the program on employment, the analysis follows treated individuals during the twelve months immediately after the program ends, defining two outcome variables: probability of being employed and number of days worked in a given month.

Finally, chapter 4 summarizes the main results obtained in each chapter and concludes with a reflection on the evaluation of public policies that summarizes the knowledge acquired during the completion of this thesis. The document concludes with three appendices whit additional information, tables and figures, preceded by a full list of references.

Chapter 1

The key role of multiple indicator analysis in anti-poverty policy evaluation: evidence from the Basque Country

1. Introduction

The fight against poverty and the right to an income sufficient for a reasonable standard of living are at the heart of European countries' redistributive policy systems. Even if these types of schemes were initially recommended by the European Council back in 1992,[1] it has been more recently when most EU member states have decided to develop some form of Minimum Income Schemes (MIS). In most of these countries, MIS play the role of last-resort income support programs aimed at protecting households from poverty and have become increasingly important in the context of economic shocks (Coady et al., 2021). In addition to income support provided to those households in need, MIS, when properly designed, play other important roles in societies, as they can contribute to support sustainable and inclusive economic recovery after an economic meltdown, have a stabilizing effect on the demand of goods and services, enhance upward social convergence and promote labor market integration for those individuals who can work (European Commission, 2022).

Despite the growing popularity of tools and policies aimed at guaranteeing a minimum level of income for all individuals (Gorjón, 2019), there is considerable debate about this type of public transfer, with some critics claiming that it is ineffective in combating poverty and/or discourages access to employment. Nonetheless, evidence suggests that, in the aftermath of the Great Recession, public policies aimed at assisting the poor (including MIS) have grown in importance in European countries (Mussida & Sciulli, 2022). Given this reality, it is critical to assess the extent to which this type of policy contributes to poverty reduction.

The contribution of this paper the academic literature evaluating the effect of MIS on poverty reduction is twofold. First, the analysis demonstrates the importance of conducting a multidimensional study when evaluating a public transfer aimed at reducing poverty. Generally, the efficiency of the public transfers is measured by their ability to reduce poverty. However, as it is proved in this paper, such reduction broadly depends on the poverty indicator chosen and even on the equivalence scale. For this reason, the usage of different indices and equivalence scales provides insight into the sensitivity of the poverty measurement tools in analyzing the impact of public transfers

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[1] See Council Recommendation 92/441/EEC of 24 June 1992: https://publications.europa.eu/en/publication-detail/-/publication/9953c2cf-a4f8-4d31-aeed-6bf88a5407f3/language-en

-MIS in this case- on poverty reduction. Conducting such an exercise provides valuable insights into the assessment methodologies that should be employed when evaluating poverty alleviation transfers. Second, the analysis undertaken in this study incorporates a temporal perspective, covering different stages within the economic cycle. This inclusion of a time dimension is of particular relevance, because it provides empirical evidence on the potential of MIS interventions in fostering socioeconomic resilience and mitigating the negative effects of economic downturns by taking into account the dynamic nature of poverty and its relationship with economic fluctuations.

The policy chosen as a case study for this analysis is the Renta de Garantía de Ingresos (RGI), which is the MIS operating in the Basque Country. Two factors explain the decision to choose this regional MIS. First, despite being the first regional MIS established in Spain (Sanzo, 2020), evidence on the impact of the RGI on poverty reduction is very scarce and exhibits limitations in terms of its static nature, as it analyzes the effects of the RGI on poverty reduction exclusively for the year 2016 (see Gorjón & Villar, 2019). Besides, in the context of the development of a new national minimum income policy, the evaluation of the earliest regional minimum income in Spain is of particular interest. Second, according to Frazer and Marlier (2016), the Basque Country is the Spanish region that includes the most significant share of population (8%) under its MIS program. Hence, this analysis also makes a valuable contribution in terms of its novelty and relevance.

To conduct the analysis, the Survey of Poverty and Social Inequalities (EPDS for its acronyms in Spanish) is used. This survey is specifically designed to analyze poverty in the Basque Country and provides information on monthly household income, including the amount of MIS received by beneficiary households. Thus, it is possible to design a microsimulation exercise that allows to assess the effectiveness of this policy on poverty reduction. The study covers the period from 2008 to 2020 to determine whether the impact of this policy varies with the economic cycle. Given that these types of policies seek to guarantee a minimum level of income, the inclusion of recessionary periods such as the Great Recession or the more recent Covid-19 pandemic is of great importance to assess the effectiveness of a MIS, as it is during these times that households may suffer the most income loss due to increased unemployment.

To measure poverty, this paper employs the family of FGT indices proposed by Foster et al. (1984). These indicators, in addition to satisfying the decomposability property, have a simple structure that encourages their widespread use in the literature (e.g., Coulter et al., 1992; Lanjouw & Ravallion, 1995; Collier & Dollar, 2002; Alkire & Santos, 2014; Ayala et al., 2020) and facilitates communication with policymakers (Foster et al., 2010). Furthermore, the use of the FGT indices allows for an analysis of poverty that considers not only the incidence of poverty (i.e., headcount ratio) but also its intensity and income inequality among the poor (i.e., severity of poverty (Villar, 2017)). Considering the results obtained by Ayala et al. (2020), who find a 70% reduction of the gap between the income of the poor households and the poverty line by jointly assessing the impact of the regional minimum income benefits in Spain, this is a key consideration when evaluating a MIS, since poverty reduction seems to be greater when intensity rather than incidence is considered. Another reason for expanding the analysis beyond the headcount ratio is that it is highly sensitive to the equivalence scale used, particularly when one household type is set as reference, as Betti et al. (2017) show after conducting several sensitivity analyses using Turkish data. Equivalence scales allow to compare the incomes of households of varying size and composition, and while there is no agreement on which equivalence scale to use, the results of poverty analyses are sensitive to the equivalence scale chosen (Coulter et al., 1992). In light of this, the main analysis employs two equivalence scales: the OECD-modified and the square root. While there is a wide range of equivalence scales that can be found in the literature, these ones stand out for their ease of use and are thus frequently applied in research (Dudel et al., 2021). These methodological aspects, combined with the dynamic nature of the analysis, allow for a thorough and robust research of the RGI's impact on poverty reduction.

The results of the analysis show that the RGI plays an important role in reducing poverty in all its dimensions, especially intensity and severity. First, although the reduction in the incidence of poverty is remarkable (around 40%), the findings show a greater impact of the Basque MIS in reducing the intensity (severity) of poverty by 60% to 70% (74% to 80%). Second, the impact of the RGI on poverty reduction increases during downturn periods, softening the increase in poverty during the economic recession. The analysis also suggests that the choice of equivalence scale influences poverty measures and the effectiveness of the RGI in poverty reduction, especially when using the headcount ratio. These differences between equivalence scales are significantly reduced when analyzing other dimensions of poverty that consider the distance to the poverty line (i.e., intensity and severity). To increase the reliability of these results, this part of the analysis is replicated by using the OECD (non-modified) equivalence scale, finding significant similarities with the two aforementioned scales regarding poverty reduction, specifically in terms of intensity and severity. Third and last, despite the relevant role of the RGI on reducing poverty, there has been an increase in poverty's incidence and intensity which was not entirely offset by economic recovery. This conclusion holds regardless of the equivalence scale, the poverty dimension or the type of poverty line (relative or anchored) employed to measure poverty over time. The lessons learned from this research are particularly relevant when designing and evaluating a minimum income policy, as is currently being done in Spain and other nearby countries. Considering the expanding public discussion on policy assessment and MIS-type policies, this study encourages policymakers to explore poverty dimensions that assess the distance to the poverty line rather than seeking to optimize the effect on more prevalent but less reliable indicators like the headcount ratio.

The rest of the paper is organized as follows. Section 2 reviews the background literature. Section 3 explains the MIS in the Basque Country. Section 4 describes the data and the methodology applied and Section 5 presents the results of the analysis. Finally, Section 6 summarizes and concludes.

2. Background literature

When attempting to quantify poverty or inequality in a society, there is widespread agreement on the need of using an equivalence scale to compare household incomes of different sizes. Nonetheless, and considering the wide variety of equivalence scales available, the choice of which equivalence scale to use is important, as different equivalence scales can lead to very different conclusions about poverty's reach and composition (Atkinson, 1992). This is because, as Blaylock (1991) shows in his analysis of income and food spending distributions, a household may be either poor or not poor depending on the equivalence scale chosen. Similarly, Coulter et al. (1992) point out that, while there is no consensus about what equivalence scale is appropriate, results are sensitive to scale choice. Consequently, several authors and studies have assessed the sensitivity of poverty indicators to different equivalency scales. Using household income microdata from the Luxembourg Income Study (LIS) database, Buhman et al. (1988) find that the choice of equivalence scale can affect measured poverty (both in absolute and relative levels) for different groups within countries or rankings of countries. Specifically, they find out that poverty rate varies greatly for older individuals, the changes being greater for single persons than for married couples. Similarly, Creedy and Sleeman (2005) examine the sensitivity of several poverty and inequality measures using data from New Zealand, concluding that considerable caution needs to be taken when choosing equivalence scales for adults, as poverty measurement varies greatly using each of the 29 scales applied in their study. Focusing on Euro Zone countries, Bishop et al. (2014) find that using subjective equivalence scales instead of the modified OECD scale reduces poverty rates and that adding the first child is, in general, more costly than adding a third adult. In a recent study with Turkish data, Betti et al. (2017) try to reduce the impact of equivalence scales on poverty measures by choosing the most appropriate household type as reference, showing that the headcount ratio is very sensitive when one adult household is set as reference. Finally, Abanokova et al. (2020) estimate equivalence scales using data from Russia, finding that poverty rates change more for different adult scale parameters than for children.

Regarding the evidence of MIS in poverty reduction, Gouveia and Rodrigues (1999) assess the impact of the Portuguese Guaranteed Minimum Income, concluding that it has a small but positive impact on reducing both poverty and inequality. In Italy, Brunori et al. (2009) analyze a local minimum income program implemented in Mola di Bari, a small town in the South of the country, finding that the transfer improves the economic conditions of benefiting households despite the low coverage rate. In the case of the Basque Country, Gorjón and Villar (2019) show that the MIS operating in this region reduces substantially all dimensions of poverty, although they find that there is room for increasing the coverage and that there exists an asymmetric treatment of households depending on their size. Ayala et al. (2020) use a mixture of administrative and survey data to analyze the regional minimum income benefits (alongside with other contributory and non-contributory benefits) in Spain as a whole, concluding that the contribution of these systems to poverty reduction is very modest and significantly lower than other benefits (i.e., contributory retirement pensions) and that regional MIS are the main form of social protection for immigrants from outside the EU and, to a lesser degree, households with children. Moreover, the authors also point out that the largest contribution to poverty reduction occurs when poverty intensity is analyzed, reducing more than a 70% the poverty gap with respect to the hypothetical scenario where households do not receive any transfer.

There are also studies that analyze the relationship between minimum incomes and the labor market, with mixed conclusions. While Chemin and Wasmer (2012) and Clavet et al. (2013) find a negative impact on labor market participation and employment in France and Canada, respectively, De la Rica and Gorjón (2019) conclude that MIS in the Basque Country does not delay entry into employment.

The analysis of MIS effectiveness generally relies heavily on the headcount ratio, which is particularly sensitive to the selection of the equivalence scale, making this a highly relevant issue. The contribution of this study aims to demonstrate that an analysis of the effectiveness of poverty reduction by a MIS should consider other dimensions beyond the poverty rate in order to minimize the effect of choosing a specific equivalence scale on poverty measures and to more accurately capture the effects on its reduction.

3. The Minimum Income Scheme in the Basque Country

The MIS that operates in the Basque Country is named as Renta de Garantía de Ingresos (RGI). The RGI aims to reduce poverty in the Basque Country without focusing on any particular group, which implies that it is a "simple and comprehensive" scheme (see Frazer and Marlier, 2009). The yearly budget allocated to this policy is between €450 and €500 million, which accounts for 4.5% of public expenditure and almost 0.7% of the GDP in the Basque Country (De la Rica & Gorjón, 2019).

Even though plans to implement a minimum income in the Basque Country began in the late 1980s the RGI in its current form was not regulated until 2008 (Act 18/2008).[2] In this regulation, several requirements were set, of which the most relevant ones are highlighted, taking into account the aim of the study. The first one is that, considering that the RGI is given to household rather than to individuals, in order to receive some amount of MIS, the monthly income of the household must be low enough, such that it fails to reach a minimum living standard. This threshold is set by the Basque Government, and it is different for each type of household, as shown in Table 1. The second requirement is to have continuously resided and been registered in the Basque Country for at least the previous three years. In addition, the RGI is strongly linked to

[2] At the time of writing this paper, an open reform process of the Basque Country's MIS is in the phase of parliamentary debate.

the activation of those individuals who benefit from the assistance. Thus, in general, both the person receiving the benefit and the other members of the household who are of working age must register as job seekers and, in the event of a suitable job offer, accept it. Finally, certain conditions also apply to the property situation of the beneficiaries, such as the impossibility of owning a property other than the usual residence.

Table 1. Maximum amount of MIS that can be received in 2020, by household type.

Type	Household Members	Maximum Amount of MIS (€/month)
1	1 adult	693.73
2	2 adults	890.81
3	3 or more, at least 2 adults	994,.89
4	Single-parent (1 child)	941.26
5	Single-parent (2 or more children)	1,035.86
6	1 retired	803.61
7	2 adults, at least 1 retired	994.94
8	3 or more, at least 1 retired	1,074.53

Source: Basque Government

Finally, one of the most important aspects of the RGI is that it makes receiving the benefit compatible with working, which is a distinguishing factor when compared to other minimum incomes and is in line with the proposal from the European Commission to adapt these policies to promote active inclusion (European Commission, 2022). The legislation of the Basque Country names this instrument as *stimulus to employment*, which works as a wage complement, as it discounts a percentage of the wage when taking into account the income of the household. In practice, this implies that the threshold that appears in Table 1 could increase by the amount of the wage excluded. This instrument is a critical component for labor activation because it allows beneficiaries to avoid the well-known *poverty trap*, which describes the fact that MIS beneficiaries' total income (wage plus transfers) may not increase, discouraging them from seeking employment.

4. Data and methodology

4.1 Database

To carry out this analysis, the Survey of Poverty and Social Inequalities (EPDS by its acronyms in Spanish) for the Basque Country is used. This survey is specifically designed to analyze poverty in the Basque Country and, therefore, it provides all the information required in order to evaluate the evolution of poverty over time. The waves corresponding to years 2008, 2012, 2014, 2016, 2018 and 2020 are used. Regarding the sample of the survey, each wave usually includes at least 10,000 individuals and 4,000 households, representing the entire Basque population that lies between 2.1 and 2.2 million. Table 2 shows the number of observations (individuals and households) for each of the waves.

Table 2. Sample observations for each wave

	2008	2012	2014	2016	2018	2020
Individuals	11,110	10,377	10,599	10,316	10,516	10,812
Households	4,502	4,133	4,350	4,327	4,533	4,632

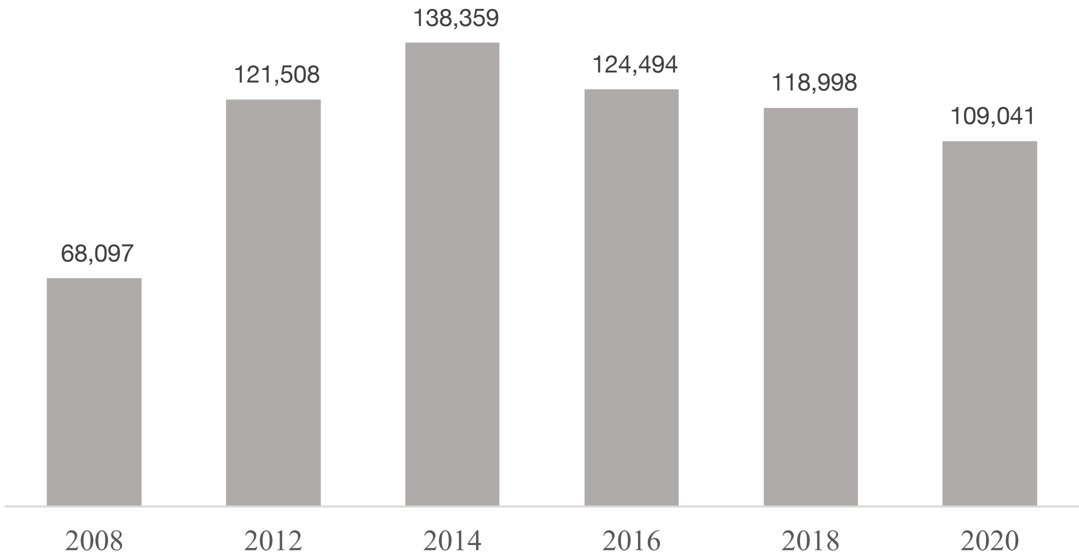
Source: EPDS

The EPDS offers a wide range of information not only about the households surveyed but also about each of the members of the household. This information contains personal details, such as gender, age or census status, but also about the education level or the labor status of each individual. However, given the focus of this study, the most interesting information is the one related to income. In the survey, there is ample information about any type of income at both individual and household level - monthly information about wages, retirement pensions, benefits and other transfers, among others.[3] The EPDS also includes one variable that states whether a household is a MIS recipient or not.[4] According to the survey, following the economic downturn, there was a significant increase in the number of people living in households that received the RGI (see Figure 1). While the total number of people benefiting from this transfer in 2008 was 68,097, the figure increased to 121,508 in 2012. In 2014, when the unemployment rate

[3] Some of the income variables (private money transfers and most of public subsidies or aids) include the amount for the whole year. Since the MIS is provided monthly, it is important to transform these variables into a monthly amount in order to obtain the correct amount of total monthly income for each household.
[4] This variable includes the Social Housing Benefit, which is a complement that MIS recipients may receive if they have to pay a housing rent.

in the Basque Country was above 16%,[5] there were 138,359 people in households receiving the benefit, which was slightly more than double the number in 2008. From 2016 onwards, and in tandem with the economic recovery, a downward trend in the number of beneficiaries began, though the most recent available data (109,041 people in 2020) shows a significant difference from 2008.

Figure 1. Number of individuals living in households that receive RGI



Note: Figures refer to the number of beneficiaries at the time of the interview
Source: Own calculations using EPDS

While the EPDS database is comprehensive and extensive, it is important to acknowledge certain limitations associated with it, particularly in terms of its inability to provide all the necessary information to verify the eligibility of a household for receiving the transfer. For example, one notable information gap is the absence of data pertaining to the ownership of a potential second home, which is a crucial factor that leads to immediate disqualification from receiving the benefits of the RGI. Consequently, it is necessary to assume that if a household falls below the poverty line outlined in Table 1 but does not receive the MIS, it is either due to the household not applying for the MIS or its inability to meet one or more of the eligibility requirements.

[5] Source: Spanish Labor Force Survey (annual data). Data available here: https://www.ine.es/jaxiT3/Tabla.htm?t=4966&L=0

Secondly, own income information is self-reported. Moreover, it is possible for one individual to report the income information of the other members of the household. This fact can have two main potential caveats. The first one, as explained by Gorjón and Villar (2019), appears when the information reported does not coincide exactly with official numbers. In this case, it is usual that the amount of MIS received by households is slightly misreported. This implies that there are some differences with the theoretical amount that, according to the Basque Government criteria, should correspond to that specific household given its income. The second potential problem is that the EPDS includes variables that are not taken into account when assigning the MIS. For example, individuals may report private money transfers from relatives or friends, but this income will not appear in official statistics when calculating total disposable income. Overall, these caveats could imply that some households' reported income is less than the poverty threshold when their current income is more, or vice versa.

4.2. Methodology

In order to analyze the impact of the Basque MIS on the reduction of poverty, two scenarios are compared using a microsimulation, following the approach applied by Gorjón and Villar (2019). The first one, defined as post-MIS scenario, considers the actual disposable income of each household, which includes the amount of MIS received. The second one is the hypothetical scenario (pre-MIS), in which the amount of MIS received is not taken into account when calculating the monthly income of each household. This way, it is possible to calculate to what extent the MIS system is fulfilling the objective of helping people living in a poverty situation to escape from it.

While the construction of this counterfactual scenario proves useful to carry out the analysis, Gorjón and Villar (2019) point out that there are additional effects that are not considered, other than the direct impact on poverty. This fact implies that no account is taken of the financing of the benefit (the extent to which the redistributed effects of benefits may be offset by taxes and other contributions) or the indirect effect of taxes and benefits on the size or the economic circumstances of the population that receive the benefits (Beckerman, 1979). Concerns are frequently raised regarding whether MIS systems have disincentives on the labor market, thereby influencing the behavior of individuals (Rodrigues, 2001). However, there is enough evidence to assume that such a change does not occur. First, the European Commission states in the 2022 Minimum

Income Report that monetary disincentives on labor market participation of MIS beneficiaries are not significant (see European Commission and Social Protection Committee, 2022). Second, and most importantly given the direct link to the policy evaluated in this paper, De la Rica and Gorjón (2019) found that the RGI did not delay entry into employment for recipients of the transfer and that active labor market policies specifically designed for this group did have a significant positive impact on finding a new job.

4.2.1 Equivalence scale

Household consumption requirements grow in proportion to the number of people in the household. However, because household consumption has economies of scale, this growth does not occur linearly with each additional individual. As a result of this, in order to properly compare households of different size, it is frequent to use some instruments known as equivalence scales, which usually take into account the size of the household and the age of its members (OECD, 2013a).

Although the use of equivalence scales is acknowledged to be a necessary mechanism to adjust the size of households, as discussed in the literature review section, there is not a consensus regarding the choice of a specific equivalence scale. In this case, the analysis applies two equivalence scales that are widely used in the literature (OECD, 2013a): the OECD-modified equivalence scale and the square root equivalence scale. The OECD-modified equivalence scale, which was introduced by Hagenaars et al. (1994), is the method adopted by Eurostat to measure poverty.[6] This equivalence scale assigns a value of 1 to the first member of the household. For the rest of the members, it assigns a value of 0.5 to each additional adult or child 14 or older, and 0.3 to each child under 14. The square root equivalence scale is calculated by taking the square root of the total number of individuals living in a household, no matter the age. According to Atkinson et al. (1995), this scale provides a good contrast between per capita income and the case of not adjusting households' income. Although there is a wide range of equivalence scales, as Dudel et al. (2021) state, these two cases stand

[6] Hagenaars et al. (1994) introduced the modified OECD scale due to previous research findings that revealed a high family size elasticity associated with the conventional OECD scale. While the OECD-modified scale is more commonly used than the original OECD scale in current poverty analysis, the addition of the latter scale helps to enhance and complement the results presented in Section 5. A detailed description of the OECD scale and the corresponding results can be found in Appendix I. The overall results align with those obtained using the OECD-modified and square root equivalence scales.

out for their ease of use and are thus frequently used in applied research. Furthermore, when comparing several methods for the estimation of equivalence scales using data from Germany, Dudel et al. (2021) conclude that equivalence scales based on more plausible estimates are similar to the modified OECD for households with fewer than two children, and closer to the square root scale for larger households.

In order to obtain the equivalent income for each household, the total disposable income of each household is divided by its corresponding equivalence scale. Then, the same equivalent income is assigned to each household member. This means that it is not possible that some household members are poor while others are not. Consequently, the entire household will be either above or below the poverty threshold. Table 3 illustrates with a simple example the application of the OECD-modified and the square root scales.

Table 3. OECD-modified vs square root equivalence scale application

Household composition	Total Disposable Income	OECD-modified equivalence scale	Equivalent income (OECD-mod)	Square root equivalence scale	Equivalent Income (square root)
1 adult	1,400.00	1	1,400.00	1	1,400.00
2 adults	2,100.00	1.5	1,400.00	1.41	1,489.36
2 adults, 1 child	2,100.00	1.8	1,166.67	1.73	1,213.87

Note: The table provides an example of the application of the equivalence scales used in the main analysis. Depending on household composition (column (1)), columns (3) and (5) display the correspondent values for the OECD-modified and the square root equivalence scales, respectively. Finally, to obtain the equivalent income of the household (columns (4) and (6)), the total disposable income (column (2)) is divided by columns (3) and (5).

4.2.2 Poverty line

Defining poverty can be a complex task. According to Villar (2017), poverty can be defined as a phenomenon that refers to the difficulty of having access to a series of good and services that ensure living with dignity and being able to have a satisfactory personal and social life. However, there is not a single and objective way of measuring poverty. In other words, there is not such a thing as a scientific poverty threshold.

Nonetheless, in order to analyze poverty, it is mandatory to set a criterion to differentiate poor individuals from those who are not. This threshold is usually named as the poverty line. This analysis uses a poverty line that is frequently found in the literature, which takes the median equivalent income in the society as reference. Specifically, the poverty line is set at 40% of median equivalent income in the Basque Country and it can be defined as “extreme poverty”. Another commonly used poverty line is 60% of the equivalent median income, which denotes the risk of poverty in a society. However, this threshold is not included in the analysis because it is too lofty a goal for last-resort benefits like MIS.

Indices that use the income of the entire population as reference rather than a fixed income threshold are known as relative poverty indices. While broadly used in the literature, relative poverty lines present some limitations in the analysis of poverty over space and time. Given that this kind of relative thresholds are based on current median or middle income, relative poverty will decrease if the incomes of all households fall, but by less at the bottom than at the middle of the distribution (OECD, 2013b). As this issue may affect the measurement of poverty, a thorough study of poverty requires complementing the picture provided by relative income poverty with different more "absolute" poverty indices linked to past living standards. For this reason, in addition to the mentioned poverty line, the current analysis is expanded by setting the relative poverty line from 2008 as an absolute threshold (adjusting it for price inflation) for the other years. The results of this approach can be found in Appendix A.

Finally, it should be noted that the poverty line is directly related to the equivalence scale used, as it affects the median equivalent income. This relationship has a direct impact on poverty’s incidence because, as Blaylock (1991) points out, one household might be poor in a distribution constructed using one specific equivalence scale but might not when another one is used.

4.2.3 Poverty dimensions and impact assessment of MIS policies

To measure the different dimensions of poverty, the FGT family of indices are used, which is one of the better-known family of decomposable poverty indices (Villar, 2017). In addition, these indicators have a simple structure that encourages their widespread use in the literature (e.g., Coulter et al., 1992; Lanjouw & Ravallion, 1995; Collier & Dollar, 2002; Alkire & Santos, 2014; Ayala et al., 2020). This family of indices, which was developed by Foster, Greer and Thorbecke (1984), is defined by the following function:

$$P_{FGT}^{\alpha}(y,z) = \frac{1}{n}\sum_{i=1}^{p}\left(1-\frac{y_i}{z}\right)^{\alpha}$$

In this function, *n* represents the total population and *p* is the number of poor individuals. The poverty line is denoted by z, while *y* represents the level of income. Finally, the parameter α will determine the dimension of poverty to be measured and might be interpreted as the degree of poverty aversion (Villar, 2017).

When $\alpha = 0$, the indicator is the headcount ratio,

$$P_{FGT}^{0}(y,z) = \frac{p}{n}$$

which is one of the most elementary measures of poverty, as it measures the share of poor in a given society (i.e., the proportion of people who fall below the poverty line). This is often defined as poverty incidence, since it only takes into account how many poor are in the society. While the headcount ratio is broadly used to analyze poverty, firstly, it is also known to be very sensitive to the choice of equivalence scale (Betti et al., 2017) because it affects the poverty line and, therefore, at the same income level, a household may be poor if one scale is used but not under another. In addition, this indicator might underestimate the impact of a MIS to reduce poverty, as some individuals may still fall below the poverty line despite receiving the benefit and, therefore, being better off. In other words, the head count ratio only captures the impact of the MIS if the amount given is enough to reach the poverty line and does not take into account the improvement in the welfare of those households which continue being poor even after receiving the MIS. For these two reasons, the headcount ratio is a limited indicator when measuring the consequences caused by MIS in particular and poverty policies in general. In order to avoid these pitfalls, the paper assesses the impact of the RGI in poverty using some other dimensions of the FGT family of indices that do consider the distance to the poverty line.

For $\alpha = 1$, the FGT index is equivalent to the Poverty Gap Ratio (PGR):

$$P_{FGT}^{1}(y,z) = \frac{1}{n}\sum_{i=1}^{p}\left(1 - \frac{y_i}{z}\right)$$

This index provides more information than the previous one as it combines poverty incidence and poverty intensity, which includes another perspective and a better understanding of the extent of poverty. In particular, this indicator captures how a transfer brings a person closer to the poverty line, even if they still fall below it. In terms of measuring the impact of transfers such as the MIS, this method gives significantly more information and, at the same time, directly addresses the limitation of the headcount ratio in measuring the impact of an MIS. This is due to the fact that by using this index to compare the pre-MIS and post-MIS scenarios, the increase in welfare of all poor individuals who benefit from the income guarantee policy can be measured. On the one hand, the FGT1 is able to measure the reduction in the number of poor people who are able to escape poverty as a result of the MIS. On the other hand, taking into account the distance to the poverty line enables the measurement of the improvement in the welfare of those who remain in extreme poverty after receiving the MIS.

However, measuring the poverty intensity ignores changes in income inequality among poor individuals. The analysis is extended to a third dimension, the severity of poverty, which can be measured when $\alpha = 2$.

$$P_{FGT}^{2}(y,z) = \frac{1}{n}\sum_{i=1}^{p}\left(1 - \frac{y_i}{z}\right)^2$$

By definition, MIS seek to bring all individuals living in households with comparable characteristics to a given threshold. In practice, this means that the further a household is from the threshold, the greater the amount of MIS it will receive, and vice versa. In practice, this type of policy establishes a single floor for all beneficiaries, thereby reducing the disparities that existed prior to the implementation of MIS. Therefore, incorporating this dimension is essential for a complete analysis of such an anti-poverty policy. This is an important consideration when evaluating a policy of this type, which aims to bring all households with similar characteristics closer to the same threshold. As a result, it is possible that a MIS does not significantly reduce the incidence of poverty, but it does have a significant impact on poverty intensity and severity. If this is the case, using the headcount ratio alone would be counterproductive, as it could imply that a MIS is ineffective at reducing poverty when it is, in fact, effective.

Thus, with the FGT 0, 1 and 2, it is possible to measure poverty, and the impact of the MIS, in three different dimensions: incidence, intensity and severity (also known as inequality), which are generally known as the three *"I"*s of poverty (Sen, 1976). This is a key consideration when evaluating a policy of this type, as some authors find that poverty reduction is greater when intensity rather than incidence is analyzed (Ayala et al., 2020). Therefore, when it comes to evaluating social transfers to combat poverty, FGT 1 and 2 seem more appropriate than FGT 0, as shown in the results section.

5. Empirical Results

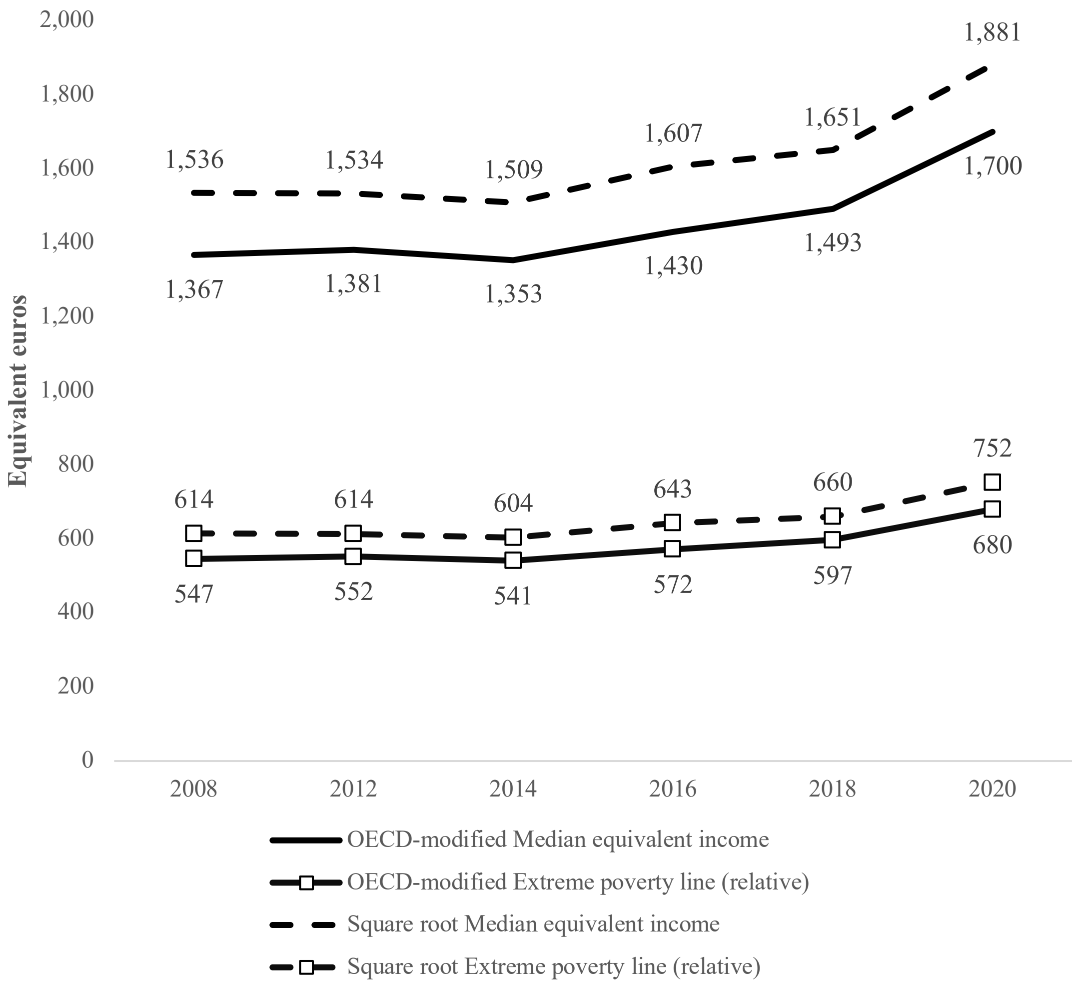
This section outlines the empirical analysis. First, the evolution of the poverty line (using both equivalence scales) in the Basque Country between 2008 and 2020 is depicted. Second, the impact of the MIS on poverty reduction is illustrated by distinguishing between the three poverty dimensions aforementioned: incidence, intensity and severity. The analysis results lead us to the conclusion that the MIS in the Basque Country significantly reduces poverty, particularly during the hardest years after the 2008 economic recession. Furthermore, the results clearly show that when evaluating a MIS-type policy, it is necessary to consider, in addition to the headcount ratio, other dimensions of poverty that consider the distance to the poverty threshold, as they provide a more precise and comprehensive representation of the policy's effectiveness in alleviating poverty.

5.1 Evolution of poverty line in the Basque Country

Figure 2 shows the median equivalent income and both poverty lines for each analyzed year with available data. As can be observed, for every year, the median equivalent income and the extreme poverty line obtained with the square root equivalence scale are higher than the ones obtained using the OECD-modified scale. This result, along with the examples in Table 3 above, has important implications as it shows the existence of households that may or may not be poor depending on the scale used. However, these results also indicate that both poverty lines follow trends that are somewhat similar. In both cases, the median equivalized income declines slightly during the recession, then increases during the recovery and most significantly during the first year of the pandemic.

Thus, regarding poverty line dynamics, it can be argued that the methodology employed for establishing the poverty line holds greater significance than the choice of a specific equivalence scale (see Figure 2 and Figure A1 in Appendix A). Furthermore, when compared to the relative poverty line, the absolute poverty line (i.e. the 2008 relative line reference adjusted for inflation) guarantees a more stable evolution over time (except for the first period if this occurs after an economic shock), though it is also true that by taking this approach, some detail on changes in income distribution in the years after 2008, which are relevant both in terms of poverty and inequality are ignored.

Figure 2. Median equivalent income and extreme poverty line



Source: Own calculations using EPDS

5.2 MIS and reduction of poverty incidence

Table 4 shows the incidence of extreme poverty in the pre-MIS and post-MIS scenarios, using both scales (OECD-modified and square root). By comparing the levels of poverty incidence, the impact that the MIS has on the reduction of the proportion of poor people is revealed. The pre-MIS columns represent the fictitious scenario in the absence of MIS and the post-MIS columns show the poverty levels presented in the previous section, i.e., those that include MIS (real ones).

The results obtained using the OECD-modified scale show that the MIS helps to prevent the incidence of extreme poverty from skyrocketing in periods of economic recession. See, for example, that in 2014, which was the year with the highest incidence of extreme poverty, the difference between the pre-MIS and post-MIS scenario was more than 3.5 p.p. for the Basque Country as a whole. It should also be noted that, although it is true that extreme poverty has become chronic in the Basque Country in recent years, the MIS continues to play an important role which, although it does not prevent from eradicating extreme poverty, does help to alleviate it considerably. In fact, the reduction in the incidence of extreme poverty has remained relatively stable since 2016 (between 35% and 38%).

When analyzing the reduction in the incidence of extreme poverty using the square root scale, a significant reduction in this dimension is also observed between the pre-MIS and post-MIS scenarios. However, as the last column of Table 4 shows, this reduction is smaller than in the previous case. For example, in 2020, the reduction is 21.9%, compared to 36.9% for the OECD-modified scale. A possible explanation for this difference could be the fact that the extreme poverty line of the square root scale is systematically higher than that of the OECD-modified scale, which diminishes the effectiveness of MIS in reducing the incidence of extreme poverty.

Additionally, these results demonstrate the sensibility of the poverty rate to the equivalence scale used, which therefore affects to the measurement of the impact of a transfer policy; while it is true that the trend in the evolution of poverty is similar in both cases, the share of population living in extreme poverty varies significantly depending on the equivalence scale applied, being higher when using the square root scale. Even most importantly, the choice of the equivalence scale also affects the reduction of poverty incidence, which proves that this indicator may not be enough to assess the effectiveness of this kind of policies. For all these reasons, it can be concluded that using the headcount ratio as the only indicator to analyze poverty might be misleading. Nevertheless, the results show that reduction in poverty incidence is quite remarkable no matter the equivalence scale used.

Table 4. Extreme poverty incidence reduction

	OECD-modified			Square root		
Year	**Post-MIS**	**Pre-MIS**	**Reduction of extreme poverty incidence**	**Post-MIS**	**Pre-MIS**	**Reduction of extreme poverty incidence**
2008	3.43%	4.63%	-25.8%	4.15%	5.38%	-22.9%
2012	3.59%	6.08%	-40.9%	4.05%	6.35%	-36.2%
2014	4.93%	8.50%	-42.0%	5.60%	8.95%	-37.4%
2016	4.91%	7.86%	-37.5%	5.34%	8.18%	-34.8%
2018	5.12%	7.95%	-35.7%	5.91%	8.44%	-30.0%
2020	6.09%	8.34%	-36,9%	6.75%	8.65%	-21.9%

Source: Own calculations using EPDS

5.3 MIS and reduction of poverty intensity

Although the MIS helps to significantly reduce the incidence of extreme poverty in the Basque Country, the fact is that there is a non-negligible part of the population of the Basque Country in a situation of extreme poverty even in the non-simulated scenario, as shown in Table 4. However, it is necessary to point out that the impact of the MIS goes beyond the reduction of poverty incidence, since it also helps in a very relevant way the people who remain poor bringing them closer to the poverty threshold. This fact can be seen in Table 5, which shows the reduction in the intensity of extreme poverty when moving from the pre-MIS to the post-MIS scenario. In this case, in line with findings from Ayala et al. (2020) and Gorjón and Villar (2019), the reduction in poverty is much more important than when the headcount ratio is considered, reaching 70% in the worst moments of the economic recession.

Table 5. Extreme poverty intensity reduction

	OECD-modified			Square root		
Year	Post-MIS	Pre-MIS	Reduction in extreme poverty intensity	Post-MIS	Pre-MIS	Reduction in extreme poverty intensity
2008	0.74	1.73	-57.20%	0.73	1.78	-58.99%
2012	0.94	3.24	-70.98%	0.95	3.31	-71.30%
2014	1.41	4.45	-68.31%	1.42	4.56	-68.86%
2016	1.23	3.88	-68.30%	1.28	4.02	-68.16%
2018	1.47	4.14	-64.48%	1.49	4.23	-64.78%
2020	1.71	4.17	-58.90%	1.76	4.24	-58.49%

Source: Own calculations using EPDS

In this case, unlike in the previous subsection showing the evolution of the intensity of extreme poverty, the differences between the two scales used are minimal. This may be due to the fact that the FGT1 index combines the incidence of poverty with the distance to the threshold. Therefore, it could be possible that in the case of the OECD-modified scale there are more people moving out of extreme poverty, while in the case of the square root scale, more people would fall below the poverty line but, on average, remain closer to the poverty line.

5.4 MIS and reduction of poverty severity

The impact of MIS on reducing poverty severity is even greater. Specifically, regardless of the equivalence scale used, this policy reduced the severity of poverty by 75% to 80% depending on the year. This greater reduction can be explained by the MIS's design, which brings all aid recipients with similar characteristics to the same threshold. As a result, income inequality between people below the extreme poverty line decreases significantly. From this result, it can be concluded that the MIS in the Basque Country is a very pro-poor policy, as it greatly benefits those who are located further away from the poverty line.

Table 6. Extreme poverty severity reduction

	OECD-modified			Square root		
Year	Post-MIS	Pre-MIS	Reduction in extreme poverty severity	Post-MIS	Pre-MIS	Reduction in extreme poverty severity
2008	0.29	1.09	-73.4%	0.26	1.08	-75.9%
2012	0.43	2.44	-82.4%	0.42	2.48	-83.1%
2014	0.68	3.28	-79.3%	0.69	3.36	-79.5%
2016	0.59	2.77	-78.7%	0.6	2.88	-79.2%
2018	0.77	3.06	-74.8%	0.76	3.13	-75.7%
2020	0.78	2.93	-73.4%	0.77	2.96	-74.0%

Source: Own calculations using EPDS

Therefore, the results of this analysis make it clear that the MIS not only reduces the number of people in extreme poverty (independently on the equivalence scale), but also significantly improves the situation of those individuals who do not manage to escape from this situation despite being beneficiaries of the aid. In addition, it has been shown that the differences between both scales are almost non-existent when analyzing poverty intensity and severity, in contrast to what occurs when headcount ratio is used, in line with Ayala et al. (2020).

From the results can be concluded that, in order to measure the impact of public transfers on poverty reduction, it is preferable to use indicators such as FGT 1 and 2 that take into account other dimensions besides the number of people living below the poverty. Taking into account the increasing importance of policy evaluation on the public debate, as well as the growing interest on the MIS type policies (Gorjón, 2019), the results obtained in this paper encourage policy designers to consider dimensions of poverty that measure the distance to the poverty line instead of trying to maximize the impact on more common but less realiable indicators such as the headcount ratio.

In addition, results also show that the MIS in the Basque Country is key to mitigate the rise in poverty during economic downturns, although there has been an increase in the incidence and intensity of poverty in the Basque Country that has not been compensated during the recovery. In 2008, the extreme poverty rate in the Basque Country using the OECD-modified scale (square root) was 3.4% (4.2%), while in 2018, in a context of economic growth, it was 5.1% (5.9%). During this period, the intensity of poverty doubled and the severity almost tripled and, more worryingly, the results in Appendix A show that this worsening is even more prominent if the 2008 living standards are set as the poverty line for subsequent years.

6. Conclusions

This paper contributes to the academic literature on evaluating the impact of MIS on poverty reduction in two significant ways. Firstly, it highlights the importance of adopting a multidimensional approach when assessing public transfers aimed at poverty alleviation, providing insights on the evaluation of poverty-fighting interventions. Secondly, it incorporates a temporal perspective, considering various stages of the economic cycle, thus offering empirical evidence on the potential of MIS interventions in promoting socioeconomic resilience and mitigating the adverse effects of economic downturns.

For this purpose, the Basque MIS (RGI), which is the longest running regional MIS in Spain, has been chosen as a case study. Using the EPDS, a survey designed specifically to measure poverty in the Basque Country, a hypothetical scenario in which households do not receive MIS is constructed. The main objective of this analysis is to contrast whether the choice of an equivalence scale or a specific indicator affects the measurement of the impact of poverty-reduction transfers. For this purpose, the FGT indices proposed by Foster et al. (1984), as well as the OECD-modified and square root equivalence scales, are used. The study covers the period from 2008 to 2020 to determine whether the impact of this policy varies with the economic cycle, complementing the analysis conducted by Gorjón and Villar (2019).

Three main conclusions can be drawn from the results of the analysis. First, the MIS helps to reduce poverty in all the considered dimensions (incidence, intensity and severity) in the Basque Country. In terms of the incidence of poverty, and despite the fact that it does not completely eradicate poverty, this public policy helped to reduce it by around 40% during the recession’s hardest times. Although the reduction in the incidence of poverty is remarkable, the findings show that the impact of the Basque

MIS extends beyond this dimension. Specifically, the RGI contributes to reducing the intensity of poverty between 60% and 70% and the severity of poverty between 74% and 80%. These results imply that the benefits of an MIS on the welfare of the low-income population go beyond the incidence dimension, as poor people significantly improve their situation despite remaining below the extreme poverty line. Consequently, this paper demonstrates that in an analysis of a poverty-reduction transfer, it is crucial to consider poverty dimensions that take into account the distance from the poverty line.

Second, the analysis' results prove that the instruments used to measure poverty should be chosen with caution, given that the choice of an equivalence scale is not a trivial matter, especially if the goal is to measure the incidence of poverty. However, the scale's significance diminishes when other dimensions (intensity and severity) of poverty are considered. The results show that the differences in poverty reduction are not as pronounced as the differences in poverty measurement, but they are smaller in any case when FGT 1 or 2 are used instead of FGT 0. Overall, it is important to keep in mind the significance of this instrument's selection not only when conducting a public policy evaluation, but also when designing the policy itself, as it determines which households can benefit from this type of assistance.

Third, despite the existence of the MIS, after the economic recession, there was an increase in the incidence and intensity of poverty in the Basque Country that has not been compensated during the recovery. In 2008, the extreme poverty rate in the Basque Country using the OECD-modified scale (square root) was 3.4% (4.2%), while in 2018, in a context of economic growth, it was 5.1% (5.9%). During this period, the intensity of poverty doubled and the severity almost tripled. The implications of these dynamics mean that, even before the Covid-19 crisis, the number of poor people in the Basque Country has not only increased, but their situation has deteriorated in a concerning way. Given that a Basque MIS reform is in the works as well as the introduction of the national MIS, more research will be required to determine how far this situation can be reversed in the future.

Chapter 2

Employment effects of the minimum wage: Evidence from the Spanish 2019 reform

1. Introduction

In a context of rising inequality and job insecurity, Minimum Wages (MW) emerge as a popular tool to reduce in-work poverty and income inequality in the labor market.[7] Several political and distributive factors explain the growing social and political popularity of MW in the US and many European countries. From a social standpoint, raises in minimum wages potentially increase income levels of workers located in the lower end of the wage distribution (e.g., teens and younger workers with lower education credentials) (Card, 1992; Neumark et al., 2014; Manning, 2021; Cengiz et al., 2021). From a political viewpoint, MW constitute a more attractive distributive policy than other monetary transfers because they achieve income predistribution without short-term fiscal costs (Barceló et al., 2021).

Despite their growing popularity, minimum wages remain a highly contentious policy from a policymaking and research standpoint. Most controversies over MW reforms stem, generally speaking, from its potential negative effects on employment. While opponents argue that MW entail certain risks for low-skilled workers by increasing unemployment or slowing down job creation (Neumark & Wascher, 2010), advocates argue that MW do not negatively affect employment (Card & Krueger, 1995). Overall, several firm adjustment methods determine the net impact of MW reforms. In this sense, firms can respond to raises in the minimum wage by (a) reducing profit margins (Draca et al., 2011), (b) passing on labor costs to consumers (i.e., through price increases) (Harasztosi & Lindner, 2019; Aaronson & French, 2007), and (c) making labor adjustments at the extensive (i.e., workforce reductions) and intensive margins (i.e., reductions in contracted hours) (Manning, 2021, Clemens, 2021). Because local economic conditions (e.g., the degree of monopsony or monopolistic competition, the elasticity between capital and labor…) determine the extent to which firms employ each adjustment path, the disagreement on whether MW deteriorate working conditions through more (less) job destruction (creation) or reductions in working hours largely remains.

[7] In a 2015 report, the OECD states that inequality in the majority of developed nations reached its highest point in the last three decades (OECD, 2015). In contrast, according to the most recent data from the Spanish Labor Force Survey for the third quarter of 2021, 26% of the salaried population in Spain has a temporary contract. In terms of work intensity, the rate of part-time employment (13.5%) is not particularly high, but one out of every two individuals working in this capacity does so voluntarily.

This chapter investigates the employment effects of MW by leveraging a substantial and persistent increase of the Spanish minimum wage. In 2019, Spanish authorities raised the MW by 22% (from €735.90 to €900). The apparent significance of this increase constitutes an interesting case study. First, the 2019 raise was the largest in recent Spanish history, well above the 5% nominal annual growth rate observed between 1981 and 2020. Second, this MW reform had substantial distributional implications by significantly increasing the MW-to-median annual wage income ratio (i.e., the Kaitz index rose from 49% in 2017 to 63% in 2019, according to Barceló et al. (2021)). Motivated by recent evidence of negative MW effects above 60% of average wage (Manning, 2021), I leverage this reform to obtain new evidence of MW effects in an environment where firms had significant incentives to restructure their production processes.

To estimate the employment effects of the 2019 reform, the analysis uses panel data from the Continuous Sample of Working Lives (CSWL or MCVL for its acronym in Spanish). This administrative register records high-frequency data about wages and employment status of a random sample of workers from the Spanish Social Security. Our empirical strategy relies on comparing employment transitions between a group of workers who earned less than the newly-established MW (treatment group) and a group of workers who earned more than the minimum wage threshold and that should therefore be unaffected by the reform (control group). While this approach somewhat resembles previous research in countries like the US (Linneman, 1982) and Germany (Dustmann et al., 2022), this analysis expands upon previous studies focusing on Spain (e.g., Galan & Puente, 2015; Barceló et al., 2021) in several ways. First, it incorporates nearly the entire working population in our sample and thus, do not restrict the analysis to full-time working population (e.g., Barceló et al., 2021). The analytical sample therefore includes two important groups of workers that generally receive lower wages and might be more vulnerable to job loss after the reform: part-time workers and people who do not work every day of the month. Second, this empirical approach compares employment transitions on a monthly-basis, therefore investigating the dynamic nature of employment adjustment and the possible differential effects of MW in the short- and the medium-term. Finally, the research design controls for observable differences between treatment and control groups using a matching strategy. While previous studies (e.g., Galán & Puente, 2015) rely on the simple inclusion of covariates in a generalized difference-in-differences type of regression, this approach is based on a more careful application of the Coarsened Exact Matching (CEM) technique (Iacus et al., 2012). The research design thus allows to attain balance in observable characteristics while simultaneously relaxing the parametric specification used by these regression approaches.

Results show no immediate significant impacts on the probability of losing employment or number of working hours following the MW reform. However, dynamic estimates indicate statistically significant and sizable negative employment effects between four and twelve months after the reform. Altogether, the results suggest that, after one year, most of the negative employment effect happens through loss of employment (69.6%) rather than a reduction in the number of working hours (30.4%). In particular, the baseline analysis suggests that, after twelve months, the 22% raise in minimum wage increased the probability of losing employment (reducing working hours) by 1.92 p.p. (0.84 p.p.) for workers affected by the reform. Taken together, the results imply an employment loss elasticity of -0.09, consistent with findings from Barceló et al. (2021) in Spain and median elasticity estimates from studies in the US (Neumark & Shirley, 2021).

To assess the presence of heterogeneous effects, separate analyses are conducted according to the gender, age and prior work intensity of workers. Several findings emerge from this analysis. First, the findings indicate limited (statistically non-significant) differences between men and women workers in the probability of losing employment. In terms of work intensity, I observe a more immediate adjustment for men and a larger impact for women over the medium term. Second, results suggest significant heterogeneity by workers' age. Interestingly, they also show that, while younger workers are more affected in terms of work intensity, older workers suffer a larger employment loss effect. Finally, there are large differences between full-time and part-time workers, with the former expecting a larger work intensity adjustment.

To validate the stability and reliability of these results, number of checks are performed. In particular, the analysis experiments with adopting a more stringent matching strategy, changing the baseline day of the week used for computing employment transitions, and evaluating possible anticipation effects. Overall, our findings withstand these robustness checks. Additionally, I run a placebo test in a period with no MW increases. This placebo test yields null results, supporting the interpretation that our findings are not primarily driven by violations of the Conditional Independence Assumption (CIA) through uncontrolled differences between treatment and control groups in the probability of harmful employment transitions. Finally, the study is complemented using a macro-approach in the spirit of Harasztosi & Lindner (2019). To this end, the analysis focuses on the difference between the pre- and post-treatment reform hourly wage frequency distributions. While suggestive, the analysis provides evidence of moderate employment loss in the economy in the year following the reform.

This chapter contributes to the vast empirical literature on the employment impact of minimum wages. Despite the growing number of microeconometric papers in the literature, the evidence on negative employment effects of MW remains mixed (e.g., see interesting surveys by Neumark & Wascher, 2010; Card & Krueger, 1995; Manning, 2021). Ultimately, a large bulk of the literature finding negligible MW negative effects is criticized because they focus on small and temporary MW reforms (Sorkin, 2015; Aaronson et al., 2018). Similar to Dustmann et al. (2022) and Haratoszi & Lindner (2019), this study addresses this issue directly by exploiting a large and permanent sharp increase in the minimum wage.

The chapter is structured as follows. Section 2 reviews the literature, both nationally and internationally. Section 3 describes the institutional setting and data sources. Section 4 explains the identification strategy. Section 5 describes the results and Section 6 summarizes the robustness check. Finally, section 7 discusses the limitations of the study and Section 8 concludes.

2. Literature Review

Due to the mixed nature of the empirical evidence, the vast research literature on labor impacts of MW remains highly contentious (Manning (2021)). For its part, empirical research has shown that an increase in the minimum wage does not necessarily have a negative effect on employment. Several papers stand out in this literature including seminal contribution by Card and Krueger (1994) and, more recently, studies such as Cengiz et al. (2019, 2021) in the US case or Dustmann et al. (2022) in Germany. All in all, Neumark and Shirley (2021) show that most of MW articles find significant negative employment effects, particularly for low-skilled individuals.

The literature examining employment effects of successive MW in Spain can be deemed as large. Overall, research papers focusing on young workers (i.e., Dolado & Felgueroso, 1997; Dolado et al., 1997; González et al., 2003; Galán & Puente, 2012, 2015 and Arellano & Jansen, 2014) find negative employment effects. However, there are a few notable exceptions, including Cebrián et al. (2010), who find no effect on teenage employment, and Blázquez et al. (2009), who find a short-term positive effect of the MW on youth employment. In contrast, González et al. (2012) find a negative effect on immigrant worker employment that varies by gender and region of origin. More recently, Cebrián et al. (2020) examine the probability of maintaining employment of people affected by the MW for the 2017 increase, discovering a negative effect just before the increase's entry into force that is not appreciated after the increase.

The number of papers focusing on the 2019 reform is scarcer. Using a micro approach, Lacuesta et al. (2019) estimate a 0.8% decrease in full-time salaried employment. The authors use microdata from the 2017 CSWL and rely on a projection of the analysis of the 2017 increase. Using a similar approach to ours, Barceló et al. (2021) use the CSWL database to examine the employment effects of the 2019 reform. The authors find a net employment loss of between 6 and 11% for workers directly affected by the measure. The estimated impact on individual employment loss, on the other hand, is between 2 and 3 p.p. for people working full-time for 30 days per month, and up to 4 p.p. in terms of equivalent working hours for workers in the hospitality sector. One important limitation of these studies is that they exclude two particularly vulnerable groups of workers from the analysis: people working part-time and workers with contracts of less than a month. Using a more macro approach, the AIReF estimates an employment loss of 0.13 to 0.23 p.p., which implies a drop of between 19,000 and 33,000 affiliated workers (AIReF, 2020). The authors find evidence of uneven effects based on worker characteristics, with female and younger workers from less developed regions being more adversely affected. Using the Spanish Labor Force Survey (EPA), the Economic Cabinet of the workers' union *Comisiones Obreras* find that the decline in enrollment in the first quarter of 2019 was very similar to that of 2018, and the probability of maintaining employment for certain groups at risk due to the rise in the MW grows from 2018 to 2019. Based on this evidence, the authors conclude that the employment effects of the 2019 reform was close to zero.

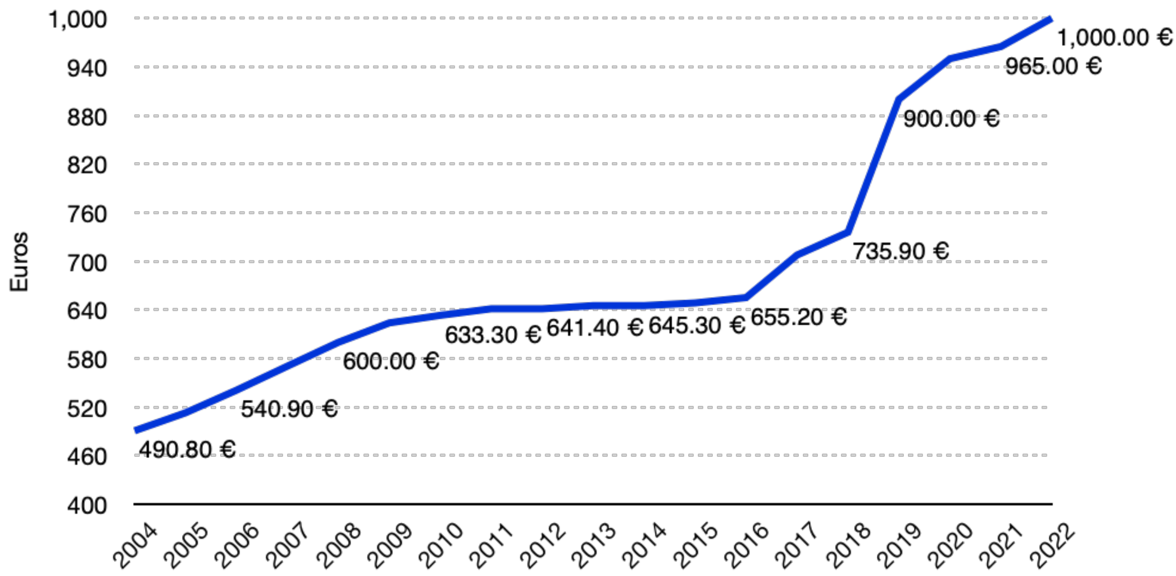
3. Institutional setting and data

3.1. Institutional setting

In Spain, the central government oversees annually the setting of MW after negotiations with the most representative unions and employers' associations. Should the negotiations between the three parties fail, the central government can unilaterally decide whether to adjust MW and by how much. Unlike in the United States, there are no territorial differences in the Spanish minimum wage since its application is national in scope. Since 1998, MW coverage extends to all workers, regardless of age or affiliation to a workers' union (Galán & Puente, 2015). Annual MW updates typically consider a number of factors, including the Consumer Price Index (CPI), the national average productivity, and the overall economic situation.

In recent years, the evolution of MW in Spain has been uneven (Figure 3). Between 2004 and 2008, the MW grew steadily from €490.8 to €600 (a nominal increase of 22.3% or 5.6% per annum). As a consequence of the economic downturn, the MW grew at a slower rate during the Great Recession and early stages of recovery, reaching €655.2 in 2016. Although the annual growth can be deemed as small (1.2% per year on average), it however ensured that the purchasing power of affected workers remained consistent with those stipulated in collective bargaining agreements (Barceló et al., 2021). After 2017, the MW began a faster growth. First, the center-right government of the Popular Party (PP) nominally increased minimum wages by 8% (from €655.2 to €707.7). Then, the center-left government of the Socialist Party (PSOE) introduced a sharp raise of the MW until €900 per month. Given that the Spanish MW in 2018 was €735.90, this decision represented a 22.3% year-on-year increase, which was unprecedented in recent decades. In subsequent years, further increases have occurred, albeit with a lesser magnitude than in 2019, until the MW reached €1,000 in 2022. The Spanish government has already indicated its willingness to increase the MW once more in the upcoming year.

Figure 3. Evolution of MW in Spain (2004-2022)



Note: The chart shows monthly MW at 14 full time payments. For the year 2004, it shows the MW in effect as of July 1. For the year 2021, it shows the MW in effect as of September 1.
Source: Ministry of Labor and Social Economy.

The 2019 MW increase occurred in a context of economic expansion and job creation, albeit with signs of deceleration. According to the Spanish Statistical Office (INE for its Spanish acronym), the Spanish economy grew by 2% in 2019, 0.3 p.p. less than in 2018 and one p.p. less than in 2017. Similarly, data from the Spanish Labor Force Survey (EPA) reveal a 2.3% increase in the number of employed individuals in 2019, which is 0.3 percentage points less than the increase in 2018 and 2017.

3.2. Data and descriptive statistics

Description of the data source. — The primary data source is the 2019 Continuous Sample of Working Lives microdata (CSWL or MCVL for its acronym in Spanish). The CSWL employs administrative tax records from the Spanish Social Security (TGSS) and the Spanish counterpart of the IRS. For the specific purposes of this paper, I use the CSWL version without tax data.

The CSWL records a 4% representative random sample of the total workers affiliated with the Spanish Social Security. The 2019 CSWL data details each worker's working history and earnings from their entry to the labor market until December 31, 2019. The CSWL has a panel data structure that allows to follow individual workers before and after the increase in the MW.

It is possible to observe individual personal characteristics of workers (e.g., gender and age), as well as registry dates of their employment and unemployment episodes of Social Security affiliation. For employment episodes, the CSWL includes, among other information, the type of contract and a part-time coefficient (hereafter, *coefpar*) that allows to impute number of working hours. Further, there is also information on the monthly contribution base for each individual and employment episode. In the analysis, this monthly contribution base is used as a *proxy* variable for wages.[8]

Based on the CSWL, a monthly panel that follows workers over the January 2018 and November 2019 period is constructed. Using individual worker-level information, the analysis uses monthly employment status to record worker-specific individual labor trajectories. As discussed later, the main analysis relies on comparing employment transitions after the introduction of the 2019 reform between a treatment and a control group. To define these employment transitions, November 2018 is designated as the baseline period or *t0*. Although December is the period immediately preceding the increase in the MW, the use November allows to alleviate seasonality concerns. Further, the analysis employs the second Tuesday of each month as the specific day to define employment transitions.[9]

To keep the panel tractable, the analysis is restricted to people working as employees and affiliated with the Social Security System on the second Tuesday of November, 2018. To enhance the validity of results, workers in the Special System for Agricultural Workers and workers in the Special System for Household Employees are incorporated to the study.[10] At the same time, I focus on people between age 16 and 60 as of January

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[8] For a detailed explanation of this database, see Pérez (2008).

[9] On this basis, we intend to reduce the variations in job creation and destruction caused by the calendar, which, in the case of Spain, have greater significance on the first and last days of each month, as well as on Mondays and Fridays (Conde-Ruiz et al., 2019).

[10] In the cases of these two special contribution systems, it is not possible to include people working part-time in the analysis, since the variable indicating the length of the working day does not exist, which makes it impossible to infer the number of hours worked and, consequently, the wage per hour worked. This fact is an important limitation in the case of the Special System for Household Employees, since 61.8% of the people in this special system have a part-time contract in t0, while the incidence of part-time work is practically non-existent in the case of the Special System for Agricultural Workers.

2018 (*t-10*) to exclude workers affected by potential transitions into retirement in the estimation period.

One data complication is that workers can display several employment spells in a given month. To define the monthly panel, I make the following decisions. In cases where an individual is both employed and unemployed in the CSWL, I keep the employment episode in the panel. Alternatively, the analysis excludes workers that hold multiple jobs or that are simultaneously employed and self-employed. As a result, 6.1% of observations in the sample are dropped.

Because information about specific creation or destruction of job positions is not available, an important limitation of the CSWL is that one cannot directly study job destruction. Thus, it is important to note that this analysis focuses on the specific impact of MW on individual employment transitions rather than on the aggregate impact on job destruction.

Sample selection and calculation of hourly wages. — To maximize representativeness and sample size, the analysis expands beyond full-time workers employed every day of the month. In particular, it includes full-time employees that do not work every day of the month and part-time workers, regardless of the number of days worked in a month. Previous work in Spain has typically excluded these types of precarious workers that are arguably more vulnerable to potential job loss from MW raises.

As a result of including these workers in the analysis, one needs to impute hourly wages to determine workers' position relative to the MW in the reference period. That is, I will directly assign individuals to treatment and control group based on whether their hourly wage in *t0* falls below or above the post-reform hourly MW. To determine the pre-treatment hourly wage, I first determine how many days each person worked during each time period. The number of daily hours worked in the reference period is then imputed using the work intensity coefficient *coefpar*. This is an important drawback of the data, that does not report actual working hours but a coefficient reflecting work intensity (ranging from 0 and 1000 for full-time workers). Also, because specific wage-earnings data is missing in the CSWL, I proxy workers' wages through data of Social

Security monthly contribution bases in the establishment each individual works. As a result, the formula used to calculate each worker's hourly wage proxy is as follows:

$$\frac{Monthly\ contribution\ basis}{Days\ worked\ in\ reference\ month}/Hours\ worked\ observed\ in\ period\ t$$

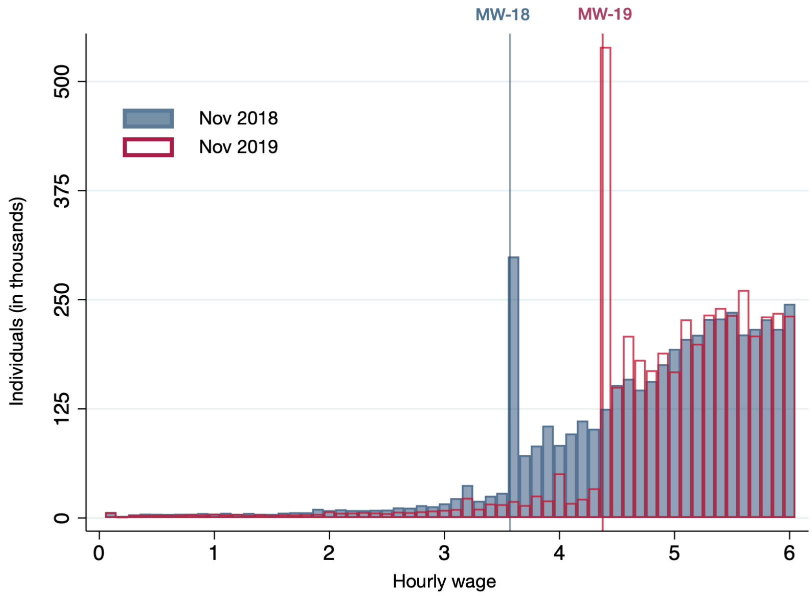
To impute numbers of hours worked from the part-time coefficient, full working day is set at 8 hours. As a result, I set to four the number of hours worked per day for, say, a person working half-time (i.e., with a work intensity coefficient of 500).[11]

To determine treatment status, each worker's hourly wage is compared with the post-reform hourly MW. That is, I consider as treated those workers who earned less than the newly-established hourly MW. To compute the 2019 hourly minimum wage, follow several steps are followed. First, because the MW is based on 14 payments and contribution bases are defined monthly, I prorate the €950 MW into 12 payments, thus resulting in a monthly MW of €1050. Second, because treatment status is assigned according to observed hourly wages, I re-express the post-reform MW into hourly wages. This approximation yields a MW threshold of €4.375 per hour.[12]

The hourly wage distribution before and after the 2019 reform. — Figure 4 depicts the hourly wage distribution prior to the new MW's implementation (November 2018) and immediately after (January 2019). As can be seen, the Figure shows a shift in wage distribution from the 2018 MW[13] to the 2019 MW and a decrease in the number of wage earners below this threshold.[14]

[11] Given that the full working day is not equivalent to 8 hours in all agreements, there is a small measurement error in this variable. The results should not be affected if this error is randomly distributed among the groups covered in this study.
[12] This reference is calculated by dividing the 12-month prorated MW (1050) by 30 days and then by 8 hours.
[13] For 2018, the MW in terms of wage/hour is €3.57.
[14] It is possible to find unusually low contribution bases, below the legal minimums, which may be due to errors or irregular situations.

Figure 4. Employees distribution (November 2018 and November 2019)

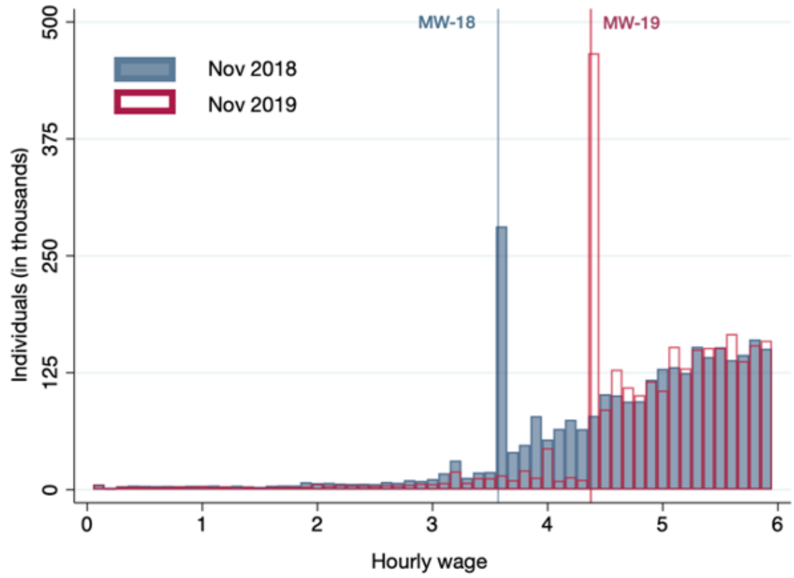


Source: Own calculations using CSWL

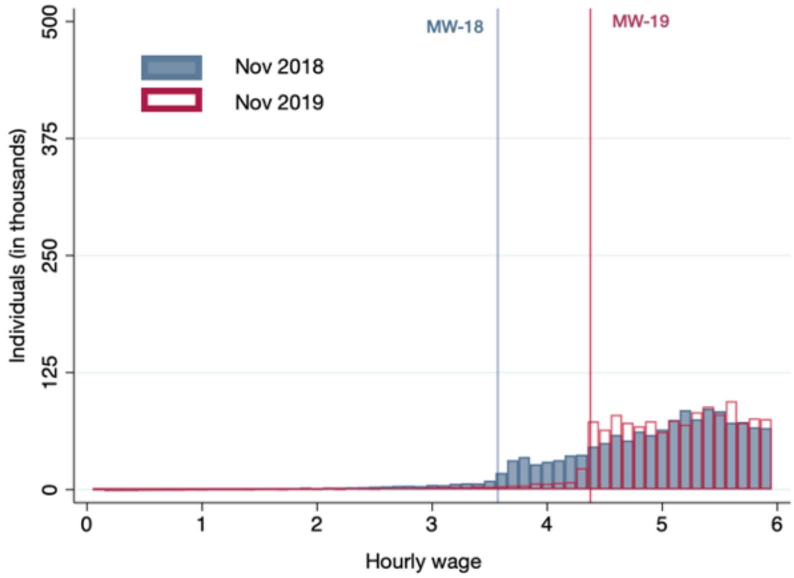
The data suggests a significant shift in the wage distribution for both full-time and part-time workers (see Figure 5). At the same time, it is possible to observe clear bunching of workers around the post-reform €4.375 hourly minimum wage threshold for both types of workers. Altogether, this suggests that the computation approach for hourly wages provides a fairly reliable identification of potentially treated workers, including part-time workers.

Figure 5. Employees distribution, by work day (November 2018 and November 2019)

a) Full-time



b) Part-time



Source: Own calculations using CSWL

Who are minimum wage workers? — Table 7 provides descriptive information about the proportion of workers affected by the reform (i.e., earning less than €4.375 per hour in November 2018). The results indicate that approximately 9% of the entire working population are affected by the MW increase. Descriptive statistics indicate that the new MW did not affect workers homogeneously. In particular, the raise in MW disproportionately affects women (10.6%), young people –notably those under the age of 26 (24.1%)—, and workers in firms with fewer than 10 employees (17.7%). I find sizable sectorial disparities of MW. The primary sector shows the highest incidence of the new minimum wage, with about 1 in 2 individuals affected (44.3%). The service sector has a relatively high prevalence, including hotels and restaurants (9.7%) and commerce (8.9%), as well as home, artistic, recreational, and social activities (27.3%).

Table 7. Incidence of new MW in November 2018 (%)

Group	% affected by MW raise
Total	**9.0 %**
Gender	
Men	7.5%
Women	10.6%
Age	
16-25 years	24.1%
36-34 years	10.6%
36-44 years	6.8%
45-54 years	6.7%
55 years and older	6.0%
Establishment size	
Less than 10 employees	17.7%
10-49 employees	7.7%
50-249 employees	6.3%
250 or more employees	3.0%
Economic activity (sector)	
Primary	44.3%
Mining and quarrying, manufacturing, supplies	3.6%
Construction	2.9%
Retail	8.9%
Transportation and storage	4.3%
Accommodation and food service	9.7%
Information and communication	5.3%
Financial, insurance and real estate	5.8%
Professional, scientific and technical	8.5%
Administrative and support services	12.7%

Public administration	6.3%
Education	8.0%
Health and social work	5.1%
Household, arts, entertainment and recreation	27.3%

Source: Own calculations using CSWL.

4. Methodology and identification strategy

Evaluation approach. — When establishing causality, it is desirable that individual (un)observed features between treatment and control groups differ as little as possible. Randomization achieves balance between two groups but rarely occurs in policymaking. The 2019 MW reform is no exception in this regard. In addition, because minimum wage in Spain is negotiated at the national-level, a control group of strictly comparable workers is unavailable (i.e., individuals who receive a wage below the new MW and who are not affected by the reform). These two elements hinder the ability to address selectivity into treatment using quasi-random (regional) variation (e.g., Cengiz et al. (2019,2021)).

To tackle these limitations and reliably assess the impact of the MW increase, the approach fundamentally relies on comparisons of employment transitions between a group of workers affected by the reform (treatment group) and a set of workers unlikely impacted by the raise (control group). I define treatment assignment statically using workers' November 2018 (*t0*) hourly wage position relative to the 2019 MW threshold (€4.375 per hour). This empirical approximation presumes that (1) individuals earning less than the 2019 MW in *t0* are directly affected by the reform and (2) workers with wages above this threshold are not impacted by the change.[15] Further, this approach crucially relies on a high comparability between workers in the treatment and in the control groups. That is, employment transitions of members in the control group need to provide a good approximation of the counterfactual transitions of treated workers, in absence of the MW increase.

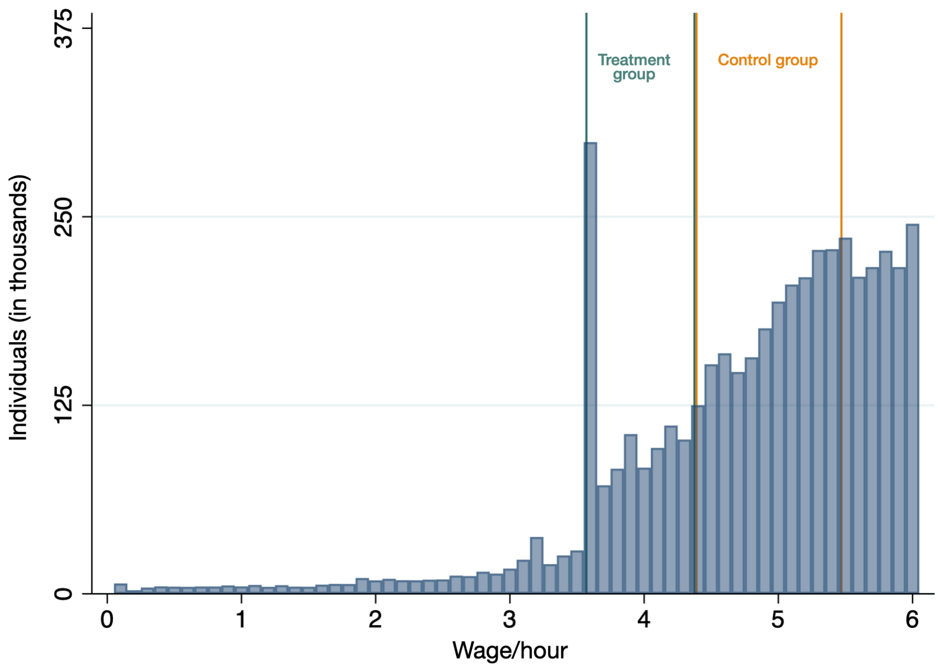
The main outcome variable identifies monthly employment transitions experienced by workers relative to *t0* (where all of them were employed). To examine the dynamic nature

[15] A possible effect that is not possible to capture in this analysis is that the increase in the SMI entails indirect effects whereby workers with wages above the MW would be negatively affected by the reform. If so, the results of the impact estimation could contain downward biases. This and other limitations of the impact study are discussed in detail in the corresponding section.

of the reform's impact, I define monthly transitions between November 2018 and 2019. The key idea is to assess the impact, from a worker's perspective, on the extensive margin (i.e., probability of employment loss) and on the intensive margin (i.e., probability of a reduction in number of working hours). To do so, three possible individual transitions are defined: (1) the affected person remains employed and their work intensity does not decrease, (2) the affected person remains employed but their work intensity is lower than at *t0*, and (3) the affected person transitions to unemployment (understood as non-employment).

Implementation of identification strategy. — I begin by comparing workers located on opposite sides of the 2019 MW cutoff in the hourly wage distribution. To mitigate possible productivity differences between groups, I define two relatively narrow hourly-wage intervals to construct treatment and control groups (see Figure 6). More specifically, we assign workers with hourly wages between the 2018 MW (€3.57 per hour worked) and the 2019 MW (€4.375) to the treatment group. Alternatively, individuals with wages between 102% and 125% of the new MW (or equivalently, between €4.39 and €5.47 per hour worked) are assigned to the control group. Individuals with hourly wages below 2018 MW threshold are discarded due to potential lack measurement error in the CSWL (i.e., firms could not pay wages below €3.57 per hour in *t0*). Workers with salaries in the 100-102% MW range are also excluded to minimize the risk of incorrectly assigning treated individuals to the control group (see hourly wage imputation process described in subsection 3.2).

Figure 6. Treatment and control groups



Source: Own calculations using CSWL.

One unlikely possibility is that falling into the treatment or the control group (or equivalently, to the left or the right of the 2019 MW threshold) is "as good as random". While this assumption cannot be directly tested, it is possible to evaluate the balancing of several observed characteristics between the treatment and the control group. Table 2 shows that there statistically significant (*p*<.001) and quantitatively large differences between these two groups. Ultimately, this suggests that one should pursue additional strategies to address selectivity into treatment and mitigate bias in the estimation process.

To enhance balance between treated and control units, we rely on a matching strategy. In particular, I apply a *Coarsened Exact Matching* (CEM) approach to match treated workers in a 1:1 ratio to controls using pre-treatment characteristics at *t0*. More specifically, the matching procedure performs exact matching between workers below and above the 2019 threshold on the following criteria: gender, nationality, establishment size intervals, contribution group, contribution regime, industry, and type of contract. This approach implies almost exact matching after coarsening the following continuous variables into intervals: age groups and work intensity coefficient.[16]

[16] Since it is possible to find multiple partners for the same individual, one is selected at random. Thus, all individuals in the treatment group are matched with the same number of individuals. As a result, when this

Compared to exact matching or PS matching, this enhances the overlap between treatment and control groups, while simultaneously avoiding practical drawbacks like the "curse of dimensionality" or the incorrect specification of the propensity score. Workers left unmatched by the algorithm are excluded from the analysis. In order to maximize sample size and enhance comparability, the analysis uses matching with replacement, that allows control units to be paired with multiple treated units. Applying this matching strategy, I find one control unit for 31,238 workers (out of a total 35,144, 88.9%).[17] Table 8 describes the quality of the matching procedure. By design, I find that differences in observable characteristics between the treatment and control groups are not statistically significant.

Table 8. Differences in means between treatment and control groups, before and after matching

	Pre-matching			Matching		
Group	Treatment (1)	Control (2)	Test (p-value) (3)	Treatment (4)	Control (5)	Test (p-value) (6)
Gender						
Men	43.4%	42.4%	0.001***	42.3%	42.3%	1.000
Women	56.6%	57.6%	0.001***	57.7%	57.7%	1.000
Age						
16-25 years	23.2%	13.3%	0.000***	19.1%	19.1%	1.000
36-34 years	28.2%	27.7%	0.1444	28.8%	28.8%	1.000
36-44 years	23.4%	29.1%	0.000***	25.2%	25.2%	1.000
45-54 years	19.0%	22.5%	0.000***	20.4%	20.4%	1.000
55 years and older	6.3%	7.4%	0.000***	6.5%	6.5%	1.000
Type of contract						
Permanent Full-Time	35.6%	48.3%	0.000***	39.3%	39.3%	1.000
Permanent Part-Time	15.9%	18.7%	0.000***	16.4%	16.4%	1.000
Temporary Full-Time	27.9%	19.8%	0.000***	30.6%	30.6%	1.000
Temporary Part-Time	11.7%	12.6%	0.000***	12.0%	12.0%	1.000
Establishment size						
Less than 10 employees	55.5%	42.8%	0.000***	54.8%	54.8%	1.000
10-49 employees	19.8%	24.5%	0.000***	20.4%	20.4%	1.000
50-249 employees	14.7%	16.6%	0.000***	14.6%	14.6%	1.000

technique is applied, the distributions of the treatment and control groups are identical, as well as the number of observations in both groups.

[17] Once matching is carried out, the sample of treated individuals accounts for 65% of all individuals who had a wage below the new MW at *t0*.

250 or more employees	9.9%	16.1%	0.000***	10.2%	10.2%	1.000
Work intensity						
Coefpar = 1000	71.6%	66.1%	0.000***	70.9%	70.9%	1.000
Coefpar [750-1000)	8.8%	12.1%	0.000***	9.0%	9.0%	1.000
Coefpar [500-750)	12.4%	14.3%	0.000***	13.1%	13.1%	1.000
Coefpar [250-500)	5%	5.2%	0.190	4.9%	4.9%	1.000
Coefpar < 250	2.2%	2.3%	0.441	2.0%	2.0%	1.000
Observations	*35,144*	*77,404*		*31,238*	*31,238*	

Note: We report the differences in observed characteristics between treatment and control groups before (columns (1) and (2)) and after (columns (4) and (5)) the matching procedure. Columns (3) and (6) report the p-values for tests for statistically significant differences in observed characteristics. In these columns, ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.
Source: Own calculations using CSWL.

This matching approach relies on a selection-on-observables type of identification, which requires that employment transitions of control units provide a reliable approximation of the counterfactual situation of treated units, in absence of the reform. That is, to identify the effect of the 2019 MW raise, it is needed that, conditioning on observed covariates, treatment assignment is independent of potential employment transitions in the case of no treatment. I believe that this matching approach somewhat improves traditional regression modelling strategies. While “controlling” for covariates through regression is (in principle) a good strategy, it has some limitations. First, regression modelling is not transparent about the distribution of covariates between treatment and control groups. Second, regression heavily relies on model specification through functional forms (i.e., extrapolation), unless there exists significant overlap between treatment and control groups.

An important drawback of this empirical strategy is that the Conditional Independence Assumption (CIA) is highly restrictive. In this sense, difference-in-differences (DID) methods constitute, in principle, a more attractive empirical approximation. For this reason, I first experimented with adopting an event-study design on the matched sample for causal identification. Ultimately, this possibility was discarded because significant pre-trend differences were found between treated and control units. Thus, the final decision reflects preferences towards pursuing a more transparent design that, despite its flaws, provides a better intuition about the target causal parameter being estimated.

Regression specification. — To estimate employment effects of MW, I compare monthly employment transitions of treatment and control units relative to baseline period *t0* (i.e., November 2018). To do so, I use multinomial logit regression in the matched sample. Again, our dependent variable reflects three different monthly transitions: (1) the person remains employed and their work intensity does not decrease, (2) the person remains employed but their work intensity is lower than at *t0*, and (3) the affected person transitions to unemployment (understood as non-employment). Formally, I propose the following log-odds linear-specification for each transition *j* = {1,2,3}:

$$\rho_{ijt} = log\frac{\pi_{ijt}}{\pi_{i1t}} = \alpha_{jt} + \beta_{jt}MW_i + X_i'\delta_{jt},$$

with the following multinomial probabilities:

$$\pi_{ijk} = \frac{exp(\rho_{ijk}\}}{\sum_k exp(\rho_{ijk})} \text{ for } k = \{1,2,3\},$$

where t = {1,…,12} and $\rho_{i1t} = 0$ following convention. Here, α_{jt} is a transition-specific constant and δ_{jt} captures the set of regression coefficients for covariate vector X_i' of individual baseline characteristics: sociodemographic variables (sex, age, and nationality) and labor variables (type of contract, type of working day, sector of activity, establishment size, and contribution group). The decision to include this set of covariates stem from efficiency concerns. The variable MW_i is our dummy-variable of interest that takes value 0 if unit *i* belongs to control group (i.e., earns an hourly wage above the 2019 MW) and 1 if she belongs to the treatment group. The model is estimated for each time period t = {1,…,12} individually. Thus, I obtain monthly-estimates of parameters α, β and δ. For interpretation purposes, I transform coefficients into average marginal effects. For the sake of brevity, only the corresponding average marginal effect of β_{jt} are reported, that is the main parameter of interest.

5. Results

Main results. – Figure 7 details regression estimates of the impact of the reform (relative to *t0)* for two outcomes: the probability of decreasing number of hours worked (shown in Panel A) and the probability of transitioning to unemployment (Panel B). More specifically, Figure 5 plots the average marginal effect of our variable of interest MW_i

from the above multinomial regression. Detailed results from regression analyses are presented in Appendix B Tables B1 though B4.

Estimates from Panel A suggest moderate, although statistically significant effects, on the intensive margin of adjustment (reduction in working hours). Panel A shows that workers affected by the reform face a higher probability of reducing job intensity than the control group. I find that the monthly effects become statistically significant at $p<.05$ two months after the reform (*t+3*). Overall, the results indicate that effect sizes remain relatively stable and close to zero between *t+2* (0.1 p.p.) and *t+10* (0.3 p.p.) Estimates are however relatively larger in the last two periods of analysis. By *t+12*, I find that the minimum wage reform increases the probability of reducing working hours by about 0.84 p.p. for treated individuals.

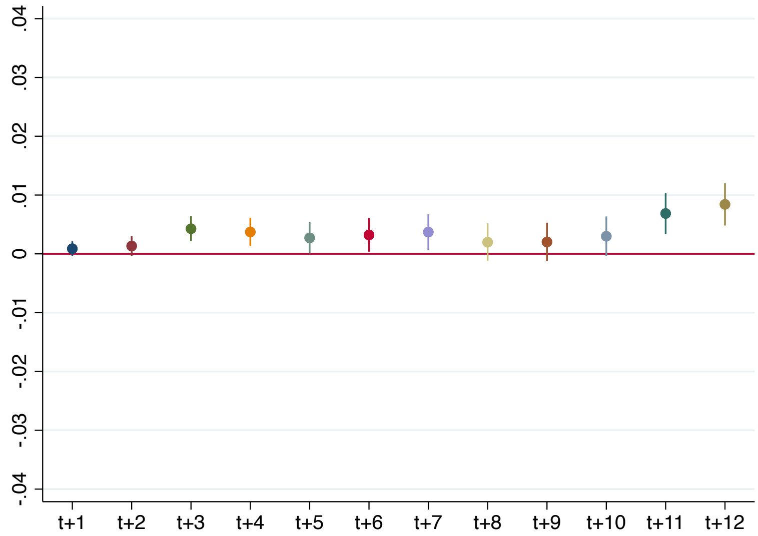
Employment loss estimates from Panel B indicate no immediate sizable effects for workers earning less than the MW in the baseline period. In particular, I find no statistically significant effects on the probability of job loss until *t+5* (0.61 p.p.). Yet, results indicate substantial time-varying heterogeneity. In particular, I find that, relative to *t+5*, the employment loss effect nearly doubles by *t+7* (1.2 p.p.) and triples in *t+12* (1.92 p.p.). Ultimately, this suggests (non-monotonically) increasing impacts on employment loss in the medium term.

Altogether, comparisons between both panels suggest that most of the negative effect of the MW increase is captured through the extensive margin (employment loss). For instance, one year following the MW increase (*t+12*), approximately two-thirds of the negative impact observed is attributable to the loss of employment (1.92 p.p.), while the remaining third is attributable to the adjustment in work intensity (0.84 p.p.).[18] This qualitative pattern is comparable to that found by Barceló et al. (2021), who also observe a greater effect on unemployment than on working time adjustment. Considering the 22,3% increase in the MW, these results indicate that the elasticity of employment loss to the MW at time *t+12* is -0.086, which is close to the median elasticity (-0.112) obtained by Neumark and Shirley (2021).

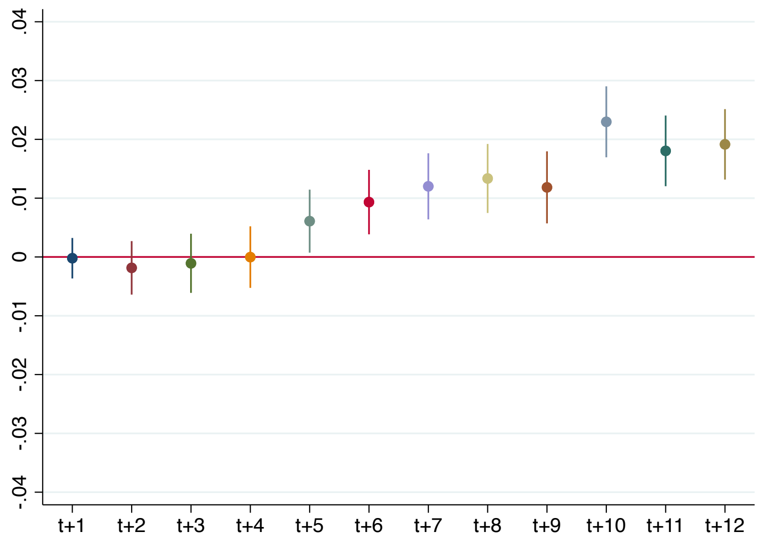
[18] Appendix I contains the tables with the estimations results.

Figure 7. Impact of MW raise on employment

a) Employment with work intensity reduction



b) Unemployment



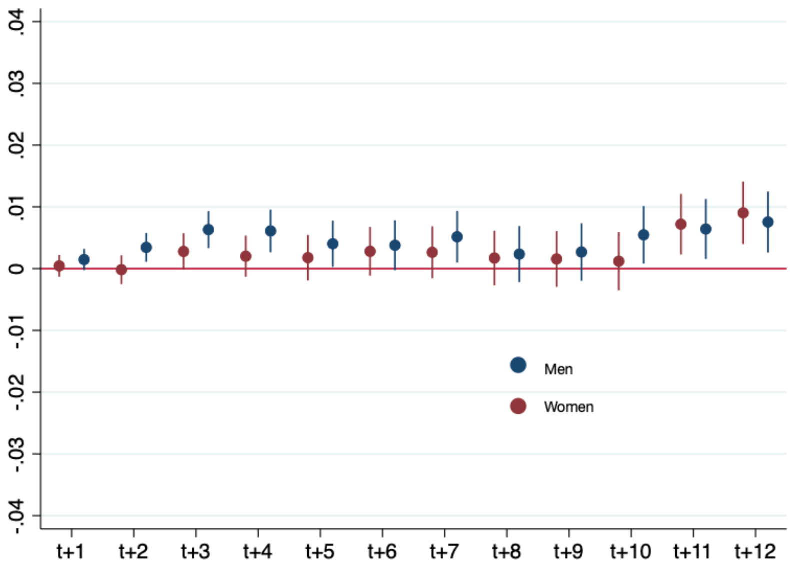
Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

Heterogeneity by group. —Thus far, the evidence suggests that the 2019 MW increase had no immediate impact on employment (during the first four months), but gradually increased over time. In addition, results indicate that this adjustment primarily occurs through a greater loss of employment (extensive margin) than through reductions in work intensity (intensive margin). Yet, it is possible that these results hide significant heterogeneity based on workers' characteristics. Thus, we analyze possible impact heterogeneity for several groups, including men and women, people over and under 30 years of age, and full-time versus part-time workers.

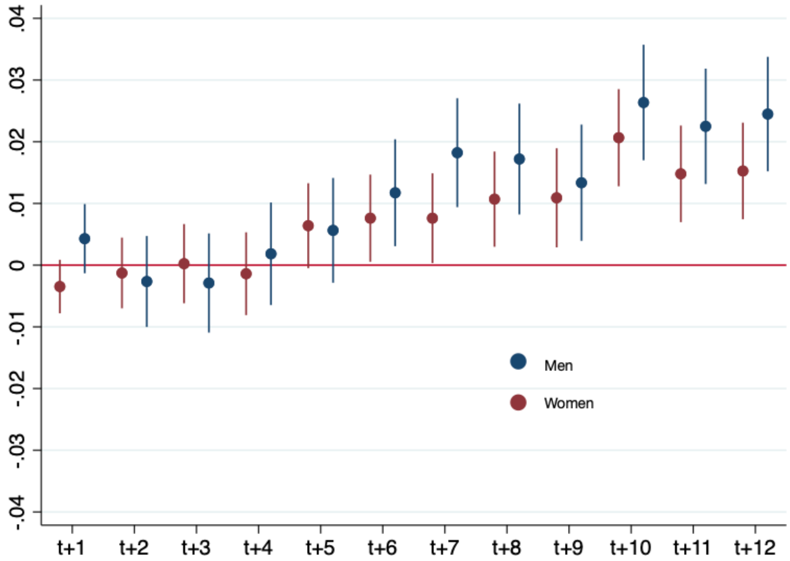
Separate results for men and women are shown in Figure 8. Panel A indicates that the adjustment in work intensity immediately after the minimum wage reform primarily affected men (approximately 0.34 p.p. in *t+2* and 0.63 p.p. in *t+3*). These significant effects, despite being close to zero, persists throughout most of the analysis horizon. Women, on the other hand, do not appear to be affected by work intensity adjustment until *t+11* (see Tables A2 and A3 in Appendix for additional details). Figure 6 panel B reveals that the effect on job loss follows a similar trend for men and women. Although the effect is larger for men than for women in most periods, the large overlap of standard errors between two type of workers prevents us from concluding that the increase had a greater impact on men.

Figure 8. Impact of MW raise on employment, by gender

a) *Employment with work intensity reduction*



b) *Unemployment*

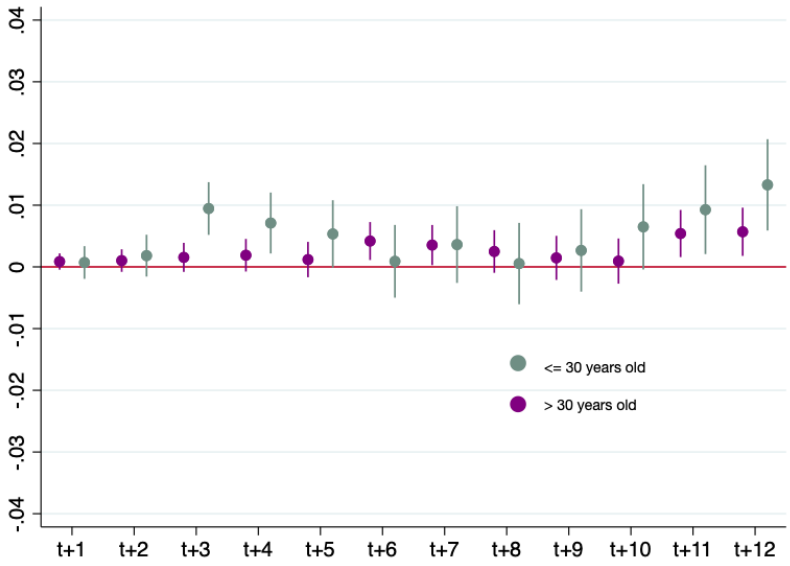


Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors. Source: Own calculations using CSWL.

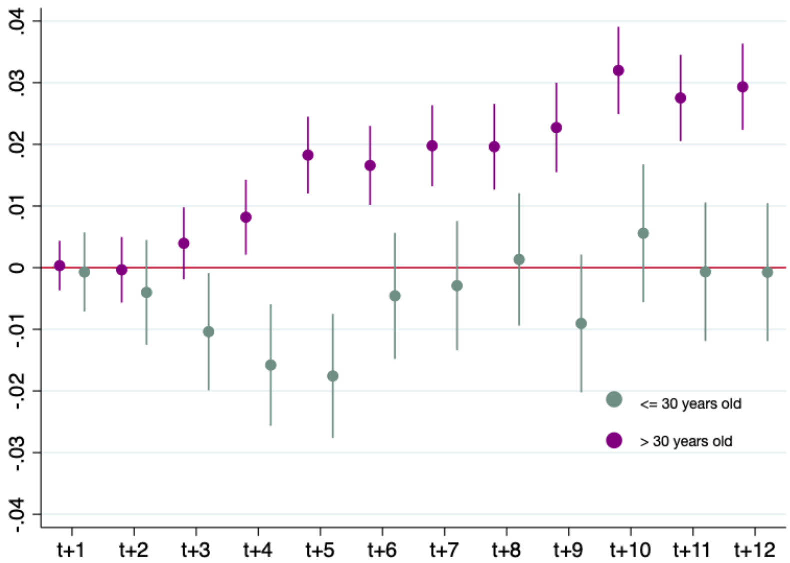
Figure 9 depicts the same analysis focusing instead on two age groups (i.e., below and above 30). In this instance, both panels reveal substantial differences between the two types of workers. Overall, people older than 30 show, on average, a larger impact on the probability of losing a job, whereas younger workers show a quantitatively greater impact on the probability of reducing number of working hours. Interestingly, I find a negative unemployment effect for young people between *t+3* and *t+5*. That is, I find that young workers affected by the reform have a positive significant employment effect that banishes by *t +8*. Panel A reveals, however, that the reduction in working hours is greater on workers under the age of 30 years. This effect is significant in the short-term (approximately 1 percentage point in t+3) and becomes more pronounced one year after the increase (1.3 p.p.).

Figure 9. Impact of MW raise on employment, by age group

a) Employment with work intensity reduction



b) Unemployment

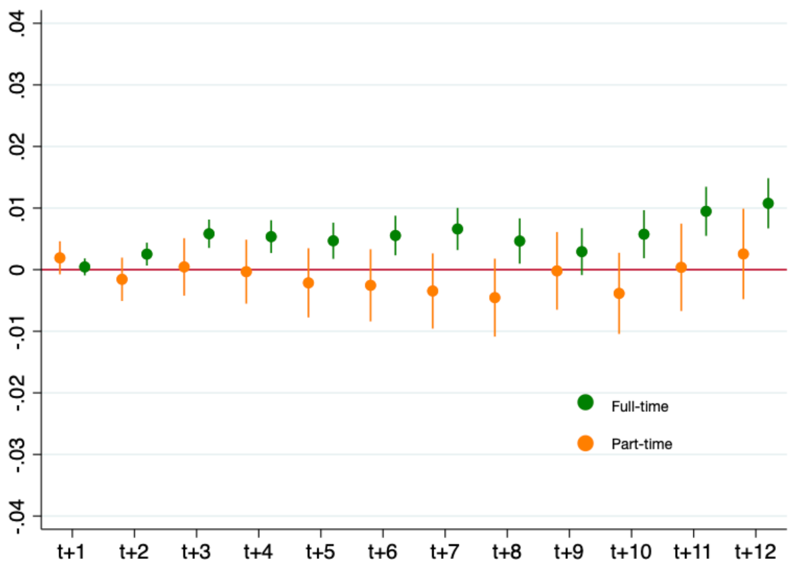


Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

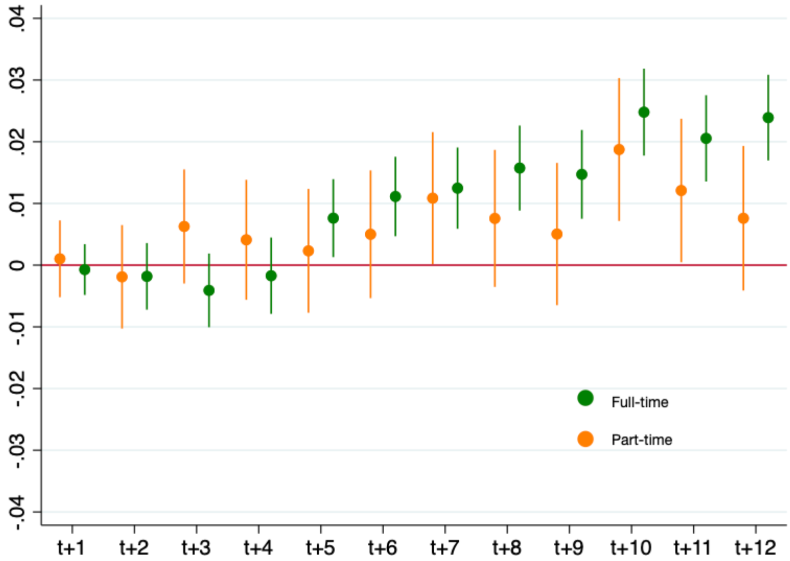
Lastly, our findings suggest significant impact heterogeneity of MW reform depending on work intensity of workers in *t0* (Figure 10). Results indicate that the reform implies a quantitatively larger impact on full-time workers than on part-time workers. Despite differences between part-time and full-time workers being small at first, I find that full time workers experience a greater increase in the job loss probability over time. In this regard, results indicate statistically significant differences in the probability of unemployment by *t+12* (2.39 p.p. for full-time workers as opposed to 0.8 p.p. for part-time workers). Similarly, I find that full time workers also experience a larger adjustment in work intensity than part-time workers. Results reveal an overall increasing impact on the intensive margin for full-time workers (i.e., the impact is 0.25 p.p. in *t+2* but grows until 1.08 p.p. in *t+12*). Part-time workers, on the other hand, face little adjustment in work intensity, as point estimates remain relatively stable in the period of analysis.

Figure 10. Impact of MW raise on employment, by workday

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors. Source: Own calculations using CSWL.

6. Robustness checks

To assess whether our findings are robust, I examine the validity of the results with several robustness checks. Specific details are reported in Appendix B, but the main results are summarized here.

Alternative matching specification. — The previous analysis employs a selection-on-observables type of identification, which is based on the conditional mean independence assumption (CIA). Ultimately, the plausibility of this condition heavily relies on the number and quality of variables used in the matching process. One clear limitation of our analysis is that I do not control for arguably important observable (e.g., autonomous region) and unobservable (e.g., productivity) features. To evaluate the sensitivity of previous findings, I re-run the analysis after experimenting with a more stringent matching procedure in terms of individuals' employment histories. I explain additional details and summarize the findings in Appendix B. Overall, the statistical and quantitative significance of findings remains. Comparing both matching strategies, the estimated employment-loss (intensity-reduction) effect goes from -1.92 p.p. to -1.44 p.p. (-0.84 p.p. to -0.79 p.p.) after one year. One important limitation is the inability to condition on even larger set of covariates without losing too many observations (i.e., by adding some covariates in the new matching specification, the effective treatment sample size already drops from 31,238 units to 20,409). Thus, the general approach seeks a reasonable compromise in the trade-off between strict comparability of treatment groups and sample size trade.

Placebo test. — In this setting, the plausibility of CIA is a strong condition that can be easily argued against theoretically. Thus, I perform a placebo test using the same procedure described in Section 4. The aim is to empirically argue that findings do not primarily reflect innate differences between treated and control units in the probability of losing employment or reducing work intensity. To do so, I replicate the baseline analysis by evaluating a fictitious MW raise in May 2018. If significance of results from Section 5 were primarily driven by baseline differences of detrimental employment transition, one would expect negative significant effects also under this placebo test. In contrast to the main analysis, I do not find significant differences between treated and control subjects (see Figure B2 for details). This finding reinforces the previous results and suggests that differences between treated and control workers are not the main driver behind the findings.

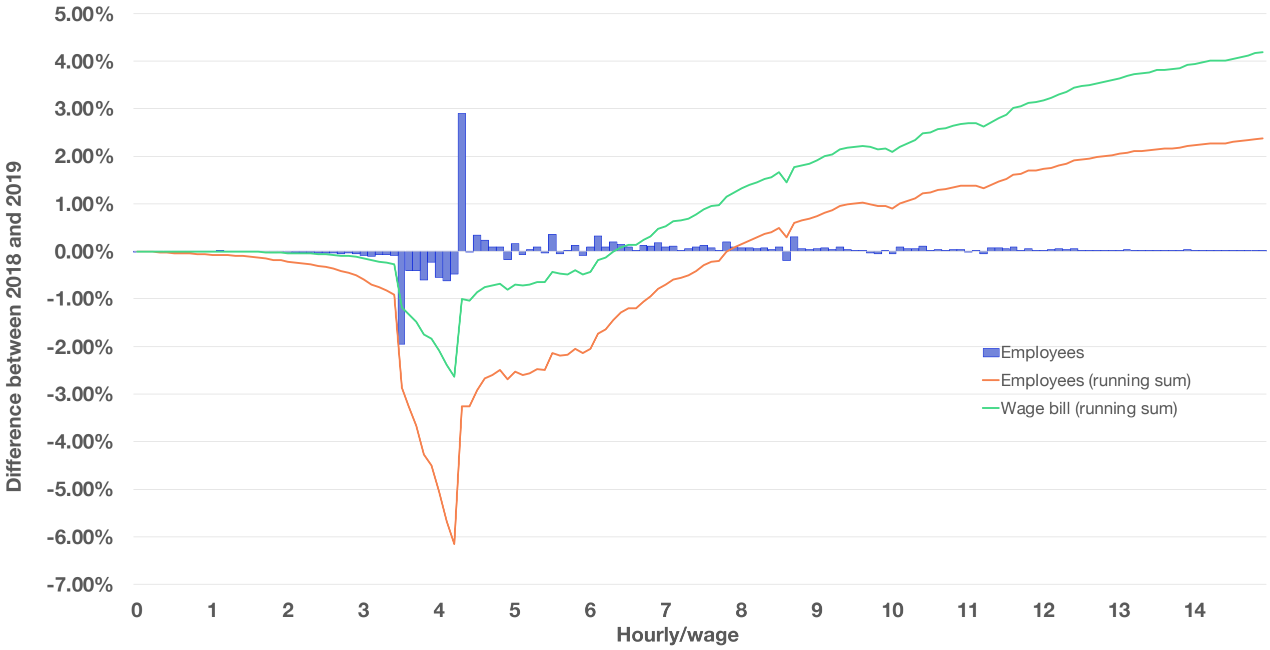
Additional sensitivity analyses. — I perform two additional sensitivity checks to assess robustness to changes in the specification. On the one hand, I experiment with using the second Saturday (instead of the second Tuesday) of each month as the reference period to define employment transitions. In doing so, individuals who work only on weekends that are excluded from the original analysis are included. Results of this check yield highly comparable estimates relative to the main analysis (see Figure B3). On the other hand, I re-run the analysis using September 2018 as the *t0* baseline reference to rule out potential anticipation effects (Cebrián et al. (2020)) The results of this robustness test do not indicate that such an effect occurs for the 2019 increase (see Figure B4).

Bunching. — Finally, I perform an additional check using aggregated data to complement previous results. To this end, I follow a similar bunching approach to that of Harasztosi & Lindner (2019). The main goal of this approach is to examine differences between the pre- and post-treatment reform hourly wage frequency distributions and evaluate whether excess workers above the MW between 2019 and 2018 compensate missing workers below this threshold. For this purpose, I use wage distributions from

Figure 3 to calculate the difference in workers between November 2018 and 2019 for each hourly wage bin. To normalize worker counts, these differences are expressed relative to total employment in November 2018. Blue bars thus show the relative change in number of employees in a given wage bin relative to November 2018. To represent the cumulative employment loss or gain up to a given wage range, the figure depicts a line (orange) that captures the running sum of cumulative changes up to each hourly wage bin. To represent the analogous loss or gain in the wage bill, I also plot the cumulative change in the wage bill using a green line.

The findings in Figure 11 highlight two main conclusions. First, there is a clear decrease in the number of employees below the new MW (€4.375). At the same time, there is a significant increase around the new MW and, to a lesser extent, in some higher wage ranges. Second, the negative cumulative sum until relatively high wage bins indicates a significant number of missing employees between 2018 and 2019, suggesting negative employment effects of the reform. The running sum drops to a sizable negative number (around 6% of pre-reform employment) below the minimum wage. It then recovers and relatively stabilizes in the 2% range, before increasing again at a faster rate around the 6€ hourly wage bin. While it is true that I observe a total positive change in cumulative employment, results indicate that the decrease in the number of employees is not compensated until the €8 per hourly wage bin. As far as the wage bill is concerned, there is a significant drop and subsequent recovery between the ranges of the old and new MW. However, the observed difference is 1% and the cumulative loss is compensated in a wage range closer to the MW (€6.4 per hour).

Figure 11. Difference in employee distribution between 2018 and 2019, by wage bins



Note: The figure depicts the difference between the two wage distributions shown in Figure 2. The orange (green) line shows the running sum of employment (wage bill) changes up to each wage bin.
Source: Own calculations using CSWL.

7. Limitations of the study

The results presented in this chapter are subject to a number of caveats. First, the analysis use contribution bases to determine whether a person belongs to the treatment or the control group. Due to the presence of bonuses and other extraordinary payments, there exists an uncertain number of workers whose contribution base is greater than the MW despite receiving a lower base wage. Because the data does not allow to deduct these allowances to compute base salaries, the use of contribution bases results in an incorrect assignment into the control group of a number of workers directly affected by the reform. Ultimately, the presence of potentially treated units in the control group introduces bias in the estimation. Intuitively, the size of the bias depends on the degree to which these individuals face a lower risk of job loss than the control group. That is, if misassigned workers have a lower risk of job loss than the control group, the actual impact of the minimum wage increase would be overestimated. On the contrary, if this was not true, estimates can be biased downward (as the real differences between the two groups would be artificially diminished).

Second, due to the inclusion of part-time workers in the analysis, findings are also susceptible to misclassification of these type of workers into the treatment and control groups. In this approach, a person belongs to either group based on which side of the hourly wage threshold they fall on. Due to the use of the imperfect work intensity coefficient *coefpar* and assumptions about the number of hours worked, this methodology is subject to risks of measurement error. Ultimately, this limitation constitutes an additional factor for potential misclassification of units in the treatment and control groups.

Third, inasmuch there exist unobservable differences between treatment and control groups, this approach suffers risks of CIA violations. Thus, the results of the study should be interpreted with caution. Since treatment assignment is based on pre-reform hourly wages, there exists potential productivity differences between workers in the treatment group and those in the control group, resulting in upward bias of the reform. While I have performed a placebo analysis to assess the extent of this bias, it is not possible to conclude with absolute certainty that the estimates partially reflect these differences.

Finally, it should be noted that the findings of the study should not be interpreted as evidence of job destruction or a slowdown in job creation. The reason being that the analysis focuses on job loss or reduced work intensity from the worker's perspective. In this sense, this approach cannot conclude whether a firm or establishment has decided to reduce its workforce or freeze new hires in response to the rise in the MW.

8. Conclusions

This article provides complementary evidence of the employment effects of a large raise in the minimum wage in Spain. To this end, the analysis uses administrative Social Security records from the 2019 CSWL. The identification strategy compares employment transitions between a group of workers who earned less than the newly-established MW prior to the reform and workers who earned more than the minimum wage threshold. Although this approach is not new, this article extends previous literature of MW in Spain in several ways. First, the study incorporates two relevant worker types traditionally excluded from the analysis: part-time workers and employees working less than the entire month. Second, I explore the impact of MW raises both on the extensive margin (probability of employment loss) and the intensive margin (probability of work intensity reduction). Third, I study impact on monthly transitions, therefore assessing the effect of the reform both in the short and in the medium term.

The findings show that the reform had no effect on employment in the period immediately following the increase (up to five months after the increase). However, a significant negative effect emerges thereafter, primarily through the extensive margin. In the 12 months following the reform, we find a negative effect of 1.92 p.p increase in the probability to unemployment transition. Taking into account the nominal increase of 22,3% in the MW, this result indicates an elasticity of -0.086 between the loss of employment and the MW. As with work intensity through a reduction in working hours, we find a relatively small effect that also grows over time. Quantitatively speaking, the effect size is significantly smaller than that observed to job loss (0.84 p.p.).

From the heterogeneity analysis, several conclusions can be drawn. First, the results show that there are few differences between male and female workers in terms of the probability of employment loss. However, there are differences in labor intensity, with a greater immediate impact on men and a greater medium-term adjustment for women. Second, there are some differences depending on the age of employees affected by the increase in MW. Specifically, the results indicate that older workers suffer to a greater extent from job loss, while in younger workers the effect is concentrated in labor intensity. Finally, I find notable differences depending on the type of working day of the affected workers, with a greater adjustment in labor intensity for those who work full time.

Chapter 3

When training meets commitment to hire: evidence from an Active Labor Market Policy in the Basque Country

1. Introduction

Active Labor Market Policies (ALMPs) have gained significant attention in recent years as effective measures to address the challenges of unemployment and labor market mismatches (Jahn & Rosholm, 2018; Kluve & Card, 2011). These policies refer to a range of initiatives and interventions that aim to enhance employability, job search activities, and reintegration of unemployed individuals into the labor market. The introduction of ALMPs is motivated by the need to reduce unemployment rates, improve labor market outcomes, and mitigate the socio-economic costs associated with long periods of unemployment.

Evaluation of ALMPs is a crucial undertaking in order to assess the outcomes of these interventions. By measuring the impact of these programs, policymakers can determine whether they are achieving their intended goals and if they are worth the investment of resources. Furthermore, understanding the impact of these courses can inform future program design and implementation, allowing for the refinement and improvement of ALMPs.

Although ALMPs have been extensively analysed in the literature, the evidence before the Great Recession was generally inconclusive and, in the case of training programmes, the effects on employment rates were rather modest (Kluve, 2010). However, since then, there has been renewed interest in these policies and their effects on employment rates. Card et al. (2018) show in their metanalysis that evidence suggests varying effects depending on the type of policy, with positive impacts seen for training and employment programs in the private sector, while those in the public sector may show no effect or even a negative impact in the short term.

This paper contributes to the academic literature by evaluating the impact on of an ALMP stablished in the Basque Country named *Training with Hiring Commitment*. This program aims to address the skills gap in the labor market by providing job seekers with training courses that align with companies' skill requirements. To qualify for participation, companies must demonstrate a shortage of these profiles in the labor market and commit to hiring a minimum number of individuals. This is an innovative policy that combines training programs for unemployed individuals with direct access to employment opportunities within participating organizations. Numerous studies have investigated the effects of ALMPs on employment outcomes (see Card et al. 2018; Kluve 2010). However, it is worth noting that these analyses often evaluate ALMPs that provide either training or access to job opportunities to the unemployed participants. Therefore, the inclusion of a hiring commitment from companies in this particular training program gives rise to significant interest as it can contribute valuable evidence to the existing body of literature.

To conduct the analysis, a set of databases compiled from administrative records were utilized. By combining records of program participants and training activities with information from job seekers from the Basque Public Employment Service (PES) and work histories from the Social Security Administration, it is possible to develop a methodological approach that utilizes matching techniques and implements Difference-in-Differences estimators. Employing matching techniques based on observable characteristics of individuals to form treatment and control groups has been used in recent analyses of ALMPs, as it is the case of Burger et al. (2021) in their comprehensive analysis of the four main programs in Slovenia. In order to construct the control groups prior to the estimation of the effect, the analysis adopts a double approach. On the one hand, one control group is constructed employing matching techniques on Basque Public Employment Service (PES) job seekers database. On the other hand, a list of individuals pre-enrolled in some training courses or placed on waiting lists is used to construct the second control group. Employing these two complementary approaches provides additional robustness to the analysis, as the outcome of an impact evaluation relies on how the control group is constructed (Mayne et al., 2015).

Although observable characteristics largely determine participation in ALMPs (Burger et al., 2021), it cannot be ruled out that unobservable characteristics may be related to program participation. Therefore, the matching procedure is combined with a Difference-in-Differences approach (Heckman et al. 1998; Smith and Todd 2005) to address this issue.

To estimate the effect of the program on employment, two outcome variables are defined: probability of being employed and number of days worked in a given month. While the outcome on employment status is widely used in the literature (Card et al. 2018), the effect on the number of days worked is of particular interest, as it provides a more comprehensive picture on the job quality and employment stability of participants.

Finally, the analysis follows treated individuals during the twelve months immediately after the program ends, which is the time horizon on which a notable majority of the existing literature focuses (Card et al., 2018).

The results of the employability impact assessment demonstrate a favorable outcome of the program, indicating an increase in both employment opportunities and duration of work. The effect is initially modest but grows over time, reaching a 20 percentage point increase in employment probability and an average of 13 days worked after one year (t+12). It should be noted that these short-term impacts predominantly apply to individuals who solely engage in training courses. Conversely, those who secure job contracts experience consistently positive effects from the program across all analyzed periods.

Moreover, the findings demonstrate that the influence on employability also varies based on the type of course. Courses in the Food Industry or Information Technology and ICT sectors have a significantly favorable effect, while courses in Commerce and Marketing or Hospitality and Tourism do not show statistically significant effects. Regarding analysis by demographic groups, there is generally a consistent impact across different groups, although certain groups standout such as women (particularly in the short term) or individuals over 55 years old. Additionally, there is a positive effect for individuals with medium/low educational attainment and no noteworthy effect for those with higher education levels. Finally, alternative control group results indicate a positive impact with a similar pattern to that found in the main analysis but at a lower magnitude (10 percentage points at t+12).

The rest of the paper is organized as follows. Section 2 reviews the literature on ALMPs evaluation. Section 3 describes the institutional setting and the data sources. Section 4 explains the identification strategy. Section 5 present the results and, finally, section 6 concludes.

2. Background literature

ALMPs encompass a range of interventions, including training measures and employment services, aimed at improving the employability and integration of unemployed individuals into the labor market. ALMPs consist of various tools such as subsidized employment, labor market services, and education and training programs (Tosun et al., 2017). These policies are believed to have the potential to counterbalance the negative effects of passive labour market policies, particularly on low-educated workers, by enhancing their human capital and social networks (Abrassart, 2012).

The significance of ALMPs in economic and labor market policy discussions is undeniable, as evidenced by their recurring presence in research. Ashenfelter's seminal work in 1978 (Ashenfelter, 1978) laid the foundation for empirical assessments of training program effects on earnings, setting the stage for the persistent interest in ALMPs. Ashenfelter's subsequent research in 1985 (Ashenfelter & Card, 1985) employed longitudinal earnings data to assess the impact of training programs, emphasizing the need for methodological rigor in evaluating ALMPs. Concurrently, in 1987, Ashenfelter argued for the use of randomized trials to evaluate training programs (Ashenfelter, 1987), marking a pivotal moment in the pursuit of rigorous policy evaluation. These early studies illustrate the enduring relevance and scrutiny of ALMPs in the labor economics literature.

In Card, Kluve, and Weber's (2018) comprehensive meta-analysis of recent ALMP evaluations, a wide range of program assessments are synthesized to provide valuable insights into the effectiveness of ALMPs. The study scrutinizes an array of active labor market interventions and their outcomes, offering a synthesized view of the current state of ALMP research. It covers various program types, including training, job search assistance, and subsidized employment, shedding light on the overall impact of these policies. Furthermore, Kluve's earlier work in 2010, which evaluates the effectiveness of European ALMPs in the context of the labor market, underscores the significance of local labor market conditions in shaping the success of ALMPs.

Biewen, Fitzenberger, Osikominu, and Paul (2007) examine the effectiveness of public-sponsored training programs in Germany, emphasizing the need to consider program heterogeneity when evaluating the impacts of ALMPs. The study reveals that the effectiveness of these programs varies based on factors such as individual characteristics and local labor market conditions and underscores the importance of tailoring ALMPs to the specific needs and circumstances of participants. This discussion was revisited by the same authors in 2014 (Biewen et al. 2014), underlining the significance of data and methodological choices in assessing the impact of public-sponsored training programs. Sianesi (2004) conducted a detailed evaluation of the Swedish System of ALMPs in the 1990s. The results of the study demonstrated program heterogeneity, emphasizing that different programs within the system yielded varying outcomes. Burger et al. (2022) conducted a comprehensive impact evaluation of ALMPs in Slovenia, yielding valuable insights into their effectiveness within a specific regional context. Their results indicated that the programs had a positive impact on the employment and income of program participants.

Ibarrarán and Rosas Shady (2009) delved into the evaluation of job training programs in Latin America, providing evidence from the Inter-American Development Bank (IDB) funded operations. Their findings illuminated the effectiveness of ALMPs in the Latin American context, adding to the international discourse on the impact of these programs. In contrast to the evidence for developed countries, the results suggest that employment effects range from modest to meaningful – increasing the employment rate by about 0 to 5 percentage points. On his part, Martin (2015) presents the stylised facts on how OECD countries have responded to the Great Recession in terms of ramping up their spending on ALMPs, concluding that that some countries have played lip service to activation principles or failed to implement them effectively, leading to disappointing outcomes.

Finally, Caliendo, Mahlstedt, and Mitnik (2017) explored the relevance of usually unobserved variables in evaluating labor market policies. Their research underscores the methodological challenges researchers face when assessing ALMPs and the importance of accounting for unobserved factors.

3. Institutional setting and data

3.1 The program Training with Hiring Commitment

The program examined in this paper aims to offer specialized and appropriate training for the new job opportunities created in the Basque Country. Through these training initiatives, the objective is to promote the labor integration or reintegration of unemployed individuals by facilitating their placement into job positions that are needed in the productive system.

Entities that can benefit from these aids are those companies that have a workplace in Euskadi or training entities, whether they are public or private. In order to access this subsidy, the requesting entities must have the necessary personal and material resources to carry out the training action or have a commitment to have these resources at the time when such action begins. Additionally, for those entities that have applied in previous calls, there is a requirement to hire at least 35% of participants trained in actions of this program. The regulations also include a series of conditions that companies and requesting entities must meet. Firstly, it is necessary to ascertain the insufficiency of registered unemployed individuals with profiles suitable for meeting demand in the labor market. Secondly, the companies and organizations requesting must commit to hiring at least 50% of the individuals who have completed the courses. These hires can be full-time or part-time with a minimum of half-day work, and they must have a minimum duration of six months for full-time employment and one year for part-time contracts. Finally, it is important to note that these hires should be made in workplaces located within Euskadi and within a maximum period of 3 months after the completion of the training program.

Regarding the intended recipients of the training, this program primarily targets unemployed individuals who are registered as job seekers in the Basque PES. In fact, the 2018 call for applications was restricted to those who were unemployed, although starting from 2019, employed individuals can also participate in training courses as long as they do not exceed 30% of total participants. However, when selecting students for these training courses, there is a priority order that gives preference to unemployed individuals. Furthermore, within the group of unemployed people, certain groups are prioritized by regulations such as individuals over 45 years old; young people up to 30 years old; long-term unemployed people; disabled persons; and beneficiaries of guaranteed minimum income.

Finally, in order to determine the amount of the subsidy, the specialty of the training action (see Appendix I), the number of hours, and the number of participants in the training course are taken into account. The maximum amount is capped at 8 euros per participant and hour.

3.2 Data and descriptive statistics

3.2.1 Databases

The analysis presented in this chapter relies on several databases constructed from administrative records. The administrative nature of the data provides the analysis with a high degree of richness and detail, both regarding the characteristics of individuals participating in the program and subsidized jobs within the program framework. Below is a brief description of each database used in the analysis.

- A set of databases from the Training with Hiring Commitment program. These databases collect information on program participants, training activities carried out, and the companies and organizations that provided such training. This information is available for calls made in 2018, 2019, and 2020. The participant database provides information on all individuals who have participated in training activities regardless of whether they completed the course or not. It also includes those who applied but did not participate or remained on the waiting list. For each participant, it is possible to know which training activity they took part in, their enrollment date, as well as a series of sociodemographic variables such as gender, educational level or nationality. The database of training actions also includes detailed information about the family and specialization to which each course belongs, the number of students and hours, the start and end dates of the action, and information about the center that provides the training. In addition, it also provides information on the amount of subsidy received for each action and the municipality where it takes place. Finally, by using this database of companies one can identify those individuals who have signed an employment contract after completing their training, as well as determine whether it is a permanent or temporary position along with its start and end dates.

- Job Seekers database in the Basque PES. This database gathers information about all individuals registered as job seekers at the end of each month in the Basque PES. The individualized information provided by the database includes a set of sociodemographic variables, beyond those available in the participant registry, such as nationality (distinguishing between EU foreigners and others), number of languages spoken, and degree of disability. Additionally, the job seeker registry also contains relevant information about each individual's employment status, duration in that particular situation, and professional experience in specific occupations. Finally, it provides information regarding each registered person's perception of RGI. The use of this database within our analysis serves a dual purpose. On one hand, the higher level of detail in sociodemographic variables allows for complementing the information from the participant database and thus creating a more comprehensive profile. On the other hand, with such a large number of individuals included in this registry, it is possible to search for those who have not participated in the program but are most suitable to form the necessary control group for impact assessment.

- Registry of employment histories from the Social Security. This database includes information on the work histories of all individuals of interest for analysis between May 5, 2018 and May 1, 2023. Extracting this information from the records of the Social Security allows for a comprehensive analysis of the work history for these individuals, as it is possible to determine their employment status on a specific day, as well as the number of days in that status between two specified dates. Furthermore, in cases where there are episodes of employment, the database provides information about contribution rates, type contracts, contribution groups, and contracting companies or entities.

Finally, it is important to note that conducting this type of analysis utilizing multiple databases from different records requires the precise identification of each individual of interest.

3.2.2 Descriptive Statistics: Participants in the program

Table 9 provides descriptive information about the sociodemographic characteristics of those who have participated in the training courses between 2018 and 2020. The majority of participants in the training program are women, comprising approximately 56% to 58%. The age distribution indicates a significant representation from the middle-aged group, constituting nearly half of all participants. Following this group is the cohort of individuals below 30 years old, making up around 31% to 35%, depending on the recruitment cycle. Conversely, participants over the age of 45 comprise a smaller portion (approximately one-fourth to one-fifth) despite being considered a priority demographic for this program. Regarding nationality, roughly 90% of those recruited are citizens of the country. Among foreigner individuals, slightly more than 10% come from countries outside the EU. The findings indicate that a considerable portion of the participants have a lower level of education. However, it is noteworthy that the proportion of individuals with higher educational backgrounds, including vocational training (around 21%) and university studies (averaging 18%), is comparable to those with an equal or lower level of education up to compulsory secondary. Furthermore, when examining participant demographics further, it becomes apparent that the majority possess prior work experience and approximately one in ten are recipients of minimum income. In comparison to the population of job seekers registered in the Basque PES (column 4 in Table 9), it is possible to observe that, among the participants in the ALMP, there is an overrepresentation of certain groups, such as women and young individuals under 30 years old.

Table 9. Descriptive statistics, program participants 2018-2020

	2018	2019	2020	Basque PES Job Seekers
Gender				
- Male	44%	43 %	42%	46%
- Female	56%	57%	58%	54%
Age group				
- <30	31%	31%	35%	19%
- 30-44	45%	46%	45%	33%
- >45	24%	23%	20%	48%
Nationality				
- National	86%	91%	88%	79%
- Foreigner UE	2%	1%	1%	2%
- Foreigner not UE	12%	8%	11%	19%
Educational attainment				
- Secondary or less	36%	39%	40%	62%
- FP Media o equivalente	7%	10%	8%	9%
- Bachillerato	16%	12%	15%	7%
- FP Superior o equivalente	22%	22%	20%	10%
- University	20%	18%	17%	13%
Labour experience				
- Yes	88%	89%	87%	15%
- No	12%	11%	13%	85%

Minimum income				
- No	91%	89%	90%	78%
- Yes	9%	11%	10%	22%

Note: For this comparison, the job seeker population for November is taken as a reference, since it is a month relatively unaffected by seasonality. The choice of November is also explained by the high number of courses starting in the last quarter of the year.

4. Methodology and identification strategy

4.1 Evaluation approach

The causal evaluation of a policy requires the use of counterfactual techniques that allow for isolating and quantifying the cause-effect association implied by that intervention or treatment. To address this task, various counterfactual methodologies aim to tackle the so-called "fundamental problem of causal inference" by applying conceptual framework of "potential outcomes" proposed by Rubin (1974). The underlying idea behind this statistical tradition is that the impact of a policy is based on comparing two states of the world, one of which is unobservable or counterfactual. To illustrate this concept, we proceed to describe it in conceptual terms through the Training with Hiring Commitment program.

To address this "fundamental problem of causal inference", counterfactual techniques rely on the construction of control groups that allow us to approximate the outcome for individuals who are not treated in the unobservable state. Counterfactual techniques typically involve constructing control groups with similar characteristics to program beneficiaries but who do not participate in the program. The underlying idea is that, by having similar characteristics, the control group provides a good approximation of the counterfactual situation for participants in the absence of treatment.

When the treatment is randomly assigned and applied in an experimental setting, the impact of the measure can be estimated by comparing the outcomes of beneficiaries and non-beneficiaries of the policy. However, when participation in the program is voluntary and access criteria are established, evaluating impact must be done through non-experimental methodologies to address issues of selection bias. There are a wide range of methodologies within causal inference that can tackle this task. For this particular program, an approach based on Difference-in-Differences methodology (DiD) is adopted.

The DiD approach provides some advantages in terms of interpretability and methodological advantages. This methodology relies on comparing changes in employability experienced after program participation by the treatment group (beneficiaries) with changes over the same period for a control group that did not participate. To make this comparison valid, DiD techniques assume that the treatment and control groups would follow a similar trajectory in the absence of intervention (i.e., parallel trends assumption). Under this assumption, labor trajectories of the treatment and control groups are compared before and after program participation to observe how beneficiaries have performed compared to non-beneficiaries.

4.2 Implementation of the Differences-in-Differences technique

4.2.1 Constructing the control groups

In this initial phase of the empirical analysis, the aim is to identify a set of comparable individuals who have not participated in the program. This allows to contrast their career trajectories with those of the beneficiaries and thus infer the impact of the intervention. The final outcome of the evaluation heavily relies on the election of the control group. Therefore, the analysis employs two complementary approaches for this purpose. Firstly, we utilize matching techniques using the Basque PES's Job Seekers database. Secondly, we use a list of individuals pre-enrolled in some training courses or placed on waiting lists as a control group. Both approaches are further detailed below.

Approach #1: Use of matching algorithms to identify controls. The initial approach relies on the utilization of matching algorithms to search for untreated units (controls) that are as similar as possible to each beneficiary. In overall terms, these techniques aim to achieve a balanced distribution between beneficiaries and the control group based on observed variables. The fundamental idea behind this approach is that by conducting matching based on specific sociodemographic characteristics, the control group will comply with the key assumption of parallel trends. Consequently, we can more accurately compare the employment outcomes of beneficiaries with those of controls and attribute any observed changes directly to the program itself.

In the specific context of the "Training with Hiring Commitment" program, we employ a "near-matching" technique on the Basque PES Job Seekers database. We have made this methodological decision for two important reasons. Firstly, the job seekers database consists of a large number of monthly observations, which would make it difficult and computationally costly to include all registered unemployed individuals as control group members. Secondly, noticeable differences in observable characteristics between treatment groups and the job seekers base are observed, as displayed in Table 9. Specifically, there is an overrepresentation of certain groups, such as women and young individuals under 30 years old, compared to the total set of job seekers. These differences imply that both groups likely exhibit very different labor trajectories, which invalidates the assumption of parallel trends. Thus, employing matching techniques allows us to mitigate these biases and obtain more accurate estimates of the program's effect.

Therefore, as a preliminary step to estimation, a matching process is carried out in order to identify, for each beneficiary who has participated in any of the actions included in the Training with Job Commitment program, a set of controls that resemble them in terms of observable variables. This process to identify the most suitable controls is done through several steps. Firstly, for each treated individual, the search scope for candidates is restricted to the month and year when the training episode begins. Within this pool of potential controls, the next filter applied includes only those individuals who are registered as unemployed and have an active job-seeking status. The next step in the matching process involves determining which variables will be used to exactly match individuals from both groups. In this case, the chosen set of variables includes gender, age, education level, nationality, receiving or not the RGI (minimum income), number of disabilities, being a long-term job seeker or not, previous month's employment status and residing region.

From this point on, in cases where there is more than one control for a treated individual, there are several possibilities to refine the matching between both groups. Given the importance of the previous employment history in the analysis presented in this report, the matching process aims to select a control with a more similar recent history to each person in the treatment group. In this step of the matching process, we use nearest neighbor technique for better alignment of individuals.

Once the matching process described in the previous section has been conducted, each treated individual for whom there is at least one control is matched on a 1:1 ratio with identical individuals on the variables that were used for exact matching. This results in an identical distribution between the treatment and control groups.

The results of the matching process can be seen in Table 10 which indicates that the differences in observable characteristics between the treatment and control groups are no longer significant. In regard to the ALMP being analyzed in this chapter, out of the 1,135 participants who meet the criteria for conducting the matching process, a match is found for 989 individuals (87.1%). The fact that a match is found for most treated individuals, considering that an exact matching process was applied to a large number of variables, ensures that both treatment actions have very similar composition as before performing the matching process.

Table 10. Differences in means after the matching process

	Treatment (N=989)	Control (N=989)	P-value
Gender			
- Male	41.7 %	41.7 %	1.000
- Female	58.3 %	58.3 %	1.000
Age group			
- <30	30.8 %	30.8 %	1.000
- 30-44	45.9 %	45.9 %	1.000
- >45	23.3 %	23.3 %	1.000
Nationality			
- National	91.3 %	91.3 %	1.000
- Foreigner UE	1.1 %	1.1 %	1.000
- Foreigner not UE	7.6 %	7.6 %	1.000
Educational attainment			
- Secondary or less	39.9 %	39.9 %	1.000
- FP Media o equivalente	7.7 %	7.7 %	1.000
- Bachillerato	13.1 %	13.1 %	1.000

- FP Superior o equivalente	21.3 %	21.3 %	1.000
- University	17.9 %	17.9 %	1.000
Labour experience			
- Yes	90.4 %	90.4 %	1.000
- No	9.6 %	9.6 %	1.000
Minimum income			
- No	90.5 %	90.5 %	1.000
- Yes	9.5 %	9.5 %	1.000

Note: The table shows the distributions of the treatment and control groups according to a number of socio-demographic variables. The last column reflects whether the differences between the two groups are significant, where a value of 0 reflects a highly unequal difference and a value of 1 reflects a statistically not significant difference.

Approach #2: Using pre-registered and waitlisted individuals as a natural control group. In addition to the matched control group, this report utilizes a second control group to further strengthen the results. The individuals in this second control group are those who either pre-registered for some of the educational activities or remained on the course's waitlist (737 individuals), while the treatment group consists of all individuals who successfully completed a course (1185 individuals). This approach has dual advantages; i) it eliminates the need for matching process, simplifying the analytical procedure, and ii) it captures to some extent participants' intentionality to engage in an educational training that treated program beneficiaries also take part in. This second aspect is particularly important as it mitigates to some extent the issue of unobservable differences in characteristics (in this case, proactivity to participate in a training course) that may exist between the treatment and control groups, following the methodology explained in previous sections.

Regarding the differences in observable characteristics, Table 11 shows that even without performing matching between both groups, the means differences are not significant for most of the characteristics, except for nationality and higher educational levels. In conclusion, it can be inferred that with this approach, both groups are reasonably similar in terms of observable characteristics.

Table 11. Differences in means between alternative treatment and control groups

	Treatment (N=1185)	Control (N=737)	P-value
Gender			
- Male	43.1 %	41.4 %	0.454
- Female	56.9 %	58.6 %	0.454
Age group			
- <30	32.1 %	28.6 %	0.112
- 30-44	45.1 %	46 %	0.716
- >45	22.8 %	25.4 %	0.195
Nationality			
- National	87.5 %	72.1 %	0.000***
- Foreigner UE	1.5 %	2.3 %	0.213
- Foreigner not UE	11 %	25.5 %	0.000***
Educational attainment			
- Secondary or less	38 %	38.7 %	0.760
- FP Media o equivalente	8.1 %	8.3 %	0.866
- Bachillerato	14.1 %	13.5 %	0.715
- FP Superior o equivalente	21.3 %	16.5 %	0.012**
- University	17.7 %	22.9 %	0.006***
Labour experience			
- Yes	87.9 %	86.3 %	0.295
- No	12.1 %	13.7 %	0.295
Minimum income			
- No	93.8 %	91.6 %	0.060*
- Yes	6.2 %	8.4 %	0.060*

Note: The table shows the distributions of the treatment and control groups according to a number of socio-demographic variables. The last column reflects whether the differences between the two groups are significant, where a value of 0 reflects a highly unequal difference and a value of 1 reflects a statistically unlikely difference.

4.2.2 Estimating the impact of the program using Differences-in-Differences

The decision to use DiD approaches is motivated by their ability to control for immutable and unobservable differences between treatment and control groups compared to other counterfactual techniques. It should be noted that the validity of DID models relies on several assumptions, including a critical assumption known as parallel trends. This assumption requires that, in absence of treatment (in this case, training program), differences between treatment and control groups remain constant over time. The violation of this assumption leads to the introduction of biases in estimating the causal effect.

Among the different specific approaches to DiD analysis, this report follows the approach proposed by Callaway and Sant'Anna (2021), which provides a unified framework for measuring average treatment effects on the treated in DiD configurations with multiple time periods and variation in timing of treatment. One advantage of this approach is that it allows for measuring the impact on different groups receiving treatment based on when they receive it. Given the nature of this policy, there is a great heterogeneity of start dates for training actions, which mark the moment when an individual begins to receive treatment. To address this issue and establish a relative start date (denoted as t0) for each person, all individuals participating in the program are treated in the first period of time after course or contract completion (t+1). In practical terms, this reduces the number of groups to two: those who received treatment and those who did not receive any treatment at any time.

The use of DiD techniques requires comparing the labor trajectories before and after the program between treatment and control groups. In this report, the pre-period extends to the 6 months prior to the start of the training course (t0). Specifically, six time periods are considered (t-1, t-2, ..., t-6), with t-1 being the month before the start date of the course. On the other hand, following subsidy is extended for 12 months after completion of treatment (course or contract). This dynamic nature of analysis also allows us to examine if there are variations in treatment effects throughout year one post-treatment.

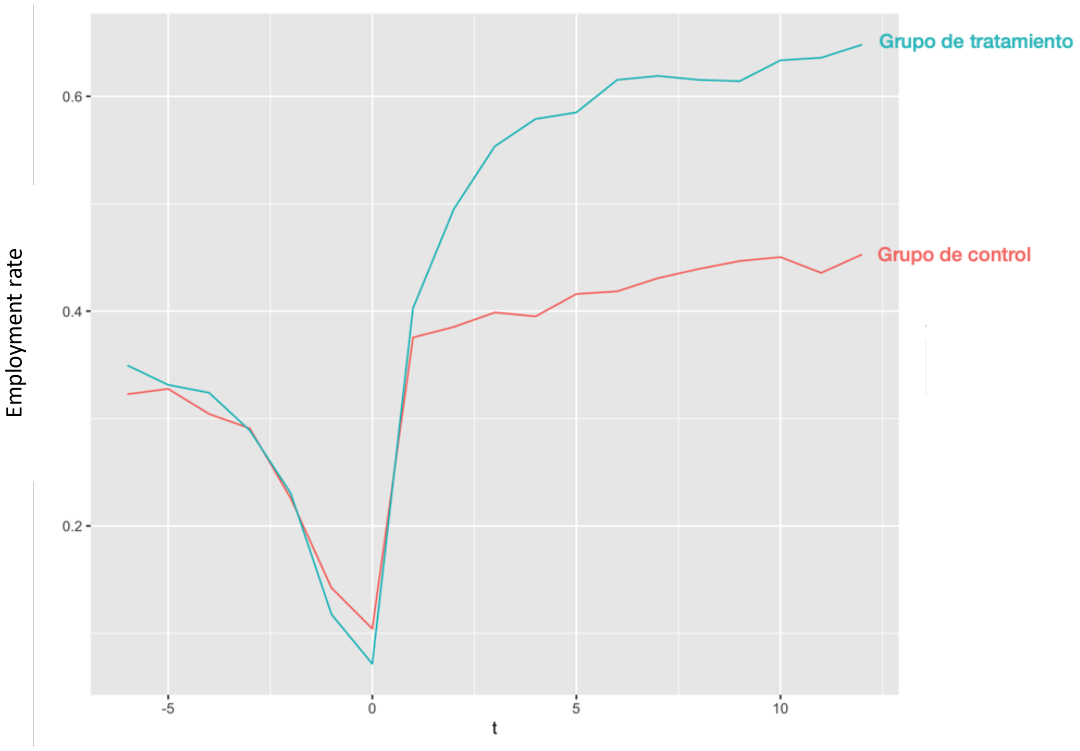
Finally, it is necessary to mention that the impact analysis of the program on employability is based on two variables. The first variable focuses on the probability of being in employment, taking a value of 1 if the individual is employed and 0 if they are not. The second variable focuses on the number of days worked by each individual between two consecutive time periods, for example, between t+1 and t+2. In this way, we aim to obtain a comprehensive overview of the relationship with employment for each group during the post-treatment period.

5. Results

5.1 Impact on participants' employability

Figure 12 illustrates the employment rate, defined as the percentage of individuals in an employed situation, for both the treatment group (blue) and the control group (red). Prior to participating in the training program, the employment rates for both groups are very similar, hovering around 35% six months before the start of the training intervention. Furthermore, it can also be observed from Figure 1 that both employment rates evolve in a highly comparable manner throughout pre-treatment period; however, notable differences emerge between these two groups once treatment has been administered. The difference between the two groups remains relatively stable until t+12, at which point the treatment group has an employment rate of 64.8% compared to 45.3% for the control group.

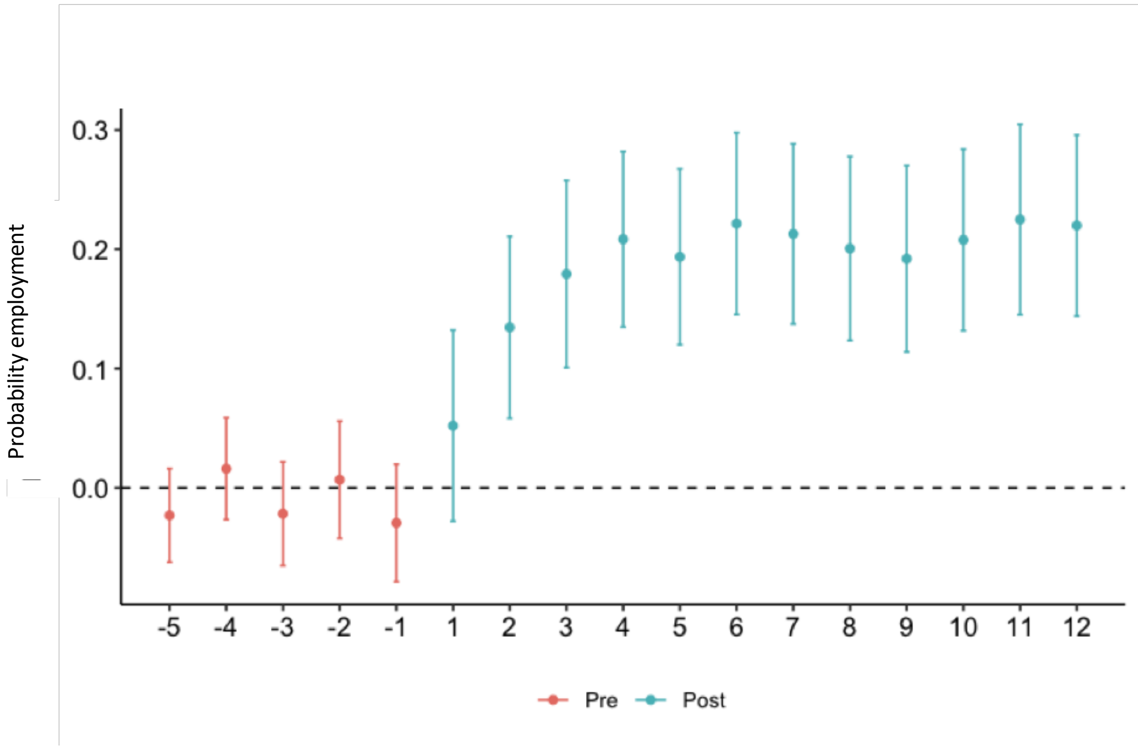
Figure 12. Employment rates in the pre- and post-treatment and control groups



Note: The lines represent the proportion of people in the treatment (blue) and control (red) group who are employed in each time period.

The observed differences in employment levels between the two groups provide a better understanding of the results shown in Figure 13, which illustrates the impact of the training program on participants' employability. As can be seen, the estimated values before treatment (i.e., training) occur, represented by red color in the figure, are very small and not significant since the bars representing confidence intervals cross the line at 0. This suggests that the parallel trends assumption is fulfilled as shown in Figure 12 where employment rates for both groups are similar both in levels and trend. In the post-treatment period, however, the estimated values (in blue) indicate a significant positive impact on the probability of being employed for individuals who have participated in the program. Beyond the direction of the effect, it is worth noting that the size of the impact varies over time. Thus, in the immediately following month after completing treatment, there is a small (5 percentage points) and statistically nonsignificant impact. However, as the months pass, the size of the impact increases to reach 20 percentage points in t+4. From that moment onwards, a relatively stable impact is found at these levels.

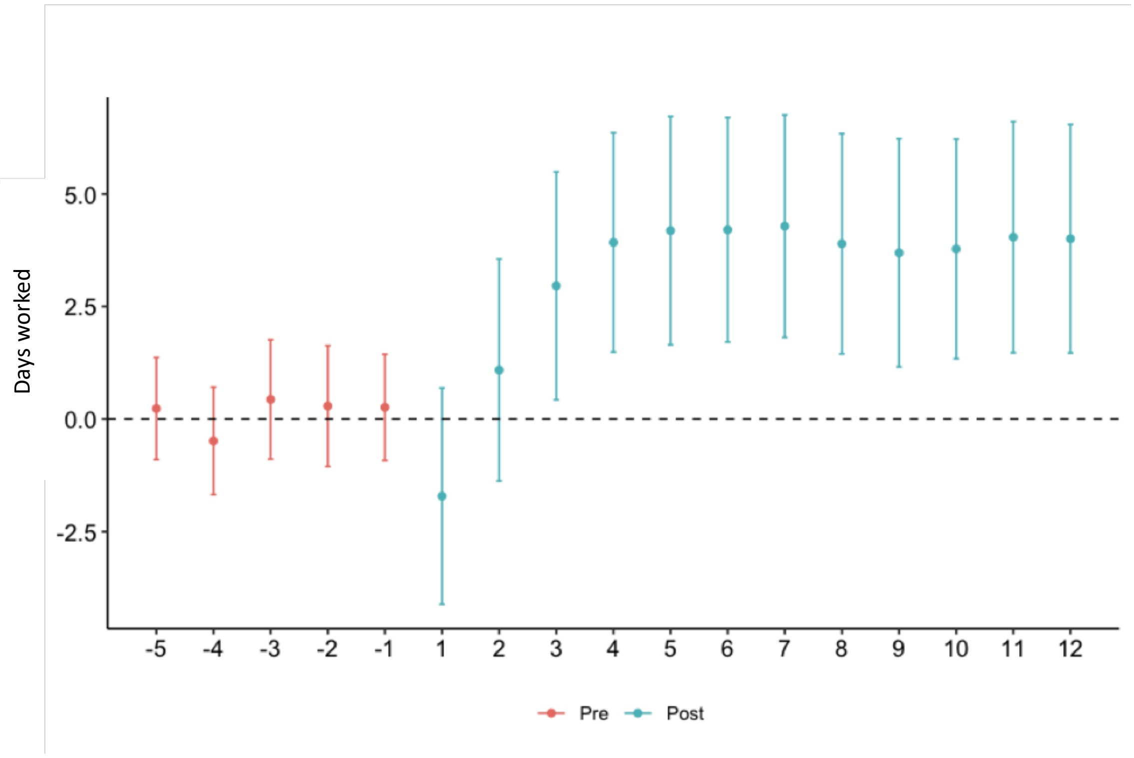
Figure 13. Impact on probability of being employed



Note: Dots represent estimated treatment effects on the probability of being employed. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

In a similar manner, Figure 14 illustrates that engaging in the Training Program with Job Commitment has a positive effect on the number of days worked. Although this impact is not statistically significant in the immediate term, it gradually intensifies until reaching an approximate impact of 4 working days per month. It is noteworthy that individuals in the control group typically work around 13 days at t+12; hence program participation leads to a substantial increase of approximately 30% in monthly working days.

Figure 14. Impact on number of days worked



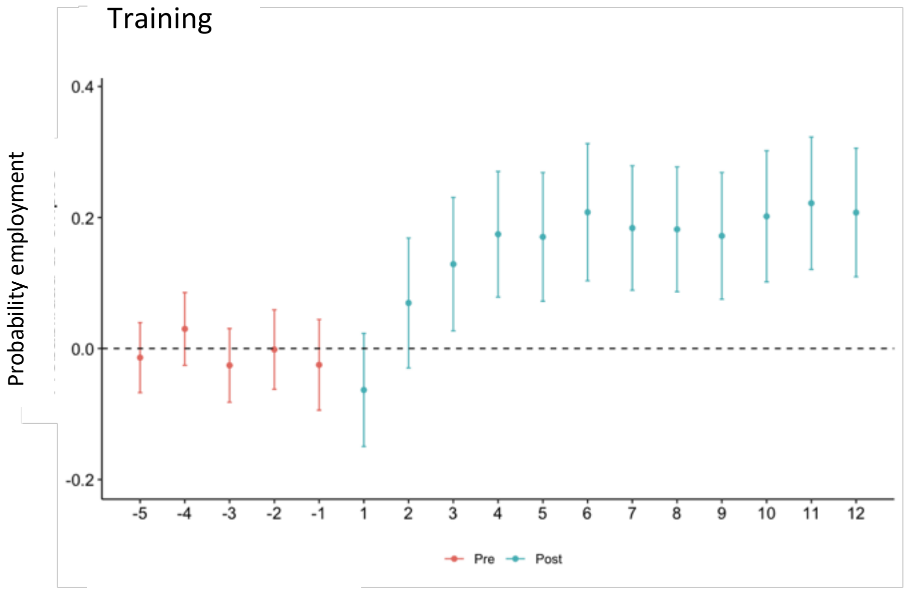
Note: Dots represent estimated treatment effects on the number of days worked. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

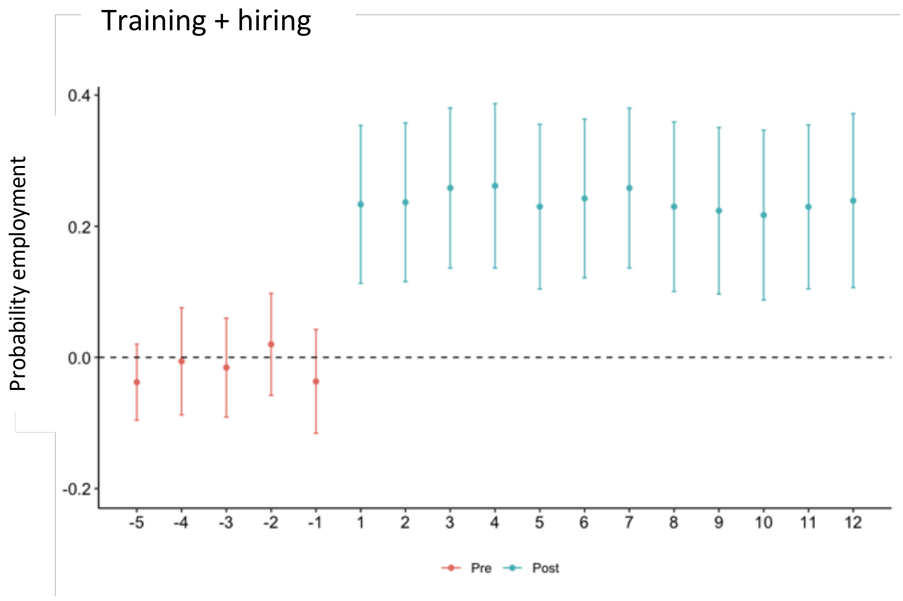
5.2 Heterogeneity analysis

After providing an overview of the overall impact on employability for individuals who underwent treatment, this analysis examines the potential presence of varied effects based on different types of treatment. Specifically, we explore whether there is a comparable impact on employability for participants who received training alone versus those who also got hires afterwards.

Figure 15 clearly illustrates that there is a notable difference in the impact between the two types of treatment, especially in the short term. When comparing training alone to the combined approach of hiring and training, it becomes evident that the latter shows a positive effect starting from t+1, with an increase of approximately 20 percentage points (p.p.), which remains relatively consistent during the year following contract completion. This stability in employment could be attributed to PAE beneficiaries either continuing their work at the same company where they committed or easily finding employment at similar companies. Regardless, both cases demonstrate a stabilization effect over time at around 20 p.p., slightly lower for treatments involving only training.

Figure 15. Impact on probability of being employed, by type of treatment

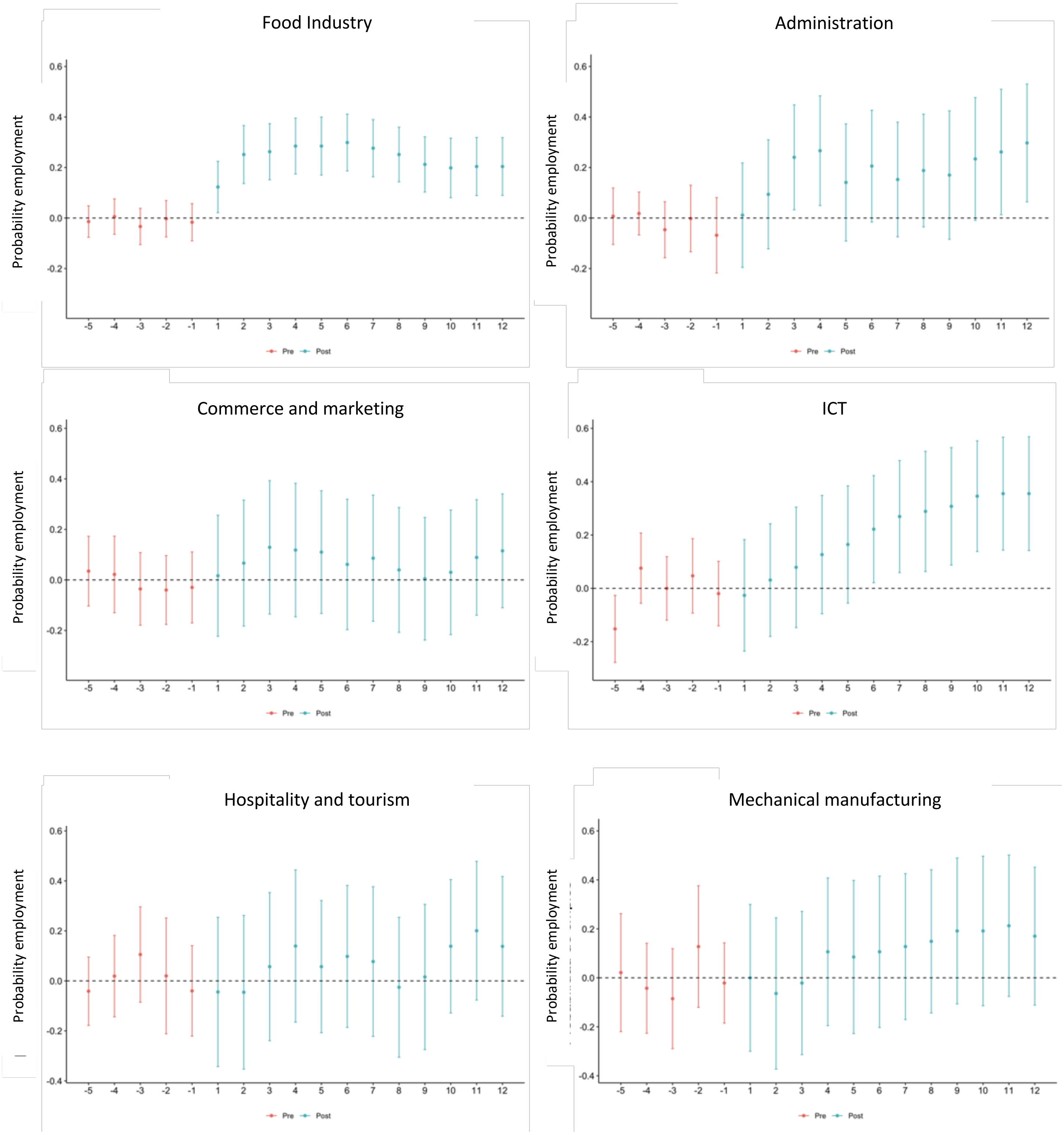




Note: Dots represent estimated treatment effects on the probability of employment. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

Another important factor to consider when studying the heterogeneity of impact is the type of course. In this regard, the results indicate that there are significant differences among different families of training courses with a larger number of participants. Thus, Food Industry courses have a highly notable positive impact on employability, achieving an increase in employment probability by approximately 28 percentage points at t+6 (see Figure 16). This denotes a high demand for jobs in this field; although it should be noted that the effect decreases to 20 percentage points at t+12. On the other hand, courses in Administration and Management as well as those in ICT show similar impacts on employment probability, not finding consistent short-term effects but having an impact above 20 percentage points at t+12 for both cases. Other families do not show a significant effect at any point in the post-treatment period, such as courses in Commerce and Marketing and those in Hospitality and Tourism. Finally, Mechanical Manufacturing courses demonstrate a positive effect although it is not statistically significant.

Figure 16. Impact on the probability of being employed, by type of course



Note: Dots represent estimated treatment effects on the probability of employment. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

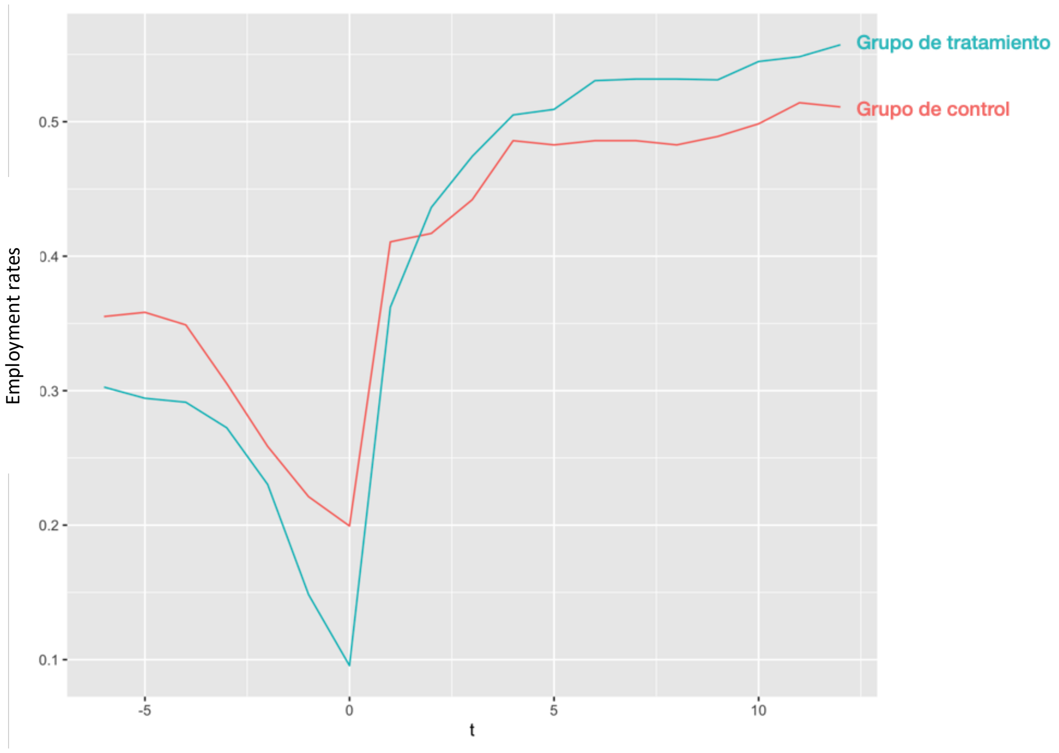
In concluding the analysis of heterogeneity, it is important to highlight that certain groups experience divergent effects. The estimation results (refer to Appendix C for detailed results) indicate a greater short-term impact for women (16.9 percentage points compared to 6.9 percentage points for men), although this difference levels out in the medium term. Additionally, there is a notable effect on employability among individuals aged 55 and older (47.3 percentage points at t+12), which can be attributed to significant challenges faced by this group when searching for employment - an issue shared by others within their age bracket. Lastly, individuals with lower or moderate educational attainment levels benefit from a positive impact while those with higher education qualifications do not show any noticeable effect.

5.3 Results using the natural control group

To conclude the results section, the aggregated impact results on employment outcomes for individuals treated using the alternate treatment and control groups proposed in Section 4 are presented.

The employment rates of both groups highlight the initial differences in the analysis conducted with the matched sample. As shown in Figure 17, the employment rate for the treated individuals is lower in this case, both during the pre-treatment period (below 30% at t+6) and during the post-treatment period (50% at t+6). At the same time, it can be observed that the employment rate for control group is higher in immediately preceding periods to start of training and more prominently at t0 (20% vs. 10% compared to matching results). In the post-program period, both control groups exhibit similar employment rates close to 45% at t+12.

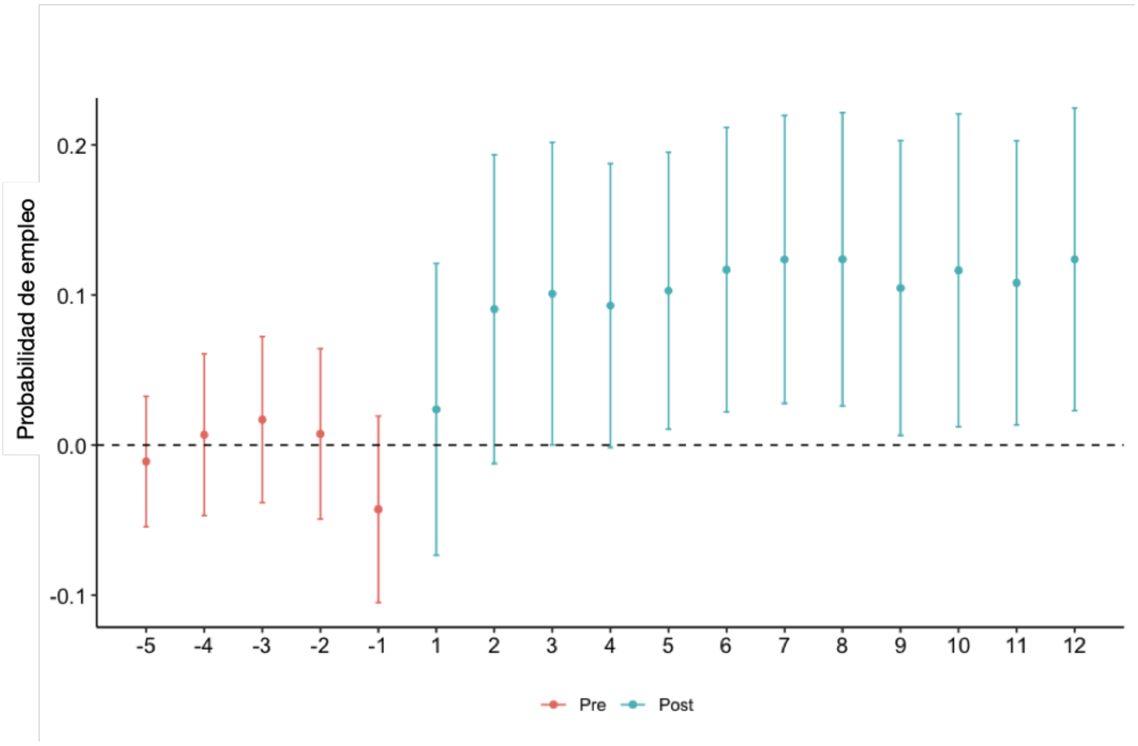
Figure 17. Employment rates in the pre and post periods of the alternative treatment and control groups



Note: The lines represent the proportion of people in the treatment (blue) and control (red) group who are employed in each time period.

Regarding employability impact, Figure 18 demonstrates a comparable trend to that of Figure 9 but with a substantial difference in the magnitude of the impact. Consequently, the short-term impact is not statistically significant; however, from t+5 onwards, a noteworthy effect emerges and remains stable at approximately 10 percentage points until t+12. Therefore, according to this alternate specification, the effect on employment probability continues to be positive albeit markedly diminished.

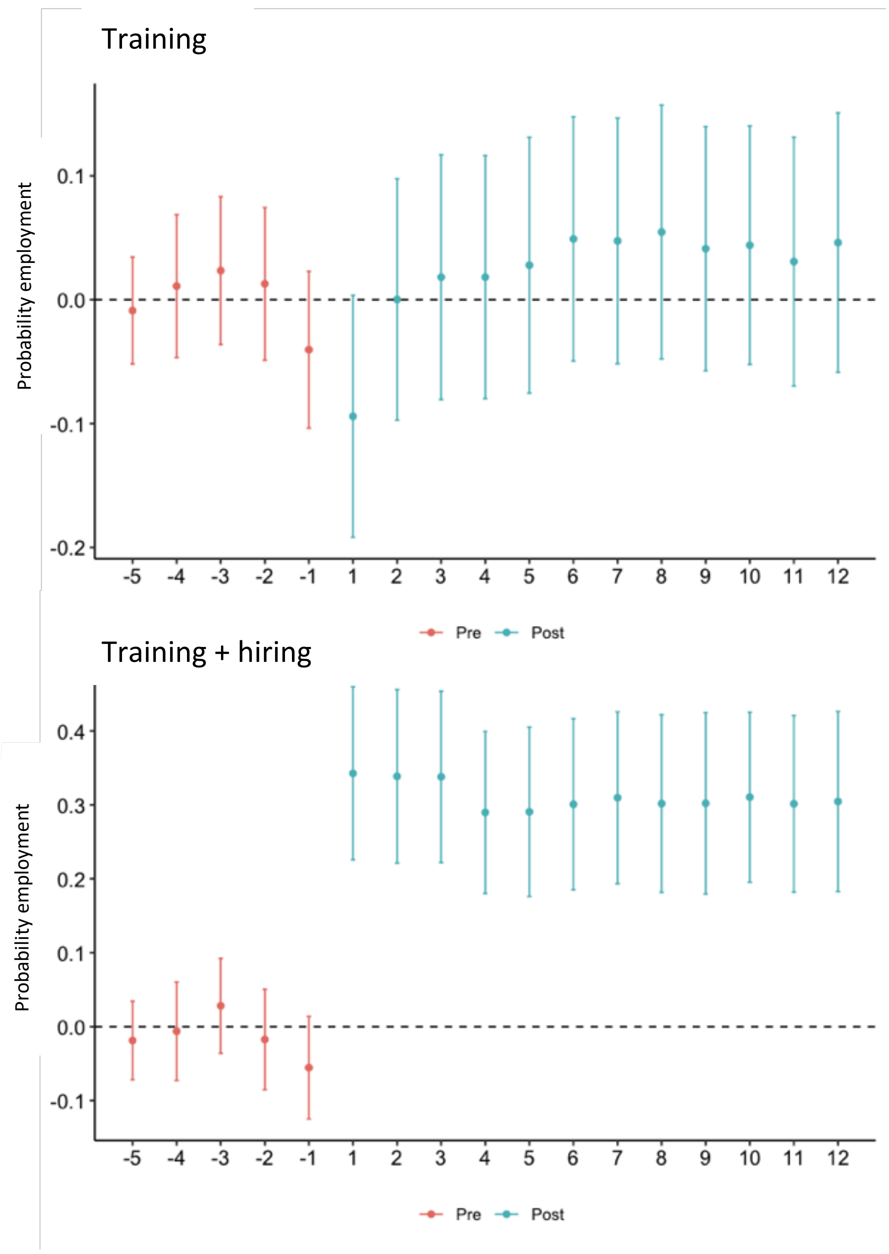
Figure 18. Impact on probability of being employed, alternative treatment and control groups



Note: Dots represent estimated treatment effects on the probability of being employed. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

A potential explanation for this difference in the intensity of impact may lie primarily on the low employment impact for individuals who only participate in one course and not in the commitment to hiring. This is evident from Figure 19, which illustrates a null impact in the case of this type of treatment. This result could be explained by the individuals in the control group who showed proactivity to participate in training actions, possibly having taken alternative training courses outside of the analyzed program. This may have reduced differences in gained competencies with those from the treatment group. In cases where individuals sign a contract under the program framework, there is an even greater impact compared to previous results (27 percentage points at t+12).

Figure 19. Impact on the probability of employment, by type of treatment



Note: Dots represent estimated treatment effects on the probability of employment. The vertical lines represent the 95% confidence interval. If this vertical line crosses the dashed horizontal line, the effect is not significant.

6. Conclusions

ALMPs play a crucial role in addressing the challenges and inequalities that exist within the labor market. These policies aim to create opportunities for unemployed and disabled individuals to actively participate in the workforce, contributing to economic growth and societal well-being. By providing support in the form of skills training, reemployment assistance, and job placement services, ALMPs can help individuals overcome barriers to employment.

It is essential for policymakers to conduct evaluations of active labor market policies in order to assess their effectiveness. By carefully measuring the impact of these interventions, decision-makers can determine whether they are successfully achieving their intended objectives and if they justify the allocation of resources. Additionally, understanding the impact of these programs can inform future program design and implementation, enabling the refinement and enhancement of ALMPs.

This study assesses the effects of an active labor market policy called Training with Hiring Commitment, implemented in the Basque Country. The program aims to bridge the skills gap in the job market by offering training courses tailored to meet companies' specific skill requirements. In order for companies to participate, they must demonstrate a shortage of individuals with these skills and agree to hire a minimum number of participants. This policy represents a novel approach that integrates training programs for unemployed individuals with direct pathways into employment within participating organizations.

The findings of the employability impact assessment reveal a favorable effect of active labor market policies, both in terms of increasing the likelihood of employment and the number of days worked. While this effect is initially modest, it gradually intensifies over time, resulting in an increase by 20 percentage points in the probability of securing employment and 13 additional days worked at t+12. It should be noted that the limited short-term impact stems from individuals solely participating in training courses. Conversely, those who enter into contractual agreements experience an immediate positive outcome that remains consistent throughout all analyzed periods. Moreover, it appears that different types of courses have varying impacts on employability: actions within sectors such as Food Industry or IT and ICT demonstrate significantly positive effects, whereas no statistically significant impact is observed for Commerce and Marketing or Hospitality and Tourism courses.

On one hand, the research conducted by various groups reveals a relatively consistent impact across different demographic categories, although there are some notable cases such as women (particularly in the short term) and individuals who are over 55 years old. Additionally, there is a positive effect for individuals with medium to low educational levels while those with higher education do not experience any significant impact. Furthermore, when considering an alternative control group, the results indicate a slightly less intense but still favorable effect (10 percentage points at t+12), following a similar trend as observed in the main analysis. The variance in intensity of this impact can be attributed to the lack of employability improvement among participants engaged in non-committal courses alone.

Concluding remarks

The main goal of this work is to highlight the importance of evaluating public policies for a better understanding of their impact on society and, ultimately, improving the decision-making process by policymakers. For this purpose, each chapter empirically analyzes a public policy of particular relevance in the actual socioeconomic context.

Chapter 1 examines the impact of MIS on poverty reduction in two significant ways. Firstly, it emphasizes the importance of taking a multidimensional approach when evaluating public transfers for poverty alleviation and provides insights on assessing poverty-fighting interventions. Secondly, it incorporates a temporal perspective by considering various stages of the economic cycle, offering empirical evidence on the potential of MIS interventions in promoting socioeconomic resilience and mitigating the adverse effects of economic downturns. The case study chosen for this purpose is the Basque MIS (RGI), which is the longest running regional MIS in Spain. Using the EPDS survey designed to measure poverty in the Basque Country, a hypothetical scenario is constructed where households do not receive MIS. The main objective of this analysis is to contrast whether using an equivalence scale or a specific indicator affects measuring the impact of poverty-reduction transfers.

The analysis reveals three key findings. First, the RGI has a significant impact on reducing poverty in all aspects in the Basque Country, leading to a 40% decrease in incidence during economic downturns. Additionally, it reduces intensity between 60% and 70% and severity between 74% and 80%. Second, caution is necessary when selecting tools to measure poverty, especially regarding equivalence scales. Nonetheless, differences in poverty reduction are smaller than measurement variances, highlighting the critical role of tool selection for both policy assessment and design. Lastly, despite the existence of the MIS, poverty in the Basque Country rose after the 2008 recession; extreme poverty intensified twofold and nearly tripled in severity from 2008 to 2018.

These results indicate that an MIS has broader benefits for improving the welfare of low-income individuals beyond simply reducing incidence; it enables them to improve their situation despite remaining below extreme poverty levels. Therefore, this study underscores the importance of considering multiple dimensions when analyzing poverty-reduction transfers. With ongoing reforms for a Basque MIS and introduction of national MIS, further research will be essential to assess possibilities for reversing this situation in future.

Chapter 2 provides evidence of the employment effects of a large raise in the minimum wage in Spain. Using administrative Social Security records from the 2019 CSWL, the analysis compares employment transitions between a group of workers who earned less than the newly-established MW prior to the reform and workers who earned more than the minimum wage threshold. Although this approach is not new, this research extends previous literature of MW in Spain in several ways. First, it incorporates two relevant worker types traditionally excluded from the analysis: part-time workers and employees working less than the entire month. Second, it explores the impact of MW raises both on the extensive margin (probability of employment loss) and the intensive margin (probability of work intensity reduction). Third, the analysis considers the impact on monthly transitions, therefore assessing the effect of the reform both in the short and in the medium term.

The results indicate that the reform initially had no impact on employment within the first five months after the increase. However, a notable negative effect becomes evident thereafter, primarily through extensive margin. In the 12 months following the reform, there is a significant negative effect with an increase of 1.92 percentage points in probability to transition into unemployment. With consideration of the nominal increase of 22.3% in MW, this result indicates an elasticity of -0.086 between employment loss and MW. Similar to work intensity through reduction in working hours, there is also a relatively small but growing effect over time; quantitatively speaking, this effect size is significantly smaller than observed for job loss (0.84 percentage points).
Separate analyses are conducted to assess the presence of heterogeneous effects based on gender, age, and prior work intensity of workers. The findings reveal limited differences between men and women in the probability of losing employment. There is a more immediate adjustment for men in terms of work intensity, while women experience a larger impact over the medium term. Significant heterogeneity by workers' age is also observed - younger workers are more affected in terms of work intensity whereas older workers suffer a larger employment loss effect. Moreover, full-time and part-time workers exhibit large differences with full-time employees expecting a larger work intensity adjustment.

Chapter 3 assesses the effects of an active labor market policy called *Training with Hiring Commitment*, implemented in the Basque Country. The program aims to bridge the skills gap in the job market by providing training courses designed to meet specific skill requirements of companies. To participate, companies must show a shortage of individuals with these skills and commit to hiring a minimum number of participants. This policy introduces an innovative approach that combines training programs for unemployed individuals with direct pathways into employment within participating organizations.

The employability impact assessment findings show that the active labor market policy had a positive effect on increasing the likelihood of employment and the number of days worked. This effect starts off modest but gradually becomes stronger over time, resulting in a 20% increase in the probability of securing employment and an additional 13 days worked at t+12. It's important to note that individuals participating solely in training courses see limited short-term impact. In contrast, those entering into contractual agreements experience immediate positive outcomes that remain consistent throughout all analyzed periods. Additionally, different types of courses have varying impacts on employability: actions within sectors such as Food Industry or IT and ICT demonstrate significantly positive effects, whereas no statistically significant impact is observed for Commerce and Marketing or Hospitality and Tourism courses.

The heterogeneity analysis shows a relatively consistent impact across different demographic categories, with some notable cases such as women (especially in the short term) and individuals aged over 55. In addition, there is a positive effect for individuals with medium to low educational levels, while those with higher education do not experience any significant impact. Furthermore, when conducting the analysis using an alternative control group for robustness check issues, the results indicate a slightly less intense but still favorable effect (10 percentage points at t+12), following a similar trend as observed in the main analysis. The variation in intensity of this impact can be attributed to the lack of improvement in employability among participants engaged in non-committal courses alone.

The examined policies in this thesis exhibit significant differences, but from the assessment process itself, some common conclusions can be drawn. Firstly, as detailed in all sections there is no such thing as a single impact. This means that the effect of a policy may vary widely among different affected groups or at different times. Secondly, methodological factors play a crucial role and can influence the outcome of the analysis. As discussed in Chapter 1, the impact on poverty reduction varies greatly depending on the dimension analyzed or equivalence scale utilized. Furthermore, as demonstrated in Chapter 3, it is essential to recognize that evaluation outcomes depend not only on the treatment group but also significantly on the control group. Last but not least, it is crucial to acknowledge that even with the most advanced methodological tools and rigorous evaluation strategies, a thorough analysis of such nature cannot be conducted without access to comprehensive and high-quality databases. Therefore, in addition to emphasizing the need for assessing policies, it's also important to focus on the rigorous processes involved in systematically structuring information within top-tier databases.

In my concluding thoughts, I want to underscore a particular aspect of public policy evaluation. The analysis presented in this thesis are predominantly quantitative in nature. This means that the questions asked and the conclusions drawn mainly focus on quantifying the impact of the policy in terms of its direction (positive, negative, or neutral) and its magnitude. While this method enables precise and rigorous assessment of how an intervention affects a specific outcome, measuring the effect is just one facet of public policy evaluation. To obtain a more complete picture and better evaluate an intervention, it would be advisable to complement the quantitative evidence with qualitative evidence which may provide valuable information that is sometimes not reflected in databases. It is important to keep in mind that behind the necessary data for these analyses, there are individuals and households who frequently encounter challenging situations. As stated in the introductory paragraph of this thesis, the main goal of public policies is to address social issues. This goal should always be borne in mind when undertaking an assessment of this kind of policies.

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Appendix A

Additional material to Chapter 1

Results using the OECD equivalence scale

The OECD original equivalence scale was developed in the 1980s to account for differences in the needs and expenses of households of different sizes and compositions. It assigns weights to each additional person in a household, with the first adult assigned a weight of 1, each additional adult assigned a weight of 0.7, and each child under 14 assigned a weight of 0.5. According to Hagenaars et al. (1981), previous research had found that this scale had a high family size elasticity, which led to these authors to the proposal of a new equivalence scale: the OECD-modified scale. Nevertheless, the addition of this scale to the analysis enhances the robustness of the results obtained in the main analysis.

Table A1 presents the reduction in extreme poverty incidence, intensity, and severity using the OECD equivalence scale for the years 2008, 2012, 2014, 2016, 2018, and 2020, as well as poverty reduction in each dimension. In summary, the overall results regarding the three dimensions of poverty align with those obtained using the OECD-modified and square root equivalence scales. However, there are two notable aspects worth highlighting. Firstly, the poverty incidence indicator exhibits greater variability compared to the other scales, showing closer alignment with the square root scale in 2008 and 2020, and with the OECD-modified scale in the remaining years. Secondly, the impact of the RGI on poverty reduction is less reliable when considering the poverty incidence dimension, as compared to the intensity and severity dimensions.

For instance, in 2020, the range of poverty reduction results for the poverty incidence dimension varies significantly, ranging from -21.9% to 36.9% depending on the scale used. In contrast, the range for poverty intensity is -53.7% to -58.9%, while for poverty severity, it is -73.4% to -74%. This suggests that when evaluating the effectiveness of anti-poverty policies, it is crucial to consider dimensions beyond the simple headcount ratio, regardless of the equivalence scale employed in the analysis.

In conclusion, these findings emphasize the need to consider multiple dimensions of poverty when assessing the impact of policy interventions, rather than relying solely on the poverty incidence measure. This holds true regardless of the specific equivalence scale used in the analysis.

Table A1. Extreme poverty incidence, intensity and severity reduction using the OECD equivalence scale

	Incidence			Intensity			Severity		
Year	Post-MIS	Pre-MIS	Reduction	Post-MIS	Pre-MIS	Reduction	Post-MIS	Pre-MIS	Reduction
2008	4.14%	4.98%	-17.0%	0.77	1.71	-54.97%	0.31	1.11	-74.8%
2012	3.43%	5.76%	-40.5%	0.99	3.21	-69.16%	0.47	2.45	-82.6%
2014	4.70%	8.04%	-41.6%	1.51	4.45	-66.07%	0.74	3.31	-79.3%
2016	5.42%	8.33%	-34.9%	1.42	3.96	-64.14%	0.64	2.79	-78.7%
2018	4.77%	7.59%	-37.1%	1.59	4.17	-61.87%	0.83	3.1	-73.9%
2020	6.58%	8.51%	-22,7%	1.96	4.23	-53.66%	0.94	3.01	-73.9%

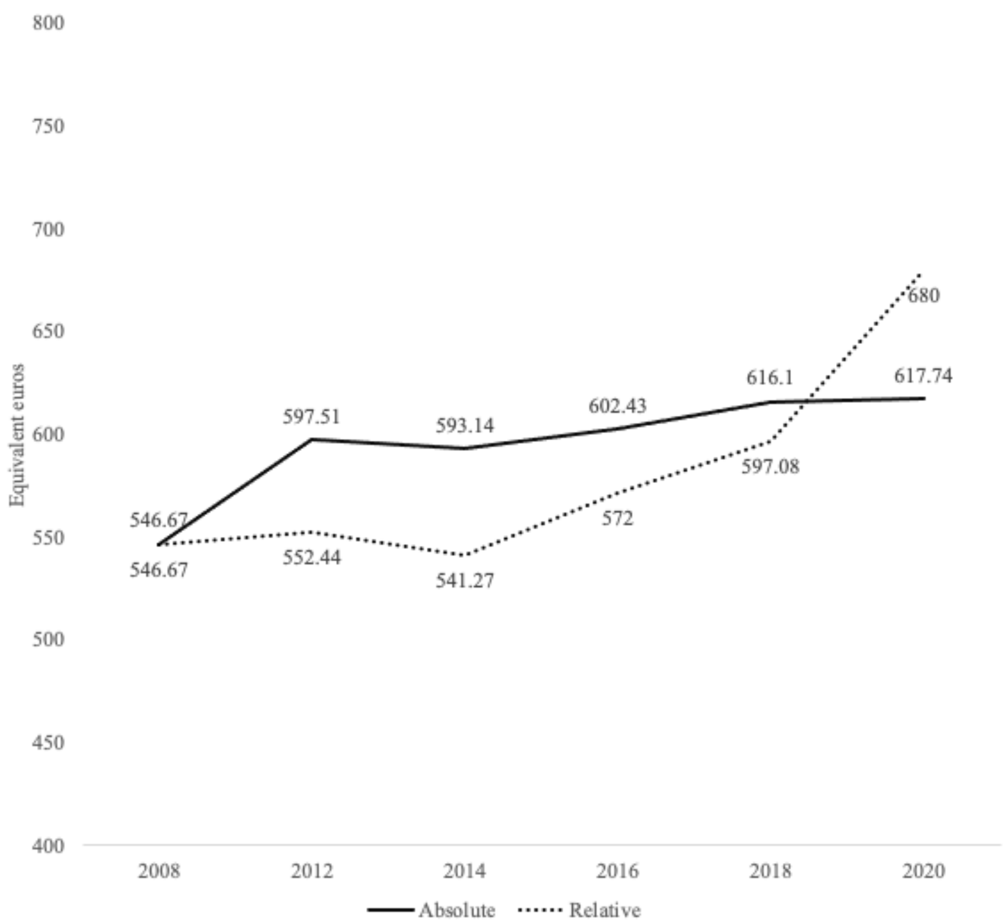
Source: Own calculations using EPDS

Anchored poverty line

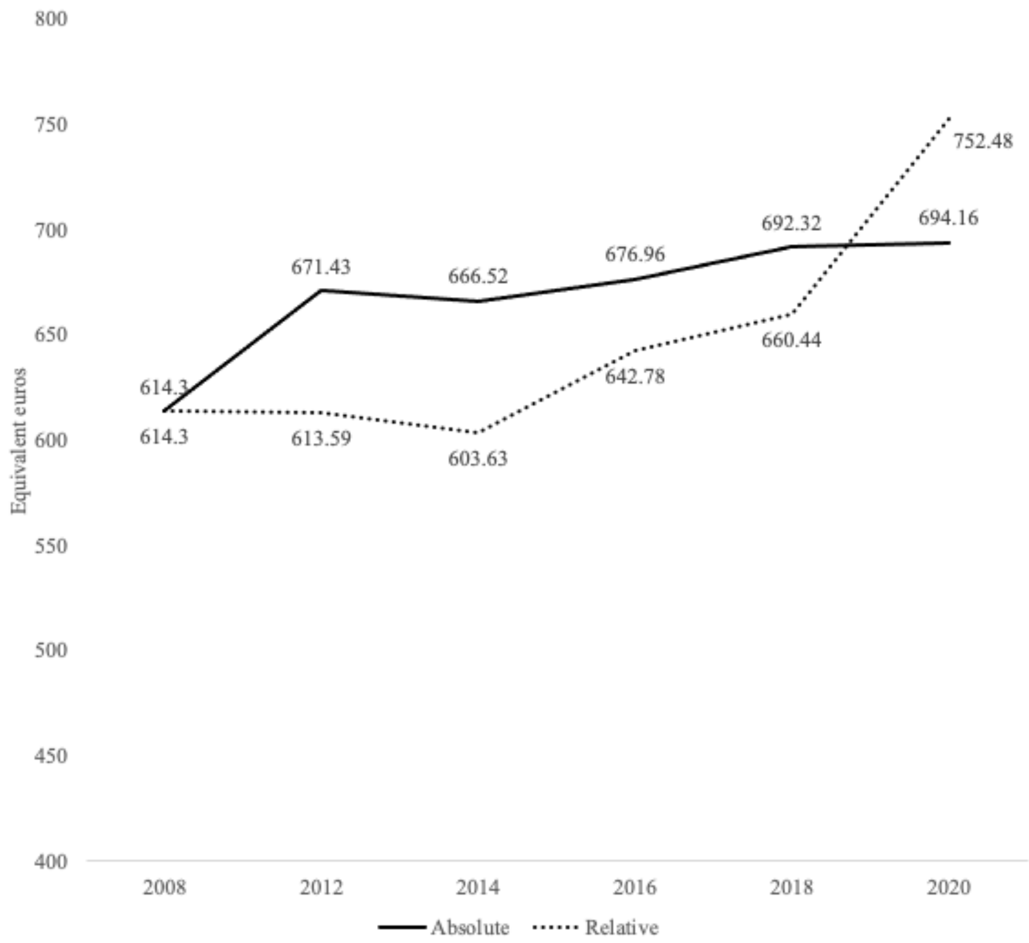
This appendix shows the results of the poverty analysis by changing the relative poverty line detailed in the body of the document to an absolute poverty line, defined as anchored poverty line. Specifically, the relative poverty line for 2008 is set as the anchored reference, adjusting for changes in inflation. Figure A1 shows the relative and absolute poverty lines using the OECD-modified scale (Panel A) and the square root (Panel B). As can be seen, the anchored poverty line is higher than the relative poverty line in all years except 2020 and, as with the relative poverty line, using the square root yields a higher poverty line. Since the RGI is updated year to year, this part of the analysis does not focus on poverty reduction by the Basque MIS.

Figure A1. Relative and anchored poverty lines (2008-2020)

A) OECD-modified



B) Square root



Source: own calculations using EPDS

The fact that the anchored poverty line is higher than the relative line for each year after 2008 (except in 2020) has direct implications for poverty measurement. As Tables A2, A3 and A4 show, the results obtained with this approach are worse in terms of higher incidence, intensity and severity of poverty, except for 2020. As a result, all indications point to the use of relative poverty lines underestimating the worsening conditions that poor households face during recessionary periods. This is due to the generalized decline in household income during an economic downturn, which implies that the poverty line falls as well. The results obtained suggest that, at least in the case of the lower income population, the living standards of 2008 have not yet recovered.

Table A2. Poverty incidence, relative vs anchored poverty line

	Relative poverty line		Anchored poverty line	
Year	OECD-modified	Square root	OECD-modified	Square root
2008	3.43%	4.15%	3.43%	4.15%
2012	3.59%	4.05%	4.84%	5.90%
2014	4.93%	5.60%	6.73%	7.90%
2016	4.91%	5.34%	6.53%	6.83%
2018	5.12%	5.91%	5.91%	6.83%
2020	6.09%	6.75%	4.91%	5.34%

Source: Own calculations using EPDS

Table A3. Extreme poverty intensity, relative vs anchored poverty line

	Relative poverty line		Anchored poverty line	
Year	OECD-modified	Square root	OECD-modified	Square root
2008	0.74	0.73	0.74	0.73
2012	0.94	0.95	1.16	1.27
2014	1.41	1.42	1.79	1.92
2016	1.23	1.28	1.47	1.53
2018	1.47	1.49	1.58	1.65
2020	1.71	1.76	1.32	1.39

Source: Own calculations using EPDS

Table A4. Extreme poverty intensity, relative vs anchored poverty line

	Relative poverty line		Anchored poverty line	
Year	OECD-modified	Square root	OECD-modified	Square root
2008	0.29	0.26	0.74	0.73
2012	0.43	0.42	0.51	0.53
2014	0.68	0.69	0.84	0.86
2016	0.59	0.6	0.66	0.68
2018	0.77	0.76	0.7	0.72
2020	0.78	0.77	0.63	0.63

Source: Own calculations using EPDS

Appendix B

Additional material to Chapter 2

Estimation results

Table B1. Impact of MW on employment

	t+1	t+2	t+3	t+4	t+5	t+6	t+7	t+8	t+9	t+10	t+11	t+12
Employment with labor intensity reduction	0.000878	0.00134	0.00427***	0.00372***	0.00272**	0.00322**	0.00371**	0.00199	0.00203	0.00299*	0.00687***	0.00842***
	(0.000639)	(0.000852)	(0.00108)	(0.00124)	(0.00135)	(0.00145)	(0.00154)	(0.00163)	(0.00167)	(0.00172)	(0.00179)	(0.00183)
Unemployment	-0.000211	-0.00185	-0.00108	-2.34e-05	0.00609**	0.00933***	0.0120***	0.0133***	0.0118***	0.0230***	0.0180***	0.0192***
	(0.00176)	(0.00232)	(0.00257)	(0.00267)	(0.00273)	(0.00280)	(0.00287)	(0.00299)	(0.00312)	(0.00308)	(0.00307)	(0.00306)
Observations	*61,193*	*61,042*	*60,941*	*60,852*	*60,743*	*60,631*	*60,439*	*60,226*	*60,122*	*60,174*	*60,276*	*60,183*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B2. Impact of MW on employment (men)

	t+1	t+2	t+3	t+4	t+5	t+6	t+7	t+8	t+9	t+10	t+11	t+12
Employment with labor intensity reduction	0.00147*	0.00344***	0.00632***	0.00611***	0.00403**	0.00378*	0.00516**	0.00236	0.00269	0.00548**	0.00642***	0.00755***
	(0.000879)	(0.00119)	(0.00153)	(0.00176)	(0.00191)	(0.00206)	(0.00213)	(0.00231)	(0.00237)	(0.00237)	(0.00248)	(0.00252)
Unemployment	0.00429	-0.00265	-0.00289	0.00184	0.00564	0.0117***	0.0182***	0.0172***	0.0134***	0.0264***	0.0225***	0.0245***
	(0.00286)	(0.00376)	(0.00410)	(0.00424)	(0.00433)	(0.00442)	(0.00450)	(0.00458)	(0.00481)	(0.00478)	(0.00477)	(0.00473)
Observations	*25,713*	*25,618*	*25,556*	*25,510*	*25,441*	*25,389*	*25,284*	*25,159*	*25,096*	*25,161*	*25,224*	*25,193*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B3. Impact of MW on employment (women)

	t+1	t+2	t+3	t+4	t+5	t+6	t+7	t+8	t+9	t+10	t+11	t+12
Employment with labor intensity reduction	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***	0.00904***
	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)	(0.00258)
Unemployment	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***	0.0153***
	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)	(0.00399)
Observations	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*	*34,990*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B4. Impact of MW on employment (30 years old or younger)

	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*	*t+11*	*t+12*
Employment with labor intensity reduction	0.000727	0.00182	0.00948***	0.00714***	0.00537*	0.000914	0.00360	0.000526	0.00263	0.00647*	0.00926**	0.0133***
	(0.00135)	(0.00173)	(0.00218)	(0.00252)	(0.00279)	(0.00301)	(0.00317)	(0.00336)	(0.00341)	(0.00353)	(0.00367)	(0.00377)
Unemployment	-0.000810	-0.00423	-0.0107**	-0.0161***	-0.0179***	-0.00482	-0.00308	0.00124	-0.00912	0.00551	-0.000638	-0.000652
	(0.00328)	(0.00434)	(0.00486)	(0.00504)	(0.00514)	(0.00522)	(0.00536)	(0.00548)	(0.00570)	(0.00572)	(0.00575)	(0.00571)
Observations	20,540	20,510	20,470	20,447	20,417	20,385	20,323	20,261	20,223	20,230	20,251	20,217

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B5. Impact of MW on employment (more than 30 years old)

	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*	*t+11*	*t+12*
Employment with labor intensity reduction	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***	0.00554***
	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)	(0.00200)
Unemployment	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***	0.0295***
	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)	(0.00358)
Observations	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*	*39,966*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B6. Impact of MW on employment (full-time)

	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*	*t+11*	*t+12*
Employment with labor intensity reduction	0.000449	0.00253***	0.00583***	0.00536***	0.00469***	0.00555***	0.00661***	0.00465**	0.00292	0.00575***	0.00947***	0.0108***
	(0.000705)	(0.000948)	(0.00118)	(0.00136)	(0.00150)	(0.00164)	(0.00175)	(0.00188)	(0.00194)	(0.00199)	(0.00204)	(0.00208)
Unemployment	-0.000717	-0.00182	-0.00409	-0.00171	0.00762**	0.0111***	0.0125***	0.0157***	0.0147***	0.0248***	0.0205***	0.0239***
	(0.00210)	(0.00275)	(0.00305)	(0.00316)	(0.00322)	(0.00329)	(0.00336)	(0.00352)	(0.00367)	(0.00359)	(0.00357)	(0.00354)
Observations	*43,495*	*43,361*	*43,287*	*43,225*	*43,156*	*43,084*	*42,947*	*42,771*	*42,675*	*42,748*	*42,878*	*42,813*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Table B7. Impact of MW on employment (part-time)

	t+1	*t+2*	*t+3*	*t+4*	*t+5*	*t+6*	*t+7*	*t+8*	*t+9*	*t+10*	*t+11*	*t+12*
Employment with labor intensity reduction	0.00192	-0.00156	0.000447	-0.000321	-0.00214	-0.00254	-0.00346	-0.00454	-0.000198	-0.00385	0.000378	0.00285
	(0.00137)	(0.00180)	(0.00238)	(0.00265)	(0.00287)	(0.00299)	(0.00312)	(0.00323)	(0.00322)	(0.00336)	(0.00362)	(0.00384)
Unemployment	0.00103	-0.00190	0.00627	0.00411	0.00232	0.00501	0.0109**	0.00757	0.00504	0.0187***	0.0121**	0.0111*
	(0.00317)	(0.00428)	(0.00472)	(0.00496)	(0.00512)	(0.00528)	(0.00546)	(0.00567)	(0.00588)	(0.00591)	(0.00593)	(0.00597)
Observations	*17,698*	*17,681*	*17,654*	*17,627*	*17,587*	*17,547*	*17,492*	*17,455*	*17,447*	*17,426*	*17,398*	*17,352*

Note: Average marginal effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Source: Own calculations using CSWL.

Alternative matching specification

The first robustness analysis entails proposing a more demanding matching in terms of individuals' work histories. In this case, in addition to the variables mentioned in the impact analysis section, a new variable is introduced to match individuals based on their work intensity prior to *t0*. To achieve this, we expand the use of the variable *coefpar*, which, as described in section 4, is employed for matching at *t0*. This approach implies that the matching considers the individual's employment status in previous periods (until *t-6*) in addition to their employment status at *t0*. Using the ranks of our *coefpar* variable, we specifically consider I whether each individual is employed or not, and ii) if employed, the work intensity of that individual.

This method makes the matched individuals more similar in terms of characteristics, particularly in the recent labor trajectory preceding the MW increase. However, the number of treated individuals for whom a match in the control group is found is reduced (20,409 vs. 31,238 in the case of the matching performed in the analysis).

Table B8. Differences in means between treatment and control groups, following the alternative matching specification

	Matching			
Group	Treatment (1)	Control (2)	Test (p-value) (3)	Treatment (pre-matching) (4)
Gender				
Men	40.8 %	40.8 %	1.000	43.4%
Women	59.2 %	59.2 %	1.000	56.6%
Age				
16-25 years	15.1 %	15.1 %	1.000	23.2%
36-34 years	29.1 %	29.1 %	1.000	28.2%
36-44 years	27.1 %	27.1 %	1.000	23.4%
45-54 years	21.9 %	21.9 %	1.000	19.0%
55 years and older	6.8 %	6.8 %	1.000	6.3%
Type of contract				
Permanent Full-Time	49.3 %	49.3 %	1.000	35.6%
Permanent Part-Time	19.2 %	19.2 %	1.000	15.9%
Temporary Full-Time	24.2 %	24.2 %	1.000	27.9%
Temporary Part-Time	6.5 %	6.5 %	1.000	11.7%
Establishment size				

Less than 10 employees	56.6 %	56.6 %	1.000	55.5%
10-49 employees	20.2 %	20.2 %	1.000	19.8%
50-249 employees	13.8 %	13.8 %	1.000	14.7%
250 or more employees	9.5 %	9.5 %	1.000	9.9%
Work intensity				
Coefpar = 1000	73.7 %	73.7 %	1.000	71.6%
Coefpar [750-1000)	9.2 %	9.2 %	1.000	8.8%
Coefpar [500-750)	12.2 %	12.2 %	1.000	12.4%
Coefpar [250-500)	3.6 %	3.6 %	1.000	5.0%
Coefpar < 250	1.3 %	1.3 %	1.000	2.2%
Observations	*20,409*	*20,409*		*35,144*

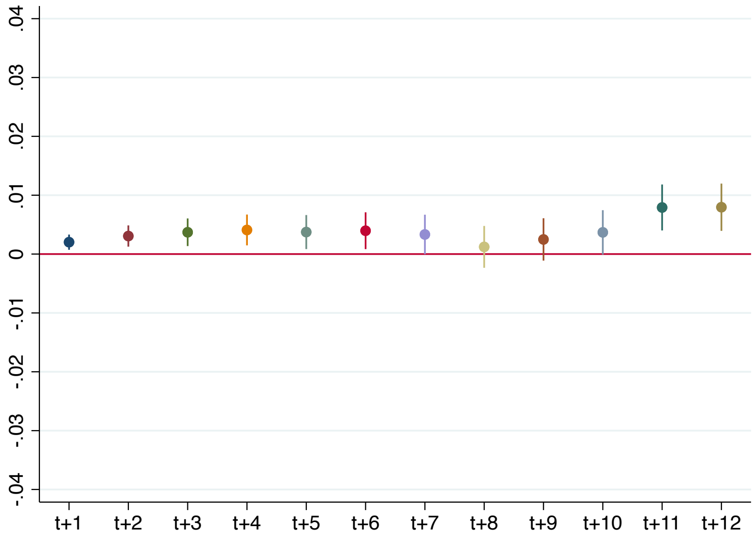
Note: Columns (1) and (2) report the means in observed characteristics for treatment and control groups the matching procedure described in Apendix II. Column (4) reports observed characteristics for the treatment group before the matching procedure.

Source: Own calculations using CSWL.

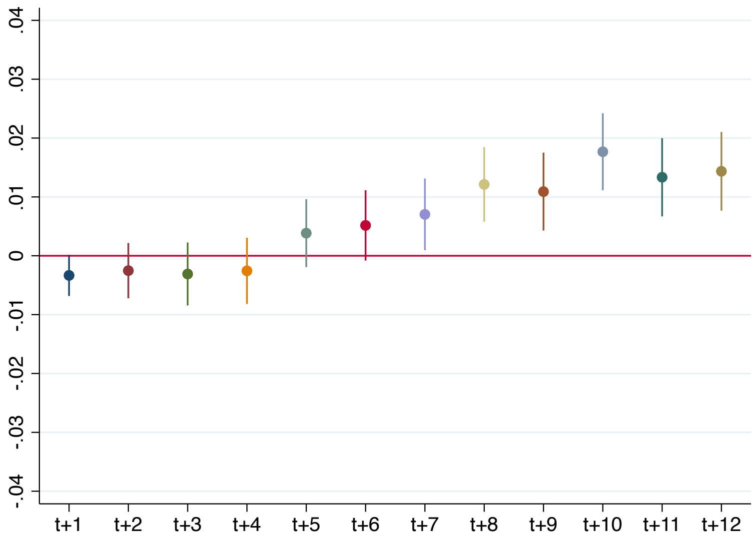
The estimates made following this matching process offer very similar results, although slightly lower, than those presented in the body of the report. First, it is observed that the effect of the SMI on employment in the short term is nil and, as of t+6, a negative impact is observed that grows over time. This impact is mainly due to the loss of employment, which in *t+12* stands at -1.44 p.p., the adjustment in hours worked being lower (-0,79 p.p.).

Figure B1. Impact of MW raise on employment, using the sample obtained in Table B8

a) Employment with work intensity reduction



b) Unemployment



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

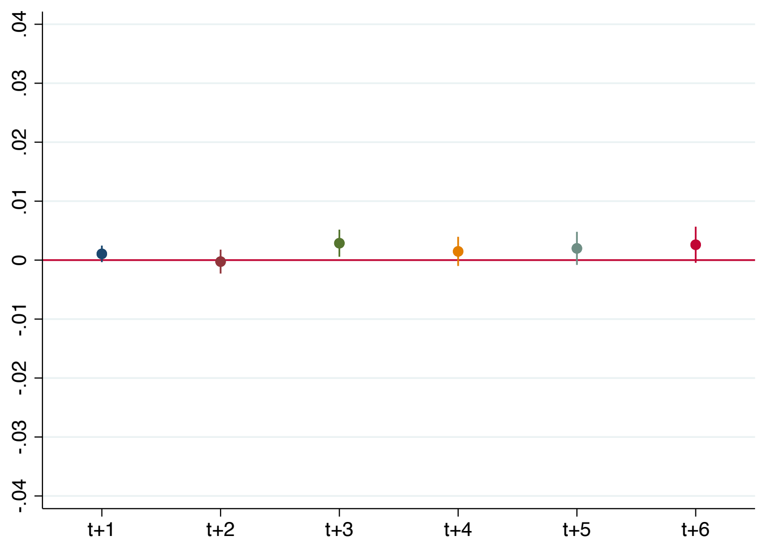
Placebo test

Finally, a placebo test was carried out in which a fictitious increase in the MW was considered. Specifically, the month of May 2018 is set as the time at which this fictitious increase would have taken place. The purpose of this test is to confirm that the results obtained in the estimates are indeed due to the SMI and not to other factors unrelated to the measure, such as differences in productivity between treated and controls. Given that this increase did not take place, and applying the same methodology and identification strategy explained in the body of the report, the expected result is that there is no impact on the probabilities of transitioning to the defined scenarios. If this were not the case, it could not be argued that the results obtained in this work are really due to the rise in the MW.

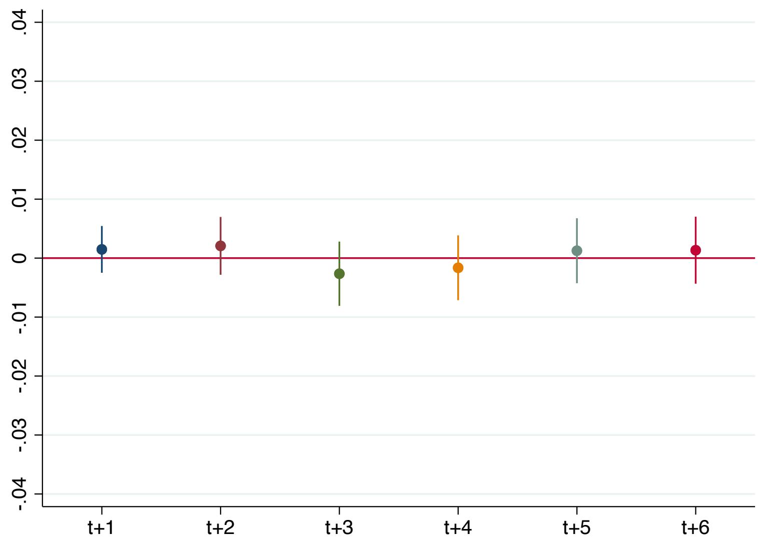
Figure A4 shows that, according to the results of this placebo test, there is no significant impact. Therefore, and at least during the months prior to the increase in the MW, there do not seem to be any significant differences between controls and treated patients.

Figure B2. Results of placebo test

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

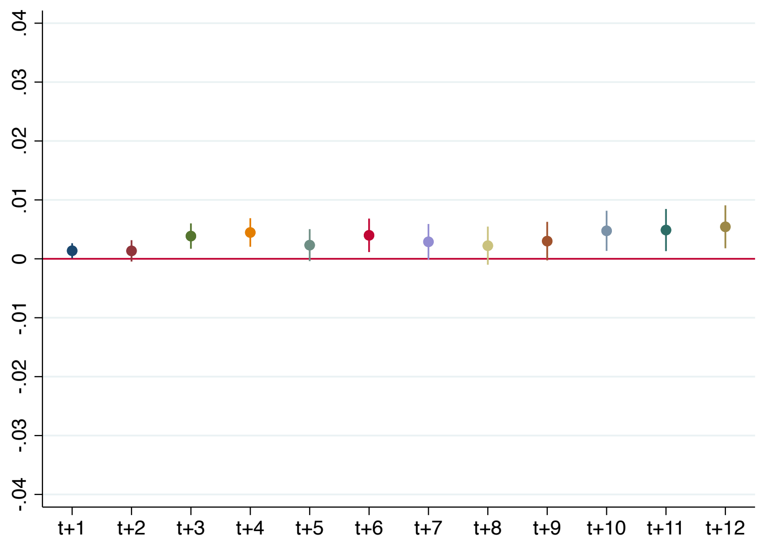
Additional sensitivity analysis (I)

The second robustness analysis consists of changing the reference day on which individuals are observed across the panel. Specifically, this dated is switched from the second Tuesday of each month to the second Saturday, keeping November 2018 as *t0*. The objective of this approach is to include individuals who work only on weekends and who, with the original approach, are excluded from the analysis. Since this is a group that, presumably, may be more affected by labor precariousness and, therefore, could be particularly affected by the increase in the SMI. The matching process applied in this robustness test is the same as that applied in the impact assessment section.

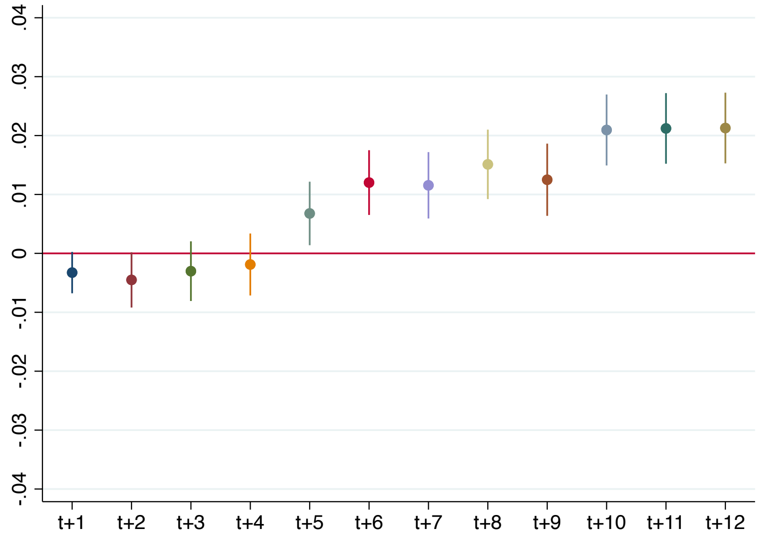
Estimates made following this approach also yield an impact of the SMI increase very similar to those presented preoviously. Again, it can be seen that, in the short term, there is no significant impact of the increase on employment. From *t+5* onwards, we begin to see an impact that grows month by month until it reaches 2.09 p.p. in the case of the probability of transitioning to non-employment. The impact on work intensity is very small.

Figure B3. Impact of MW raise on employment, setting Saturday as reference day

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors. Source: Own calculations using CSWL.

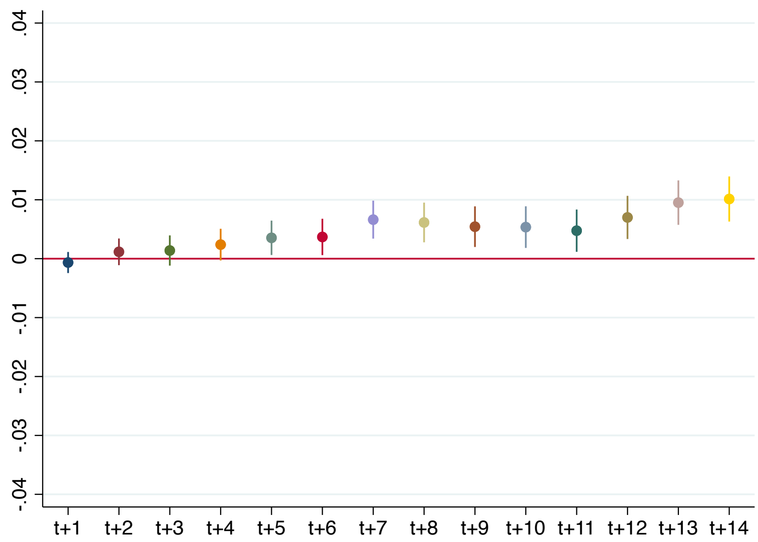
Additional sensitivity analysis (II)

In their analysis of the impact of the MW increase on employment survival, Cebrián et al. (2020) find that the MW increase has a negative impact prior to the implementation of the increase. This is because employers, knowing in advance that the MW increase is going to occur, make the decision to make labor adjustments before the MW increase takes effect. Therefore, this third robustness test consists of setting a month prior to November -September 2018- as *t0*, in order to rule out the existence of such an effect, which would imply that the estimated impact of the MW increase would be incorrect. As in the original approach, individuals are observed on the second Tuesday of each month.

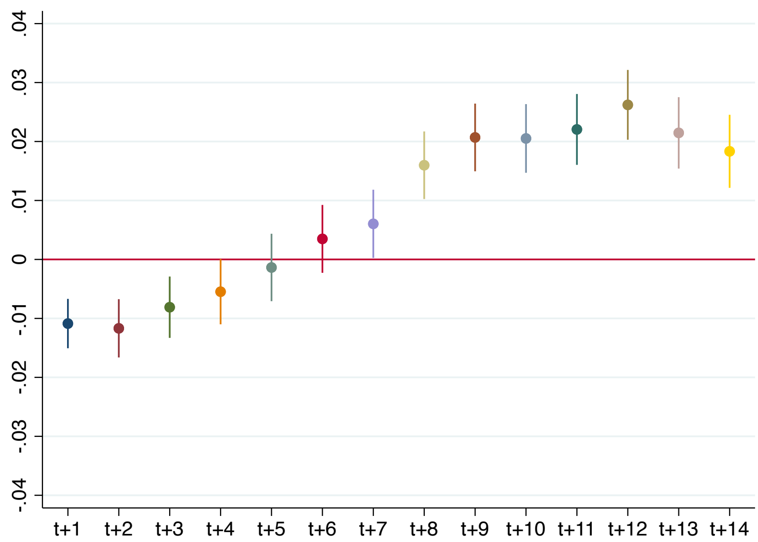
The estimation results suggest that, in the case of the MW increase analyzed, there is no negative impact before the measure takes effect -before *t+4*, with this approach-. Once the increase has taken place, the results obtained are similar to those presented in the impact analysis section of the report, since a null impact is observed in the short term, which increases over time to values close to 2 p.p. in the case of job losses and around 1 p.p. in the adjustment in hours worked.

Figure B4. Impact of MW raise on employment, setting September as *t0*

a) Employment with work intensity reduction



b) Unemployment



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors. Source: Own calculations using CSWL.

Appendix C
Estimations results from Chapter 3

Estimation results

Table C1. Estimation results, by treatment type (Probability of employment)

	Total		Course		Course + contract	
t	Effect	Standard error	Effect	Standard error	Effect	Standard error
-5	-0.0231	0.0142	-0.0138	0.0197	-0.0378	0.0233
-4	0.0160	0.0158	0.0300	0.0189	-0.0059	0.0273
-3	-0.0217	0.0154	-0.0257	0.0190	-0.0155	0.0252
-2	0.0068	0.0176	-0.0015	0.0206	0.0199	0.0286
-1	-0.0294	0.0188	-0.0249	0.0207	-0.0366	0.0275
1	0.0521	0.0269	-0.0632	0.0320	0.2334 *	0.0431
2	0.1345 *	0.0287	0.0695	0.0340	0.2366 *	0.0413
3	0.1792 *	0.0291	0.1288 *	0.0345	0.2585 *	0.0417
4	0.2084 *	0.0282	0.1745 *	0.0353	0.2617 *	0.0442
5	0.1936 *	0.0283	0.1704 *	0.0362	0.2300 *	0.0422
6	0.2215 *	0.0280	0.2081 *	0.0344	0.2425 *	0.0424
7	0.2129 *	0.0264	0.1839 *	0.0355	0.2583 *	0.0424
8	0.2006 *	0.0274	0.1820 *	0.0358	0.2299 *	0.0404
9	0.1921 *	0.0273	0.1719 *	0.0345	0.2237 *	0.0399
10	0.2078 *	0.0277	0.2018 *	0.0359	0.2171 *	0.0423
11	0.2250 *	0.0263	0.2218 *	0.0339	0.2296 *	0.0413
12	0.2199 *	0.0275	0.2076 *	0.0345	0.2391 *	0.0434
Obs	*1,648*		*1,036*		*612*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C2. Estimation results, by type of course (Probability of employment) (part 1)

	Food Industry		Administration		Commerce and marketing	
t	Effect	Standard error	Effect	Standard error	Effect	Standard error
-5	-0.0140	0.0235	0.0072	0.0386	0.0349	0.0149
-4	0.0056	0.0250	0.0179	0.0284	0.0218	0.0125
-3	-0.0335	0.0235	-0.0463	0.0388	-0.0356	0.0137
-2	-0.0028	0.0273	-0.0020	0.0421	-0.0399	0.0143
-1	-0.0168	0.0285	-0.0682	0.0521	-0.0297	0.0151
1	0.1229 *	0.0393	0.0113	0.0745	0.0165	0.0235
2	0.2514 *	0.0394	0.0938	0.0729	0.0666	0.0232
3	0.2626 *	0.0386	0.2402 *	0.0756	0.1291	0.0239
4	0.2849 *	0.0391	0.2665 *	0.0704	0.1181	0.0229
5	0.2849 *	0.0400	0.1409	0.0784	0.1101	0.0238
6	0.2989 *	0.0381	0.2055	0.0763	0.0614	0.0238
7	0.2765 *	0.0383	0.1525	0.0817	0.0861	0.0250
8	0.2514 *	0.0386	0.1880	0.0743	0.0396	0.0241
9	0.2123 *	0.0390	0.1700	0.0808	0.0047	0.0247
10	0.1983 *	0.0407	0.2339	0.0810	0.0301	0.0239
11	0.2039 *	0.0385	0.2614 *	0.0788	0.0890	0.0242
12	0.2039 *	0.0388	0.2973 *	0.0731	0.1151	0.0242
Obs	*728*		*234*		*178*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C3. Estimation results, by type of course (Probability of employment) (part 2)

	ICT		Hospitality and tourism		Mechanical manufacturing	
t	Effect	Standard error	Effect	Standard error	Effect	Standard error
-5	-0.1520 *	0.0464	-0.0412	0.0447	0.0213	0.0644
-4	0.0757	0.0424	0.0191	0.0646	-0.0426	0.0738
-3	-0.0002	0.0418	0.1054	0.0751	-0.0851	0.0752
-2	0.0470	0.0470	0.0200	0.0804	0.1277	0.0906
-1	-0.0198	0.0455	-0.0400	0.0634	-0.0213	0.0638
1	-0.0263	0.0711	-0.0442	0.1098	0.0000	0.1121
2	0.0310	0.0738	-0.0455	0.1088	-0.0638	0.1128
3	0.0788	0.0750	0.0570	0.1093	-0.0213	0.1081
4	0.1265	0.0741	0.1395	0.0996	0.1064	0.1060
5	0.1645	0.0773	0.0570	0.0939	0.0851	0.1081
6	0.2219 *	0.0745	0.0978	0.0948	0.1064	0.1047
7	0.2693 *	0.0766	0.0774	0.0965	0.1277	0.1007
8	0.2886 *	0.0780	-0.0255	0.0967	0.1489	0.0987
9	0.3075 *	0.0776	0.0157	0.1008	0.1915	0.1013
10	0.3456 *	0.0747	0.1386	0.0971	0.1915	0.1074
11	0.3552 *	0.0730	0.2007	0.0955	0.2128	0.1034
12	0.3553 *	0.0752	0.1382	0.1068	0.1702	0.1060
Obs	*218*		*98*		*94*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C4. Estimation results by gender (Probability of employment)

	Men		Women	
t	Effect	Standard error	Effect	Standard error
-5	-0.0486	0.0256	-0.0082	0.0181
-4	0.0465	0.0282	-0.0005	0.0206
-3	-0.0351	0.0275	-0.0131	0.0200
-2	0.0368	0.0311	-0.0079	0.0218
-1	-0.0148	0.0301	-0.0376	0.0222
1	0.0141	0.0454	0.0722	0.0354
2	0.0697	0.0447	0.1649 *	0.0369
3	0.1420 *	0.0426	0.2010 *	0.0361
4	0.1793 *	0.0445	0.2190 *	0.0370
5	0.1779 *	0.0458	0.1918 *	0.0339
6	0.2026 *	0.0438	0.2168 *	0.0345
7	0.1814 *	0.0472	0.2158 *	0.0335
8	0.2273 *	0.0475	0.1746 *	0.0362
9	0.2664 *	0.0465	0.1503 *	0.0346
10	0.2619 *	0.0439	0.1802 *	0.0354
11	0.2633 *	0.0453	0.2012 *	0.0365
12	0.2456 *	0.0445	0.1983 *	0.0353
Obs	*636*		*1,012*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C5. Estimation results by age group (Probability of employment)

	< 35 years old		> 55 years old	
t	Effect	Standard error	Effect	Standard error
-5	-0.0077	0.0226	-0.1250	0.0869
-4	0.0232	0.0237	0.0322	0.0590
-3	-0.0490	0.0242	-0.0322	0.0666
-2	0.0174	0.0269	-0.0322	0.0724
-1	-0.0306	0.0301	-0.0625	0.0434
1	0.0233	0.0407	0.2812	0.1173
2	0.1196 *	0.0415	0.4697 *	0.1267
3	0.2007 *	0.0426	0.4706 *	0.1119
4	0.2056 *	0.0412	0.4081 *	0.1165
5	0.1653 *	0.0412	0.4706 *	0.1087
6	0.2276 *	0.0412	0.4394 *	0.1077
7	0.1972 *	0.0394	0.4403 *	0.1202
8	0.1877 *	0.0395	0.5019 *	0.1157
9	0.2134 *	0.0396	0.4706 *	0.1162
10	0.2184 *	0.0392	0.5009 *	0.1068
11	0.2407 *	0.0407	0.4697 *	0.1090
12	0.2363 *	0.0409	0.4706 *	0.1162
Obs	*782*		*68*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C6. Estimation results by level of education (Probability of employment)

	Low/medium education		High education	
t	Effect	Standard error	Effect	Standard error
-5	-0.0568	0.0262	-0.0057	0.0186
-4	0.0042	0.0281	0.0249	0.0213
-3	0.0070	0.0258	-0.0369	0.0219
-2	0.0394	0.0306	-0.0067	0.0222
-1	-0.0276	0.0322	-0.0299	0.0229
1	-0.0027	0.0511	0.0749	0.0328
2	0.0595	0.0475	0.1624 *	0.0331
3	0.1264	0.0467	0.2031 *	0.0348
4	0.1552 *	0.0524	0.2264 *	0.0337
5	0.1038	0.0504	0.2272 *	0.0337
6	0.1434	0.0518	0.2438 *	0.0331
7	0.1010	0.0504	0.2524 *	0.0343
8	0.1130	0.0505	0.2342 *	0.0350
9	0.1034	0.0480	0.2398 *	0.0335
10	0.1568 *	0.0485	0.2368 *	0.0341
11	0.1630 *	0.0514	0.2537 *	0.0339
12	0.1844 *	0.0477	0.2288 *	0.0328
Obs	*588*		*1,060*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C7.Estimation results, by type of treatment (Days worked)

	Total		Course		Course + contract	
t	**Effect**	**Standard error**	**Effect**	**Standard error**	**Effect**	**Standard error**
-5	0.1486	0.3542	0.3614	0.4427	-0.1863	0.5270
-4	-0.4694	0.3205	-0.3379	0.4026	-0.6766	0.6077
-3	0.0234	0.3878	-0.0517	0.4689	0.1426	0.6286
-2	-0.3061	0.4083	-0.2417	0.4912	-0.4069	0.6563
-1	-0.2006	0.3803	-0.2332	0.4234	-0.1493	0.5938
1	0.2549	0.7152	-3.3467 *	0.7583	5.9194 *	1.1474
2	2.8574 *	0.7026	0.2701	0.9002	6.9253 *	1.2089
3	4.6584 *	0.7540	2.7497 *	0.8973	7.6590 *	1.1711
4	5.5681 *	0.7756	4.3999 *	0.9302	7.4020 *	1.2403
5	5.8779 *	0.7258	4.9119 *	0.8924	7.3936 *	1.2442
6	5.9772 *	0.7474	5.3702 *	0.9443	6.9269 *	1.1475
7	6.1231 *	0.7342	5.5369 *	0.9389	7.0411 *	1.2325
8	5.7862 *	0.7633	5.0257 *	0.9833	6.9791 *	1.2653
9	5.6250 *	0.7613	4.8461 *	1.0264	6.8470 *	1.2628
10	5.7815 *	0.7620	5.2281 *	1.0054	6.6483 *	1.2738
11	6.0379 *	0.7488	5.6732 *	1.0303	6.6059 *	1.3195
12	6.0040 *	0.7811	5.7764 *	0.9615	6.3561 *	1.2747
Obs	*1,648*		*1,036*		*612*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.

Table C8. Estimation results, alternative control group (Probability of employment)

	Total		Course		Course + contract	
t	**Effect**	**Standard error**	**Effect**	**Standard error**	**Effect**	**Standard error**
-5	-0.0114	0.0158	-0.0088	0.0158	-0.0186	0.0233
-4	0.0064	0.0186	0.0110	0.0186	-0.0061	0.0273
-3	0.0246	0.0203	0.0235	0.0194	0.0283	0.0252
-2	0.0046	0.0202	0.0128	0.0232	-0.0173	0.0286
-1	-0.0445	0.0216	-0.0404	0.0223	-0.0554	0.0275
1	0.0236	0.0350	-0.0941 *	0.0326	0.3426 *	0.0431
2	0.0916	0.0350	0.0002	0.0362	0.3385 *	0.0413
3	0.1046 *	0.0359	0.0182	0.0367	0.3379 *	0.0417
4	0.0919 *	0.0310	0.0182	0.0328	0.2897 *	0.0442
5	0.0991 *	0.0333	0.0278	0.0368	0.2906 *	0.0422
6	0.1174 *	0.0346	0.0491	0.0361	0.3007 *	0.0424
7	0.1186 *	0.0347	0.0474	0.0364	0.3096 *	0.0424
8	0.1217 *	0.0348	0.0546	0.0353	0.3017 *	0.0404
9	0.1112 *	0.0336	0.0412	0.0363	0.3020 *	0.0399
10	0.1155 *	0.0355	0.0440	0.0361	0.3104 *	0.0423
11	0.1035 *	0.0357	0.0308	0.0351	0.3014 *	0.0413
12	0.1155 *	0.0363	0.0461	0.0362	0.3045 *	0.0434
Obs	*1,922*		*1,554*		*772*	

Note: The asterisk indicates that the confidence interval does not cross 0 and therefore the estimated effect is significantly different from zero.