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# The Youth Employment Initiative in the EU: Is it NEET?

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# Abstract

This thesis evaluates the effectiveness of the Youth Employment Initiative (YEI), a major active labor market program (ALMP) implemented by the European Union from 2014 to 2020. The YEI aimed to reduce the rate of young people not in employment, education, or training (NEET) in NUTS2 regions with youth unemployment rates of 25% or higher in 2012. Using a fuzzy regression discontinuity design (RDD), the 25% eligibility threshold is exploited to estimate the causal impact of the YEI on NEET rates across EU regions. The results reveal that the YEI had a statistically significant effect in reducing NEET rates when controlling for economic conditions, on average, by 5.29 percentage points between 2014 and 2019. Tests on the methodology, including McCrary tests, RDdensity analysis, and covariate balance checks, were used to verify the identification strategy. Moreover, robustness checks were used to further very the findings under different conditions, which include a sensitivity analysis with different bandwidths and a placebo test for the McCrary test. This study contributes to the understanding of large-scale youth employment interventions. The findings suggest that targeted ALMPs can effectively reduce NEET rates, but their effectiveness may be influenced by economic conditions.

**Keywords:** European Union, European Structural and Investment Funds (ESIF), Youth Employment Initiative, active labour market policy, youth unemployment, subsidies.

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# List of Acronyms and Abbreviations

NEET - Young person not in employment, education or training

ESIF - European Structural and Investment Funds

YEI - Youth Employment Initiative

ALMP - Active labor market program

NUTS - Nomenclature of Territorial Units for Statistics

RDD - Regression discontinuity design

IV - Instrumental variable

AMSE - Asymptotic mean square error

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# Acknowledgements

This is the cherry on top to finish what I had always wanted to pursue: an education in economics. Initially, I did not have the courage to do a bachelor's in economics, as I was scared of maths. However, in my master's program, I decided to set aside my math insecurities and follow my passion. Admittedly, this master's degree was challenging; theoretical and empirical maths were hard to follow at first. However, after blood, sweat and tears (or "bloed zweet en tranen," as the Dutch would say), I was able to get through the content and understand it.

I am grateful for this experience and the people that have contributed to my education in Rotterdam. Firstly, I would like to thank my parents for encouraging me to take this risk and not opt for a pre-masters program (as I had initially planned). Also, their emotional support and reminders have been crucial for me throughout this year. Secondly, I would like to thank Dr. Webbink for sparking my interest towards econometrics, a field with which I had little experience. Admittedly, his course "Policy Evaluation" was essential for writing this thesis. Thirdly, I would like to thank my friends (in The Hague and Rotterdam) for their daily support, advice, and motivation during my time at Erasmus School of Economics. They have been my main source of happiness in the last year. Lastly, I would like to thank Dr. Kevin Spiritus for his feedback throughout this thesis journey.

**Note to readers:** This thesis has been written to be understandable for all readers, regardless of their background. For empiricists, some information may seem unnecessary or obvious. However, these details have been included to ensure accessibility for all audiences.

# 1. Introduction

Active labor market programs (ALMPs) are a popular policy choice among public officers to reintegrate young individuals into the workforce or enroll them in educational programs. These policies include job search assistance and monitoring, accessible training programs or wage subsidies (Caliendo and Schmidl, 2016). Nonetheless, the effectiveness of ALMPs have to be evaluated to ensure its goals are met. This certainly applies to the Youth Employment Initiative (YEI), a policy enforced during the European Commission's multiannual financial framework (the Commission's budgetary term) between 2014 and 2020. The initiative aimed to fund ALMPs through direct subsidies in EU regions with a youth unemployment above or equal to 25% in 2012.

YEI is arguably one of the largest ALMPs in the world, and if efficient, the initiative could benefit the youth not employed, in education, or training (NEETs), who made up 13% of all 15 to 24-year-olds in the European Union in 2013, according to Eurostat (2024d). Most notably, countries like Italy, Romania, and Bulgaria have had a worryingly high percentage of NEETs in 2013 and 2014, close to 20% or surpassing 20% (as in the case of Italy) (Eurostat, 2024d). Certainly, these high numbers in 2013 could have been influenced by the Sovereign Debt Crisis, which significantly affected the European Union and caused a negative labor supply shock. Hence, YEI could play a crucial role in addressing the high NEET rates caused by the Sovereign Debt Crisis, offering a path to better opportunities for Europe's youth.

Undoubtedly, the EU should aim to deliver efficient policy solutions to the youth, since young people are the most sensitive towards labor market fluctuations. For example, between 2008 and 2009, the EU faced an increase of five percentage points in EU youth unemployment rate from 15% to 20%, while "adults" experienced a two percentage point increase from 9% to 11% (Caliendo et al., 2011). This example highlights how the youth face a higher risk of unemployment compared to adults due to various factors, including limited work experience, fewer networking opportunities, incomplete or interrupted education, and the lower stability of initial jobs (Tomić, 2018). All of these factors can help create a "scarring effect", in which young Europeans can get stuck in persistent unemployment (Tomić, 2018). Thus, EU policymaking should focus on mitigating the disproportionate impact of labor market fluctuations on young people and preventing the long-term scarring effects of youth unemployment in the EU.

Moreover, NEETs can impose significant costs on society. They do not contribute to government tax revenue, which affects public funding (Tomić, 2018). Additionally, they may participate in protests or social unrest, destabilizing communities (Tomić, 2018). Furthermore, skilled youth leaving the country for better opportunities abroad reduces the nation's potential for economic growth (Tomić, 2018). Therefore, the significant costs to society imposed by NEETs call attention to need effective policies to integrate them into the workforce and mitigate these economic and social challenges.

Furthermore, YEI takes 2% of the cohesion policy budget in the EU, or 10 billion euros of the 531 billion euros available for economic, social or territorial development (European Commission, 2024). As the new European Parliament begins its term and new members take on key roles in policymaking, insights from YEI's performance during the 2016-2020 multiannual financial framework can fuel meaningful debates on tackling NEET rates. With high NEET rates persisting across the EU —- with countries like Romania reporting a 16.5% in 2023 (Eurostat, 2024d) — effectively tackling this issue remains an urgent challenge.

In sum, the YEI program is aimed at helping NEETs which impose significant costs to society, if their problems are not tackled. As such, an analysis of the policy needs to provide enough statistical evidence that it can reduce the NEET rate during the 2014-2020 multiannual financial framework to be considered effective. Otherwise, the YEI may be reformed or removed to make room for other policies.

The goal of this paper is to find the effect of the Youth Employment Initiative implemented in NUTS2 regions on 16 to 24-year-olds without education, employment or training in the European Union during the budgetary term between 2014 and 2020, excluding the year 2020. In order to achieve this goal, a fuzzy regression discontinuity design (RDD) was constructed. The design exploits YEI's exogenous threshold, where only NUTS2 regions with a youth unemployment rate above 25% in 2012 were eligible to benefit from the policy during the 6-year-long budgetary term. Nonetheless, not all regions met the 25% threshold and were given funds despite not reaching the minimum youth unemployment rate. Hence, an instrumental variable (IV) was added to isolate the exogenous variation in the treatment assignment. As such, the model allows for a comparison between treated and untreated regions driven by an exogenous cutoff. The results of the fuzzy regression discontinuity design indicate a coefficient of -5.29, suggesting that YEI recipient regions experienced a reduction in the NEET rate by 5.29 percentage points compared to non-recipient regions, after controlling for economic conditions. In sum, the research findings do provide sufficient evidence to conclude that the YEI had a significant effect on NEET rates among 16 to 24-year-olds in NUTS2 regions.

The thesis is organized as follows: firstly, the background section outlines the importance of YEI and provides the context needed to fully understand the policy. Next, the literature review section describes the current state of the literature and identifies gaps that this investigation aims to fill. Following this, the data section details the sources of the data, discusses the challenges encountered, and explains how these obstacles were addressed. The methodology section then extensively explains the identification strategy, including the choice of the bandwidth and the local polynomial. Afterward, the results section presents the findings at three levels: without controls, with controls, and across different years, to offer a comprehensive understanding of YEI's impact. The falsification tests section evaluates the robustness of the identification strategy through various tests. Then, the discussion section highlights the key findings and limitations of the study. Finally, the conclusion summarizes the main aspects of the investigation and reflects on the implications of the results for future research and policy design.

# 2. Background

## 2.1. What are NEETs?

A NEET is defined as a young person aged between 15 and 24 years old, who is unemployed and is not in any form of training or education (Mascherini, 2018). In EU publications, NEETs are reported as a percentage of the young population that are not in education, employment or training. Hence, the indicator is constructed through the following simple calculation:

$$NEET\ rate\ (\%) = \frac{Number\ of\ young\ persons\ not\ in\ education, employment\ or\ training\ (15-24)}{Total\ population\ of\ young\ persons\ (15-24)}$$

Unequivocally, the NEET rate differs from the youth unemployment rate. This is because the youth unemployment rate only measures the active youth population. In other words, the youth unemployment rate measures the number of unemployed young individuals actively searching for a job relative to the total young population that is either employed or actively searching for a job (Mascherini, 2018). As such, the youth unemployment rate tends to be higher than the NEET rate. Nonetheless, the absolute number of NEETs surpasses the unemployed youth. For instance, Mascherini (2018) uses the example of the NEET rate and youth unemployment rate in 2015: the youth unemployment rate in the EU was 20.3% and the

NEET rate was 12%. Yet, this accounted for 4,640,000 unemployed young people and 6,604,000 NEETs (Mascherini, 2018). Therefore, the NEET rate and youth unemployment rate differ significantly: while the first includes all young people not in education, employment, or training, the second only measures those actively seeking work, often resulting in the NEET rate being lower.

Furthermore, it is important to make a distinction between two types of NEETs, voluntary NEETs and non-voluntary NEETs. In this research, involuntary NEETs will be the focus, as they represent the most vulnerable group targeted by the YEI, given that they do not choose to be in this position. Mascherini's (2018) research highlights two principal factors that lead to the proliferation of involuntary NEETs in the EU: educational disadvantage and disaffection. The former is related to social factors such as personal characteristics, family and the school one attends to, while the latter concerns negative attitude that young people have towards education or schooling, leading to unexcused absences or expulsion from school (Mascherini, 2018). Nonetheless, the Eurofound adds a layer of complexity to that answer. As it argues that there is an interplay of "institutional, structural and individual factors" that affect the chances of an individual becoming an involuntary NEET (Mascherini et al., 2012, p. 53). Hence, focusing on involuntary NEETs is crucial, as they represent the most vulnerable group, influenced by a complex interplay of educational disadvantage, disaffection, and broader institutional and structural factors.

Moreover, there is a clear consensus on the heterogeneity among NEETs, as they do not all share the same characteristics. Notably, variations exist across factors such as gender, age, immigration background, perceived health status, education level, religiosity, and living status (Mascherini, 2018). For instance, the NEET rate was 16.7% for females compared to 13% for males in 2015 (Mascherini, 2018). Additionally, young people with an immigration background are 68% more likely to become NEET than nationals, and those with poor health or disabilities are 38% more likely to be NEET compared to their healthier peers in 2015 (Mascherini, 2018). Hence, NEETs show significant differences based on factors like gender, immigration background, and health.

In sum, NEETs are defined as young people between 15 and 24 years of age who are unemployed and are not in any form of training or education. This indicator is significantly different to youth unemployment rate, as it does not only look at the active youth population. Also, it is important to distinguish between involuntary and voluntary NEETs, as involuntary NEETs are more vulnerable to the interplay of institutional, structural, and individual factors that may lead them to becoming NEETs. Lastly, there is heterogeneity among NEETs, for example females and those with poor health conditions are more likely to be NEETs.

## 2.2. The development of the Youth Employment Initiative and its current relevance

NEET rates in the EU were very low, steadily declining until they reached their lowest level in 2008 at 10.7% (Mascherini, 2018; Eurostat 2024d). However, this decreasing trend halted as the Great Recession, the Sovereign Debt Crisis, and the subsequent austerity measures impacted the EU's overall economy. Consequently, the trend reversed, resulting in a NEET rate of 15.9% in 2013, the highest ever recorded (Mascherini, 2018). This high number illustrates that new entrants to the labor market are typically more affected than established workers during crises (Petrescu et al., 2024). As soon as the EU recognized the negative impact of the economic crises on NEETs, policymakers began discussing a common policy initiative in 2010 to reduce NEET rates across the union (Petrescu et al., 2024).

This shift toward prioritizing youth employment and education was crucial because NEETs are more vulnerable than adults. NEETs are exposed to the risk of experiencing long-term 'scarring effects' during extended economic crises, and they can cause significant economic costs to society. Firstly, the youth face a higher risk of unemployment compared to adults due to various factors, including limited work experience, fewer networking opportunities, incomplete or interrupted education, and the typically less stable nature of initial jobs (Tomić, 2018). Secondly, Cervia (2015) emphasizes the importance of securing employment or continuing education for recent graduates and young people to prevent the 'scarring effect' of economic crises. This effect results from poor labor market prospects during prolonged downturns, which can lead to worse long-term employment and income prospects (Cervia, 2015). Meaning that NEETs were at risk of getting trapped in a cycle of unemployment or informal unemployment. Therefore, if the youth are left without any aid under their vulnerable conditions, their problems could become structural issues which are more complicated to alleviate. Thirdly, NEETs also cause long-term negative economic impacts, contributing significantly to costs in social safety nets and lost tax revenue. For example, the cost of NEETs in 2015 amounted to a total of 142 billion euros annually, with a 12.2% NEET rate (Eurofound, 2024). At a societal level, high numbers of NEETs exert additional pressure on social welfare systems, which can have enduring negative consequences if NEETs remain unemployed or without education.

Ultimately, the prioritization of NEETs in the EU policy agenda led to the establishment of the Youth Guarantee program in 2012 (Tymowski, 2017). The initiative was created with the goal of ensuring that young people can find suitable jobs or educational programs for themselves. Under the Youth Guarantee program, all member states pledged to initiate active labor market policies (ALMPs) such as traineeships and apprenticeships, in order to reduce the number of NEETs (Bratti et al., 2022). Additionally, NEETs are central to the Youth Guarantee initiative, because it aims to ensure that all young people aged 15 to 24 who are not employed, in education, or in training receive a quality offer of employment, further education, or an apprenticeship or traineeship within 4 months of becoming unemployed or leaving formal education (Mascherini, 2018).

Yet, some nations faced a larger burden due to notably high levels of youth unemployment and were unable to fund as many projects as needed for the Youth Guarantee program. Therefore, the Youth Employment Initiative was implemented alongside the Youth Guarantee program, in order to deliver financial support to the regions facing an extraordinary level of youth unemployment in 2013 (Tymowski, 2017). Between 2014 and 2020, the YEI allocated 9 billion euros to Youth Guarantee projects aimed at NEETs aged 16 to 24 in EU NUTS2 regions with a youth unemployment rate of 25% or higher (European Commission, 2023a; Tymowski, 2017; Mascherini, 2018). YEI is also considered an active labor market policy under the OECD's definition, which states that ALMPs are those policies that are "aimed at the improvement of finding gainful employment or to otherwise increase their earnings capacity" (Vanroy, 2016, p.1). Hence, YEI is considered an ALMP because it aims to improve NEETs' chances of finding employment or education—thereby enhancing human capital formation and ultimately affecting their earning capacity—through regional subsidies.

After its implementation between 2014 and 2020, YEI remains important in the European stage. In May 2021, the European Commission released the European Pillar of Social Rights Action Plan, which stated the goal of reducing the rate of NEETs between 15–29 years of age to 9% by 2030 (Eurofound, 2024). To put this goal into context, in 2021 the NEET rate was at 13.1% and even today, the rate is at 11.2% (Eurostat, 2024). Notably, some member states have already reached the goal, such as The Netherlands or Denmark with NEET rates well below the 9% targeted. Nevertheless, it is still crucial to tackle exceptionally high rates in countries like Bulgaria, Italy or Greece since their NEET rates are surrounding 20% (Eurostat, 2024d). These high rates can be explained by the lasting effects of the COVID-19 pandemic. During the pandemic, the EU underwent heavy restrictions, including the suspension of schooling and restricted opening hours (affecting the ability for businesses to hire new workers). These effects damaged young people's ability to accumulate human capital and kick-start their professional lives (Eurofound, 2024). Hence, YEI can still take an important role to achieve the European Commission's goal to reach a NEET rate of 9% by 2030 through the provision of important subsidies to traineeships or studies. This study can provide evidence to better understand the impact of YEI in NEETs and how it takes a role to reach the European Commission's target.

In essence, NEET rates experienced a drastic increase during a period of prolonged economic stagnation after 2008. Consequently, EU policymakers acted to safeguard the naturally vulnerable NEETs, as well as to prevent long-term damaging effects such as the scarring effect and further societal costs including high pressure on welfare programs. Ultimately, it led to the development of the Youth Guarantee Program and the complementary financial program, the Youth Employment Initiative during the multiannual financial framework between 2014 and 2020. Nowadays, YEI remains relevant as European NEETs still suffer from the effects of the COVID-19 pandemic, which impaired their ability to accumulate human capital and enter the job market.

## 2.3. YEI within the European budget

The EU budget has always been a subject of intense debate, sparking heated discussions on how the EU funds should be distributed since the foundation of the European Community in 1958. Historically, the common agricultural policy (CAP) has amounted to the largest share of EU funds, corresponding to 92% of the budget at its peak in 1970 (Baldwin and Wyplosz, 2019). However, as soon as the EU experienced its first enlargement in 1973 with the addition of Ireland, Denmark and the United Kingdom; the budget has been increasingly shifting towards European Structural and Investment Funds (ESIF) (Baldwin and Wyplosz, 2019). ESIF's goal is to promote regional and urban development, or in other words, its goal is to enhance regional and urban cohesion with the rest of the union. Nowadays, the CAP and ESIF together account for a total of 80-85% of the entire budget, with ESIF alone comprising over 40% (Baldwin and Wyplosz, 2019). Indeed, this change towards cohesion spending is in line with the EU's goals. Article 174 of the Lisbon Treaty supports the need to aid the least favored regions in EU member states, to strengthen economic, social and territorial cohesion (Baldwin and Wyplosz, 2019).

Moreover, the EU budget is decided through medium-term agreements with a length of seven years under the "Multiannual Financial Framework". Currently, the 2021-2027 budgetary term is ongoing. Nonetheless, this paper will focus on the budgetary term 2014-2019 with the aim of analyzing its past effects.

During this period, the EU was committed to spend one third of the multiannual financial framework's budget on European Structural and Investment Funds, and it ultimately amounted to a total of 525 billion euros (Baldwin and Wyplosz, 2019; De Keersmaecker & Favalli, 2023). The total amount of 525 billion euros was divided into five different categories: the European Regional Development Fund, the European Social Fund, the Cohesion Fund, the European Agricultural Fund for Rural Development (EAFRD), and the European Maritime and Fisheries Fund (European Commission, 2023a). Notably, the regional development fund received the largest percentage of funds at 42.1%, followed closely by the social fund at 20.7%; conversely, the maritime and fisheries fund accounted for only 1% of the entire budget (European Commission, 2024). Similarly, the YEI program does not take a large part of the European Structural and Investment Fund's budget as it only makes up for 2%, or a total of 9 billion euros (European Commission, 2024). YEI is in a category on its own, which means that it is not in a subcategory of the social fund or the development fund. However, half of this fund is financed by the European Social Fund, and it is implemented in accordance with its rules (European Commission, 2023b). This means that the European Social Fund and YEI both share the objective of enhancing social inclusion through improving employment opportunities by employing substantial financial resources (Regulation (EU) No 1304/2013, 2013). The other half remaining of YEI's funds comes from dedicated funding towards the cause (European Commission, 2023b).

To sum up, the Youth Employment Initiative (YEI) is part of the Multiannual Financial Framework for the period 2014-2020. While it falls under the European Structural and Investment Funds, it operates within its own distinct category. Despite this separate classification, it adheres to the rules of the European Social Fund, with half of its total budget amounting to 9 billion euros coming from the European Social Fund.

# 3. Literature Review

## 3.1. Theoretical reasoning

A theoretical understanding of active labor market policies (ALMPs) —such as YEI— is crucial in order to be able to anticipate their effects. One can look at the potential impact of ALMPs through the lens of Kluve et al. (2016). According to the authors, youth participation in ALMPs will improve their employment and earning outcomes. In order to support such claim, they present positive causal chains that explain how active labor market policies enhance youth opportunities, as follows:

Firstly, NEETs in the EU may lack skills in technical, vocational, cognitive, and non-cognitive areas (Kluve et al., 2016). Through the supply of skills training, recipients will benefit from technical, vocational or non-cognitive skills that boost their human capital and make them desirable in the job market (Kluve et al., 2016). Hence, one should expect a reduction in youth unemployment rate after they benefit from training. Secondly, NEETs may lack information on desirable skills to employers or about job opportunities (Kluve et al., 2016). Through the delivery of employment and intermediation services, they lower transaction costs for NEETs to obtain information by facilitating information exchange between employers and potential employees (Kluve et al., 2016). Thus, the youth would be able to find more easily jobs that suit their preferences, and as such, youth unemployment rate would lower. Thirdly, NEETs may have little or no work experience (Kluve et al., 2016). Through the provision of subsidized employment, hiring costs lower and employers become more likely to hire young Europeans (Kluve et al., 2016). Therefore, individuals gain experience and become more productive, making them more attractive in the job market and leading to lower youth unemployment rates.

Furthermore, the Council of Economic Advisers (2016) also argue in favor of positive effects of ALMPs towards NEETs. They explain that ALMPs, particularly training programs, positively impact employment by enhancing participants' skills and productivity, making them more attractive in the job market. Hence, one should expect that YEI should reduce the youth unemployment rate after their recipients benefit from training.

Nonetheless, YEI could have negative impacts towards its recipients. Kluve et al. (2007) point out that policies that lead to direct job creation can lower the employability of the beneficiaries, because of the stigma created when entering the job market through a government position. Employers in the open market may view these individuals as less competitive or less capable than those who secured employment through standard market mechanisms.

In summary, the theoretical grounds in which YEI would work are vague in nature. Most of the theory predicts a positive outcome, nonetheless, empirical evidence is necessary to complement the theoretical reasoning.

## 3.2. Academic, national and EU literature

The literature available does not reach an overall consensus on the potential outcome of YEI. On the one hand, European Commission, European Parliament and national reports endorse the positive results of YEI at a national level using factual evidence or impact analysis techniques. On the other hand, academic papers show mixed results on the potential outcomes of YEI.

Firstly, The European Commission and European Parliament reports display positive results of YEI. Ferrer et al. (2016) in behalf of the Commission, use factual evidence to conclude that the YEI has played a positive role in reducing the number of NEETs during the period 2014-2016. They use national data from the 28 member states to display positive results. For example, in France, 32,000 participants took part in YEI funded programs. Out of the sample, 45% found a job, 22% got into education or training and the rest were still looking for a job (Ferrer et al., 2016). However, one of the main drawbacks of this paper is the lack of an econometric analysis. Hence, the paper lacks a layer of empirical depth of the findings, to critically evaluate whether the effect in the youth labor market was statistically significant.

Moreover, Tymowski (2017) in behalf of the European Parliament, uses data from the European Commission as well as information from the European Parliament to argue in favor of the effects of YEI in the EU until 2017. The author explains that the national evaluations from France and Poland, as well as the data from Greece and Spain, show that one third of the participants of YEI are entering the labor market (Tymkowski, 2017). In addition, the paper highlights Italy as a country where 44% of its YEI participants —who finished the program— were employed 6 months after they finished the program. Moreover, the author outlines the claim of the Commission which states that the EU experienced a decrease by 700,000 NEETs when comparing 2015 with 2013, as a direct result of Youth Guarantee program with support of YEI. However, just like Ferrer et al., Tymkowski does not use any econometric techniques to support his claims, only factual evidence derived from official records. Similarly, his statements show a strong support towards the program.

Then, national non-academic analyses also display positive results of YEI in 4 countries: England, Portugal, Slovenia and Croatia. They are summarized in Table 1. Most notably, the paper on England is the most detailed and complete analysis among the available papers. Atkinson and Cutmore (2022) unveil the effects of YEI in England during the budgetary term 2014-2020. They used a mixed methods approach by combining qualitative data such as interviews, and an empirical analysis through the use of diff-in-diff and propensity score matching, to estimate the effects of the financial support. Evidence shows that the number of NEETs were reduced from 786,000 to 651,000 from 2014 to 2018 (Atkinson and Cutmore, 2022). Also, the authors found out that YEI participants were employed for 56 days longer than non-participants and that 25% of those who left the YEI program were finding employment

(Atkinson and Cutmore, 2022). Additionally, the researchers conducted a cost-benefit analysis based on 20 YEI projects, considering four benefits: increased earnings for individuals, increased tax revenue, reductions in government welfare spending, and health benefits from being employed. In regard to costs, the 20 projects had different costs per participant, making each project unique. Nonetheless, the average cost per participant was calculated to be a total of 2,600 pounds (Atkinson and Cutmore, 2022). The results of the cost-benefit analysis estimated a social return of £1.50 to £1.55 per £1 spent in YEI, based on the four benefits considered and the cost per person of each project. Therefore, the overall results of YEI were positive in England.

Additionally, Duarte et al.'s (2020) analysis on the effects of YEI in Portugal, employs a sophisticated empirical methodology through the use of an optimal matching algorithm in order to deliver causal effects. The authors identify different effects of distinct youth guarantee programs, such as internships from 6 to 18 months, employment from 6 to 18 months or, a mixed program including an internship of 12 months and employment for 12 months. Overall, YEI had a positive impact in Portugal, leading to higher wages for those treated, with an increase of 145 to 313 euros compared to their counterfactual (Duarte et al., 2020). Also, those who benefited from projects financed by YEI are 7.7 to 31.7 percentage points more likely to be employed than those in the same situation without YEI support. Hence, YEI had a significant positive impact on the treated in Portugal.

However, not all national reports are as thorough and sophisticated as in the case of Duarte et al. (2020) and Atkinson and Cutmore (2022). Botrić (2017) and Ramsak (2017) evaluated the effects of YEI in Croatia and Slovenia respectively only using factual evidence provided by the national governments. Their analyses were mainly informative about the labor market in Croatia and Slovenia in 2017, and they also described the socio-political context of the nations. As such, the authors' analyses were not causal, and they only outlined the factual evidence provided by the national governments. For instance, Botrić (2017) states that the overall youth unemployment rate lowered from 42.1% in 2012 to 31.1% in 2016 while YEI was being implemented in Croatia. It is surely informative, but the decrease in youth unemployment rate cannot be fully attributed to YEI. In the case of Slovenia, Ramsak (2017) indicates that YEI was able to benefit 2891 people between 2016 and 2017. This fact requires a deeper analysis to understand how it affected the whole youth unemployment rate in Slovenia. Thus, the two analyses by Ramsak (2017) and Botrić (2017) are mainly descriptive. In regard to the rest of EU member states, they all have released a national statement on YEI in 2016 called “First results”. They are also descriptive in nature, and they display a general positive sentiment towards the policy.

In summary, the papers by the European Commission and the European Parliament show a strong support towards YEI, however, they do not employ any econometric techniques to reach causal claims. In the case of national reports, Portugal and England have sophisticated assessments through the use of econometric methodology, and in the case of England even a qualitative analysis and a cost-benefit analysis. They display very positive results and argue in favor of YEI. On the other hand, countries like Slovenia and Croatia provide national reports only based on factual evidence. They lack any sort of critical evaluation of the policy, and they are informative in nature, but with a positive outlook on YEI.

| Country | Method of analysis | Date of publication | Findings |
|---|---|---|---|
| Portugal (Duarte et al., 2020) | Optimal matching algorithm | 2020 | Higher earnings in internships, between 145 and 313 euros more. Higher employability than non-treated, between 7.7 and 31.7 p.p. |
| Slovenia (Ramsak, 2017) | Factual data | 2017 | 2981 young people found employment. |
| England (Cutmore, 2022) | Cost benefit analysis, and mixed methods (qualitative and quantitative analysis) | 2022 | Reduction in NEETs from 786,000 in 2014 to 651,000 in 2018. Longer employment. Social return of £1.50 to £1.55 per £1 spent in YEI. |
| Croatia (Botrić, 2017) | Factual data | 2017 | Decrease in youth unemployment rate from 42.1% in 2012 to 31.1% in 2016. |
| Rest of EU | Factual data | 2016 | General decrease in youth unemployment. |

**Table 1:** National analyses of YEI

Now looking at the available academic literature, there are three types of papers: literature on Eurepean Structural and Investment Funds (ESIF), papers in programs funded by YEI or research in general youth employment programs. Firstly, ESIF papers display mixed results. Arguably, the most important paper on European Structural and Investment Funds was written by Becker et al. (2010). The author looks at the impact of ESIF in economic growth and unemployment from 1986-2006 while looking at NUTS2 regions. This paper is the closest to the work that is intended to be done in this thesis, with the main difference being a different budgetary term and the focus on YEI. The results of the investigation are derived through the use of a regression discontinuity design (RDD) and they display no employment growth effects of any kind. Moreover, Crescenzi and Giua (2020) use a spatial RDD to explore the effects of Cohesion Policy on economic growth and employment in the period from the year 2000 until 2014. The authors conclude with positive and significant EU-wide impacts in employment and economic growth. These two papers display the effects of cohesion policies and their impact on employment, however, these did not target NEETs specifically nor were they aimed towards the youth.

Then, there is very limited literature on specific Youth Guarantee programs that benefitted from YEI. Nonetheless, the paper by Bratti et al. (2022) is the only one that I was able to find. The research analyzes the effectiveness of a vocational training program for NEETs in Latvia which was funded by YEI. It uses a fuzzy RDD to estimate the impact of participating in the program. Yet, the results show no significant effects on employment status or labor income after training.

Lastly, the literature on general youth employment programs can be summarized by the meta-analysis by Kluve (2010). It explores the effectiveness of European active labor market programs (ALMPs) until 2010. As a result, the author explains that ALMPs targeted towards the youth are less effective than those targeted at adults and that there is no association between unemployment rate and ALMPs targeted towards the unemployed youth. Nonetheless, Kluve (2010) points out that the latest papers on youth ALMPs were showing positive results, probably due to new estimation methods which are more reliable. But even after this remark, the results remain insignificant and one would not expect positive results of YEI.

In sum, there is an important gap in the empirical literature because there are no papers that address the impact of YEI on an EU wide level. As of now, most reports focus on results at a national level, and they lack depth; while academic papers address either specific projects that were funded by YEI or they look at the wider picture through the analysis of all the European Structural and Investment Funds. Therefore, a paper addressing the effects of YEI on the EU as a whole, would be able to fill the current gap in the literature.

# 4. Data

## 4.1. The data used

The main data source used in this investigation is Eurostat. The source provided data on NEET rates, youth unemployment rate, YEI funds allocated, real growth rate and real labor productivity. As such, most of the data used in the report comes from this governmental source, excluding the statistics on the amount of YEI funds received, which comes from the Directorate-General for Regional and Urban Policy of the European Commission. All the observations in this study are recorded annually and regionalized according to the NUTS method at level 2. Additionally, the data covers the population aged between 16 and 24. The reason for this range is simple, the YEI targets NEETs aged between 16 and 24 years old (Tymkowski, 2017). Moreover, the observations cover the time frame between 2014 until 2019; except for youth unemployment rate, which covers only the year 2012 —because 2012 is the only year in which youth unemployment is used to determine treatment status. Even though the multiannual financial framework covers the years 2014 until 2020, 2020 has been excluded. The year 2020 was extraordinary due to the COVID-19 pandemic, which significantly affected the NEET rate in the EU. The impact of YEI might have been undermined by the ongoing health crisis, making 2020 an outlier and not representative of normal economic conditions. Therefore, removing 2020 from the analysis ensures consistency in the data and allows for an accurate evaluation of YEI's typical performance.

## 4.2. NUTS

The NUTS (Nomenclature of Territorial Units for Statistics) system is Eurostat's method for classifying economic regions based on national administrative subdivisions and population size to facilitate the EU's fund delivery mechanisms (Eurostat, 2024a). Eurostat divides European regions into three levels (level 1, level 2, and level 3) according to these criteria. This investigation will focus on NUTS2 (NUTS level 2) which are the regional subdivisions in each member state with populations between 800,000 and 3 million people, identified by a two-digit code (Eurostat, 2024a). The decision for choosing NUTS2 is because YEI funds are distributed to NUTS2 regions.

Notably, NUTS classifications last at least 3 years to ensure some statistical stability. For example, NUTS 2010 covers the classification of regions from 2012 until 2014. However, due to national interests or population changes, regions may undergo reclassification after this period. There are five potential changes regions might experience in their NUTS reclassification: name change, recoding, discontinuation, creation of new region, and boundary shift or splitting (Eurostat, 2024a). Appendix 1 provides further details on each of the changes. Most importantly, it highlights the creation of a new region, boundary shifts, and region splits as the most disruptive changes in NUTS reclassification.

In sum, NUTS is Eurostat's regional classification system which last for 3 years, and it is based on administrative subdivisions and population size. After 3 years, they may experience changes of which, the creation of a new region, boundary shifts and region splits are the most significant changes.

## 4.3. Challenges with NUTS

The data available on Eurostat is presented in varying NUTS classifications. In this investigation, data covers the budgetary period from 2014 to 2019, which spans six years and could include at least two changes in NUTS classifications. Indeed, this was the case, with a shift from NUTS 2010 to NUTS 2013, followed by a change from NUTS 2013 to NUTS 2016. NUTS 2010 was in effect from 2012 to 2015, NUTS 2013 from 2015 to 2017, and NUTS 2016 from 2018 to 2020. As there were two reclassifications, there was the risk of receiving data from Eurostat at different NUTS classifications if the datasets were not all simultaneously and homogeneously updated. Certainly, Eurostat's datasets did not provide all the data in one NUTS classification, so the regional observations were categorized under NUTS 2010, NUTS 2013 and NUTS2016. Thus, a decision was made to settle on one common NUTS classification to preserve consistency throughout the panel data and allow for regional comparisons. NUTS 2010 was chosen as the main classification system to which all observations would be transformed. This classification was selected because YEI funds were delivered based on the youth unemployment rate of 2012, and the NUTS classification in use in 2012 was NUTS 2010.

The conversion into NUTS2010 classification was calculated through the use of asymmetric spatial interpolation in R by Hennicke and Werner (2024). This technique uses three different weights: regional area in square kilometres, population size and built-up area. The R package allows to transform data from later NUTS versions to older NUTS versions and vice versa. Therefore, through the use of the R package by Hennicke and Krause, the conversion to NUTS 2010 was possible. Nonetheless, not all regions needed to be converted to the 2010 classification. Only regions that underwent a significant change, such as discontinuation, creation of a new region, boundary shift, or split, had to be converted back to

NUTS 2010. The relevant changes that regions experienced with newer NUTS classifications are detailed in Appendices 2 to 4.

Ultimately, asymmetric spatial interpolation was the best method to obtain NUTS 2010 through the use of weights and considering differences across time, because there are no repositories for data from previous versions or alternative methods for gathering NUTS 2010 data.

## 4.4. Descriptive statistics

| Variables | (1) Observations | (2) Mean | (3) SD | (4) Min | (5) Max |
|---|---|---|---|---|---|
| Treated Regions | 272.00 | 0.68 | 0.47 | 0.00 | 1.00 |
| YEI Funding Allocated | 272.00 | 21,255,386.31 | 41,130,325.45 | 0.00 | 306,350,150.00 |

**Table 2:** YEI allocation and treatment

| Variables | (1) Observations | (2) Mean | (3) SD | (4) Min | (5) Max |
|---|---|---|---|---|---|
| Youth Unemployment 2012 | 256 | 26.70 | 15.12 | 4.10 | 72.30 |
| NEETS 2012 | 264 | 13.03 | 6.03 | 3.10 | 35.90 |
| NEETS 2019 | 253 | 10.64 | 5.63 | 2.70 | 35.00 |
| Avg NEETs 14-19 | 269 | 11.61 | 5.75 | 3.17 | 32.87 |

**Table 3:** Youth unemployment and NEET rate

Tables 2 and 3 provide descriptive statistics to identify initial patterns and summarize data. In Table 2, one can see a balanced table with 272 regions which are identified in the NUTS 2010 classification. This is because the data comes from DG Regio which is partly responsible for covering and managing YEI funds (European Commission, 2024). However, Table 3, is not balanced. This occurs because EUROSTAT relies on NUTS2 regions to gather the data. For instance, there are only 256 observations out of the possible 272 for the youth unemployment rate in 2012.

Furthermore, Table 2 shows that, on average, NUTS2 regions received 21,255,386 euros to fund Youth Guarantee programs. Notably, the Andalusia region in Spain received the highest YEI funding, totalling 306,350,150 euros.

In regard to patterns and trends, one can observe in Table 3 that NEET rates have decreased from 2012 to 2019, on average 2.39 percentage points. Such a feat, could have been caused by the wide availability of YEI funds across the EU. On Table 2, one can observe that

68% of NUTS2 regions were treated, and hence, they received YEI funds. Additionally, the notion that YEI funds contributed meaningfully to a decrease in the NEET rate is further supported by the fact that NEET rates were lower on average during the entire treatment period from 2014 to 2019 compared to 2012.

In summary, despite initial signals indicating a reduction of NEET rates during the treatment period, an econometric analysis with a sound identification strategy is required to be able to make causal claims on whether YEI funds truly contributed to a decrease in NEET rates.

# 5. Methodology

## 5.1. Identification strategy

The objective of this research is to estimate the effect of the Youth Employment Initiative (YEI) on NEETs from 2014 to 2019, focusing on NUTS2 regions with a youth unemployment rate of 25% or higher in 2012—referred to as treated regions. To achieve this, a regression discontinuity design (RDD) has been employed. RDD is useful for estimating treatment effects in non-experimental settings, particularly when treatment is assigned based on whether an observed variable surpasses a specific cutoff (Thistlethwaite & Campbell, 1960). In this study, RDD will quantify the impact of receiving YEI funding for regions with a youth unemployment rate above the 25% threshold in 2012. The model relies on the assumption that regions just above and below the cutoff are comparable, except for the treatment received. This allows for a direct comparison at the cutoff and accurate estimation of YEI's effects. Hence, RDD is chosen for its ability to utilize the exogenously determined 25% threshold to evaluate the impact effectively. As such, a simple RDD model can be constructed, with subscripts $t$ for time (in year) and $i$ for the specific NUTS2 region:

**Equation 1:** $N_{it} = \beta 0_t + \beta 1 T_{it} + f(Y_{it}) + \varepsilon_{it}$

Equation 1 resembles a sharp regression discontinuity design. Where $\underline{N}$ is the dependent variable, and it represents NEET rate per NUTS2 region. Then, $T$ represents the main independent variable, which is a dummy denoting whether a NUTS2 region has been allocated YEI (Youth Employment Initiative) funds, taking the value of 1 if the region has received such funding and 0 otherwise. Lastly, $Y$ denotes the youth unemployment rate in 2012, which is the running variable/assignment variable. In essence, the running variable determines which observations are eligible for treatment.

Furthermore, it is essential to analyze the data to assess whether the sharp regression discontinuity design (RDD) in Equation 1 is appropriate. This is because there are two main types of RDD: sharp and fuzzy. In order to determine which one to choose, it is important to understand the nature of the assignment rule. Specifically, it is necessary to check whether there is perfect compliance on both sides of the cutoff (deterministic) or imperfect compliance (probabilistic) (Hahn et al., 2001). If the assignment rule is deterministic, meaning that the treatment is assigned strictly based on whether the running variable is above or below the cutoff, a sharp RDD is appropriate (Hahn et al., 2001). On the other hand, if there is imperfect compliance, where the treatment assignment is not strictly adhered to based on the cutoff, a fuzzy RDD is a better choice (Hahn et al., 2001).

To facilitate the identification of a deterministic or probabilistic assignment rule, Graph 1 displays the probability of receiving treatment near the threshold, specifically within 5 units above and below the 25% youth unemployment rate. On the right side of the cutoff, there is perfect compliance; all observations are treated. However, on the left side of the cutoff, there are observations that receive treatment despite not being eligible for it. Therefore, fuzzy regression discontinuity design is the most appropriate RDD type, because there is a probabilistic assignment rule (imperfect compliance around the threshold).

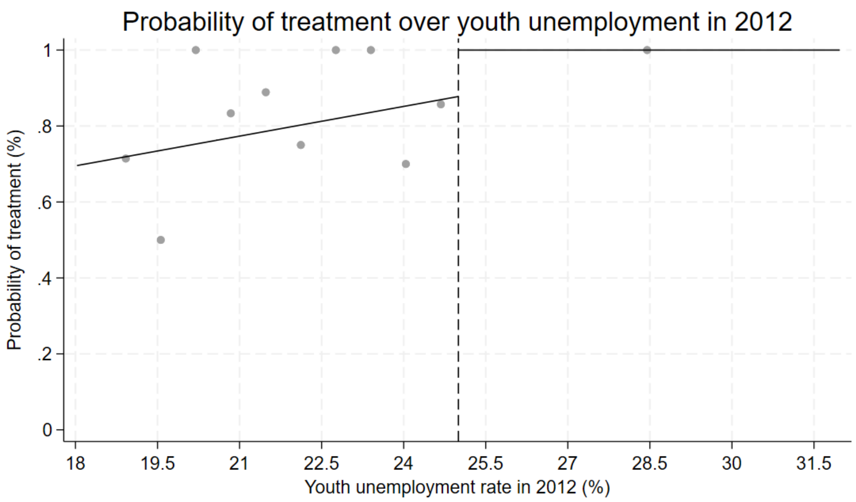


**Figure 1**: Probability of receiving treatment near the threshold

A fuzzy RDD is characterized by the use of an instrumental variable (IV), in order to prevent endogeneity concerns. Endogeneity can be defined as the case in which an explanatory variable is correlated to the error term. Stated differently, the instrumental variable will allow us to eliminate the risk of other variables affecting the probability of treatment. For instance, political leverage or past deals with the EU might have influenced NUTS2 treatment status. As such, these concerns can be tackled through an IV that is exogenous. With the introduction of an instrumental variable, the model will calculate a local average treatment effect (LATE). This means that once the IV is added, the design will no longer estimate the effect for the entire population, instead the effect for the subgroup that becomes treated because of the introduction of the instrumental variable. In this paper, the instrumental variable is a binary indicator that captures compliance with the 25% youth unemployment threshold rule for YEI fund allocation. More specifically, the IV equals 1 if a region has a youth unemployment rate of 25% or higher and receives YEI funds. While the IV equals 0 if the region has a youth unemployment rate below 25% and does not receive YEI funds. Hence, the IV design would calculate the effect of the policy on the subgroup of regions that fully comply with the exogenous rule.

After adding the instrumental variable to the regression discontinuity design, the model becomes a so–called "two-stage" model. This is because the IV cannot influence the outcome variable directly. In the first stage, the instrumental variable (IV) is used to estimate the probability of treatment, ensuring that any variation in the treatment variable is due to the IV and no other confounding factors. In the second stage, the probability of treatment estimates the effect on the outcome variable. This approach ensures that the outcome is influenced by the IV only indirectly through its effect on the treatment variable. The first stage of the model is defined as follows:

**Equation 2 (First stage):** $T_{it} = a0_t + a1D_{it} + f(Y_{it}) + \mu_{it}$

Where $T$ is a binary variable indicating if a NUTS2 region received YEI funds, with $T$=1 for regions with funding and $T$=0 otherwise. Then, $D$ is the instrumental variable, a dummy variable indicating compliance with the 25% youth unemployment threshold rule for YEI fund allocation. It equals 1 if a region's youth unemployment rate is 25% or higher, qualifying it for YEI funds, and 0 if the rate is below 25%, resulting in no YEI funds. Furthermore, $Y$ is the running variable: youth unemployment rate in 2012. Lastly, $\mu$ is the error term of the first stage. Moreover, the second stage is specified as follows:

**Equation 3 (Second stage):** $N_{it} = \beta0_t + \beta1\hat{T}_{it} + X_{it} + f(Y_{it}) + \varepsilon_{it}$

Where $\hat{T}$ explains how much of $T_{it}$ is explained by $D_{it}$ in the first stage. In essence, this illustrates how the IV affects the outcome variable. Then, $N$ represents the rate of NEETs per NUTS2 region. $N$ will take the form of the average rate of NEETs between 2014-2019. Additionally, it will also be shaped as the NEET rate for each individual year in the period of interest in each analyzed NUTS2 region. Moreover, $X$ represents a vector of control variables, including real labor productivity and real growth rate in 2012 and the average during the treatment period. Lastly, $\varepsilon$ is the error term of the second stage. Ultimately, the local average treatment effect (LATE) is calculated through the second stage estimation, providing insights into the impact of YEI funding on NEET rates.

## 5.2. Identification assumption

An RDD is able to identify the treatment effect through the identification assumption that the observations just above or just below the cutoff are on average the same, with the only difference being whether they have been treated or not. This approach selects observations below the threshold as the control group and those above the cutoff as the treatment group, based on the running variable (youth unemployment rate). The effect of the policy is then calculated as the difference between these two groups.

In order to ensure that the identification assumption is satisfied, two potential issues have to not be apparent: i) manipulation of the treatment and ii) discontinuities in covariates.

### 5.2.1. Manipulation of the treatment

Manipulation of the treatment may occur if EU regions have the ability to control their own youth unemployment statistics. If regions were able to alter their statistics, the assignment of treatment would no longer be as good as random. For example, if regions manipulated their data to obtain additional YEI funds, it could introduce selection bias, leading to a sample that is neither representative nor balanced on average.

Overall, there are no significant concerns suggesting manipulation of the treatment in the design. To support this assertion, both theoretical reasoning and statistical evidence will be provided. Statistical evidence will be presented through a McCrary test — and an RDensity analysis located in appendix 4—, while theoretical reasoning will be discussed subsequently.

The main theoretical argument against the possibility of sorting at the threshold is based on the assumption that NUTS2 regions can not manipulate historical data. The Youth Employment Initiative was proposed and launched in 2013 for the budgetary period from 2014 to 2020, with eligibility determined by youth unemployment data from 2012 (European Commission, 2023b). Therefore, since NUTS2 regions could not have altered their youth unemployment data from 2012, they could not have known in advance that they needed to meet the 25% youth unemployment threshold to qualify for YEI funds. As such, this theoretical reasoning supports the assumption that the assignment of treatment around the threshold is essentially random, supporting the credibility of the RD design.

Furthermore, the McCrary test is designed to check for manipulation of the running variable in an RDD at the cutoff point, by looking for discontinuities. Figure 2 displays a visual depiction of the test, with a dashed line which shows the cutoff, a solid black line that represents the estimated density function and two lines above and below the black line which represent the confidence intervals. Upon inspection, there is a noticeable jump in the density function of approximately 0.02 units at the threshold. This suggests a higher density below the cutoff and lower density above the cutoff. Therefore, there are initial signs of potential manipulation of the running variable in NUTS2 regions to benefit from YEI funds.

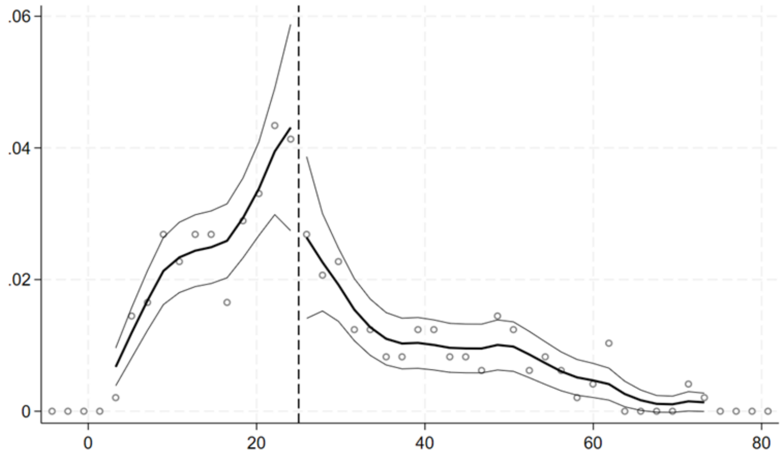


**Figure 2:** McCrary test, graphical display

However, a graphical analysis is not sufficient to establish that the discontinuity is significant. As such, one has to rely on the estimates provided by the test itself, which can be seen in Table 4. The McCrary test yielded an estimate of -0.470, which suggest a discontinuity at the threshold. Yet, the standard error is equal to 0.396, which leads to a t-statistic of -1.19 and a p-value of  0.235, providing no significant evidence of manipulation at the threshold. This suggests that there is no strong evidence of manipulation of the running variable at the cutoff point.

| | (0) |
|---|---|
| Cutoff point | 25 |
| McCrary estimate | -0.470 |
| p-value | 0.24 |
| T-statistic | -1.19 |
| Bandwidth | 6.97 |

**Table 4:** McCrary test at the cutoff

Additionally, Appendix 4 provides an *RDdensity* test (including a visual and statistical component) in order to check for manipulation at the threshold. Similarly, this test raises no concerns of manipulation at the threshold.

#### 5.2.2. Discontinuities in covariates

The identification assumption in a Regression Discontinuity Design (RDD) may be questioned if control variables show a discontinuity at the cutoff. This assumption relies on the idea that any observed jump in the outcome variable at the cutoff is due solely to the treatment effect. Therefore, it is important to check for continuity in control variables around the threshold. If control variables do not remain continuous at the cutoff, it suggests that other factors besides the treatment might be influencing the outcome, which could weaken the validity of the RDD.

Overall, there are no significant concerns about discontinuities in the control variables. To confirm this, both visual and statistical analyses will be conducted. Specifically, STATA commands *rdplot* and *rdrobust* will be used to check for any discontinuities in the control variables. In this case, a McCrary test was not possible because it is designed to be used only for the running variable.

Additionally, the *rdrobust* command is also used for the estimation of the treatment effect in the results section. *rdrobust* estimates are presented in three forms: conventional, bias-corrected and robust. Appendix 6 explains the differences between the three types. Nonetheless, the main takeaway is that this thesis will primarily focus on conventional estimates and, more importantly, robust estimates. The conventional estimate shows a baseline effect without bias correction, whereas the robust estimate provides the most accurate result.

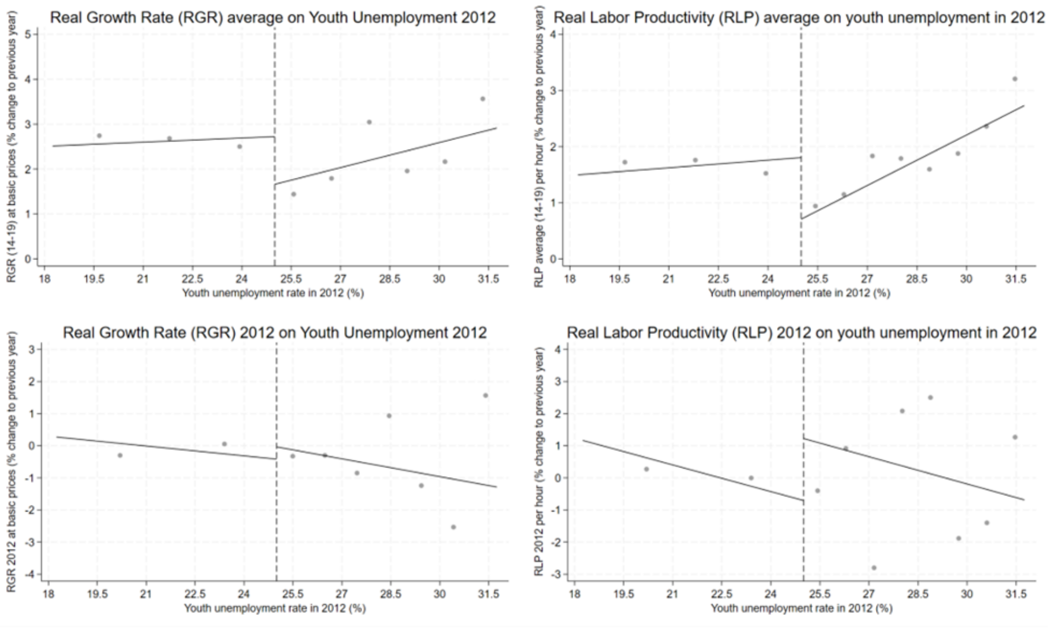


**Figure 3:** graphical display of the covariates at the threshold

Figure 3 presents four panels: the top panels show the average values of the real growth rate (RGR) and real labor productivity (RLP) from 2014 to 2019, while the bottom panels display specific data for 2012. In all panels, there appears to be a discontinuity at the threshold. Notably, the RLP graphs display notable discontinuities at the threshold. The bottom right panel shows a noticeable drop at the threshold of about 1 percentage point, while the top right panel depicts a large upward shift at the cutoff of approximately 1.5 percentage points. These visual discontinuities call for further statistical analysis to determine their impact on the validity of the RD design. Conversely, the RGR graphs show smaller discontinuities at the threshold. The top left panel indicates a drop of approximately 1 percentage point, while the bottom left panel shows a minor upward shift of about 0.01 percentage points at the cutoff. In sum, the discontinuities observed in Figure 3 require a statistical analysis to assess the validity of the RDD.

| | (1)<br>Avg. RGR 2014-2019 | (2)<br>Avg. RLP 2014-2019 | (3)<br>RGR 2012 | (4)<br>RLP 2012 |
|---|---|---|---|---|
| Conventional | -0.91 | -0.66 | 0.28 | 1.36 |
| | (0.74) | (0.56) | (1.16) | (1.52) |
| Bias-corrected | -1.09 | -0.77 | 0.37 | 1.59 |
| | (0.74) | (0.56) | (1.16) | (1.52) |
| Robust | -1.09 | -0.77 | 0.37 | 1.59 |
| | (0.89) | (0.68) | (1.43) | (1.82) |
| Conventional p-value | 0.22 | 0.24 | 0.81 | 0.37 |
| Robust p-value | 0.22 | 0.26 | 0.80 | 0.38 |

Standard errors in parentheses

**Table 5:** covariate jump at the threshold

To address the concerns raised in the visual analysis, the *rdrobust* results are displayed in Table 5. Firstly, addressing the variables that raised concerns in the visual analysis, the average RLP shows a conventional estimate of -0.66 and a bias-corrected estimate of -0.77. However, the p-values of 0.24 (conventional) and 0.26 (robust) suggest that this difference is not statistically significant. Then, RLP in 2012 shows the largest point estimates which are 1.36 in its conventional form and 1.59 when bias-corrected, aligning with the visual concerns. Yet, the p-values of 0.371 (conventional) and 0.38 (robust) indicate that these differences are not statistically significant. Furthermore, when looking at the variables that did not raise as large concerns, the average RGR estimate shows a decrease of approximately 1 percentage point at the threshold. Again, the p-values of 0.22 indicate that this difference is not statistically significant. For RGR in 2012, the estimates are positive, being equal to 0.28 if conventional, and being 0.37 if bias-corrected. Nonetheless, the high p-values also undermine a potential discontinuity at the threshold. All things considered, while the visual analysis suggested discontinuities in the control variables, especially for RLP, the statistical analysis using *rdrobust* does not confirm these as statistically significant. Therefore, there should not be major concerns in the validity of the model in regard to the continuity of the control variables across the threshold.

## 5.3. Instrumental variable assumptions

The model chosen is a fuzzy regression discontinuity design, and it uses an instrumental variable. In order to have a valid IV, there are 4 criteria that have to be complied: i) meaningful first stage, ii) monotonicity assumption, iii) independence assumption and iv) exclusion restriction.

Overall, there are no significant concerns about any of the IV assumptions. To corroborate the validity of the IV, theoretical reasoning will be provided for all IV assumptions. Additionally, the meaningful first-stage assumption will be examined through a two-stage least squares regression.

### 5.3.1. Meaningful first stage

A meaningful first stage requires that the instrument is strongly related to the treatment variable (Columbia University, 2024). Otherwise, the instrumental variable would fall under weak instrumental variable concerns. A two-stage least squares regression was conducted as a test to examine the first stage's significance. In the first stage, the treatment dummy was regressed on youth unemployment in 2012, using the IV to instrument for youth unemployment. The results strongly support the relevance and strength of the instrument. The first-stage F-statistic is 351.789, well above the conventional threshold of 10, indicating that the instrument is strong and relevant. Hence, the finding suggest that there should not be any weak instrument bias concerns. Indeed, the results make sense because the IV is a subset of the treated. Particularly, the complying regions that abided by the exogenous rule imposed by the EU. Therefore, it makes sense to have a meaningful first stage which correlates with the endogenous variable in the first stage. In the second stage, there is a statistically significant relationship between youth unemployment in 2012 and the treatment dummy, with a coefficient of 0.04. This result indicates that higher youth unemployment rates are associated with a higher probability of receiving the treatment, which aligns with the program's nature. Overall, these results strengthen the confidence in the instrumental variable strategy, suggesting it is appropriate for estimating the causal effect of the YEI on NEET rates.

### 5.3.2. Monotonicity assumption

Furthermore, the monotonicity assumption affirms that there are no defiers, which are observations that work the opposite way as the treatment assignment suggests (Columbia University, 2024). Hence, there is only the presence of compliers: regions that get the treatment when eligible, but do not get the treatment when they are under the threshold. From a theoretical standpoint, true defiers are unlikely to exist. After adding the IV, all non-complying regions have been “excluded” from the regression, turning the treatment eligibility from probabilistic to deterministic. This means that as the youth unemployment rate increases, the probability of receiving the YEI funding also increases, with a sharp jump at the 25% threshold. The sharp jump can be seen in Figure 4, where the probability of treatment before the threshold is at 0, but experiences a substantial jump after the threshold. The IV ensures that regions with higher youth unemployment rates are always more likely to receive treatment than those with lower rates, regardless of their position relative to the cutoff. This mechanism effectively rules out the possibility of defiers, as the treatment probability is monotonically increasing with the running variable. Consequently, the IV approach satisfies the crucial monotonicity assumption required for causal inference in fuzzy RDD settings, strengthening the validity of the empirical strategy.

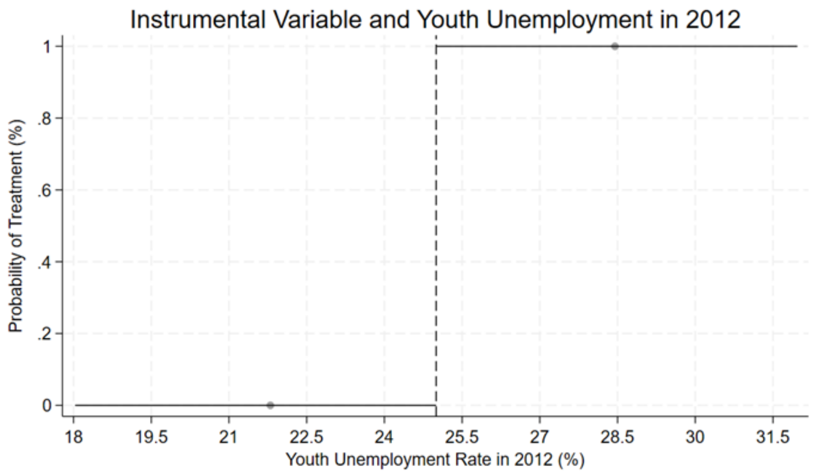


**Figure 4:** The effect of IV on compliance

### 5.3.3. Independence assumption

Moreover, the independence assumption states that the allocation of YEI funds is as good as random among the units around the threshold (Columbia University, 2024). This assumption can be theoretically justified by the nature of the instrument and the context of YEI implementation. The 25% youth unemployment threshold used for allocating YEI funds acts as an exogenous cutoff set by EU policymakers. This exogeneity is crucial, as it allows us to isolate the causal effect of the YEI on NEET rates. Additionally, the threshold is not affected by regional characteristics or trends that might influence NEET rates, but instead is a fixed criterion regions must meet to qualify for YEI funding. Therefore, regions just above and below this 25% threshold are expected to be on average similar, except for whether they receive YEI funds. This similarity means that any systematic differences in NEET rates between these regions can be attributed to the YEI funds rather than other factors. In sum, the exogenous nature of the threshold ensures that the assignment of treatment near the cutoff is as good as random, hence, fulfilling the independence assumption.

### 5.3.4. Exclusion restriction

Lastly, the exclusion restriction states that the instrumental variable (IV) affects the outcome only through its impact on the treatment, ensuring an unbiased estimation. In this context, the exclusion restriction is likely to be met due to the nature of the policy. The 25% youth unemployment threshold for YEI fund allocation was established as an exogenous cutoff by EU policymakers. This exogenous threshold implies that crossing it should not directly influence NEET rates except through the use of YEI funds. Surpassing the threshold does not affect NEET rates by itself, it only determines program eligibility. Hence, there should not be a direct relationship between the passing the threshold and lower NEET rates. This should only occur through the use of YEI funds, which are given if a region meets the 25% threshold. In summary, the 25% threshold instrument should theoretically only affect NEET rates through its role in determining YEI fund allocation, satisfying the exclusion restriction.

## 5.4. Bandwidth selection

In RD analyses, the focus is on the observations that are close to the threshold. Therefore, one must make a sound decision on which observations should be regarded as "close enough" to the threshold, the so-called "bandwidth". Very importantly, there is a trade-off between bias and variance in the estimate when choosing the bandwidth. A narrower bandwidth, closer to the cutoff, reduces bias because the observations are more similar at the cutoff point, but it increases variance due to a smaller sample size. Conversely, a wider bandwidth includes more observations further from the cutoff, reducing variance but introducing more bias as these observations may differ significantly from those near the threshold. As such, selecting the appropriate bandwidth is essential to balance the need for a low bias and low variance estimate of the treatment effect.

The selection of the bandwidth was made using the *rdbwselect* command in STATA, developed by Calonico et al. (2014). The command offers multiple bandwidth selection procedures; the one used in this project was *mserd*. Specifically, this command calculates a common bandwidth for both sides of the cutoff (left and right) through the asymptotic mean square error (AMSE) procedure, a measure that minimizes the bias and variance of an estimator (Pei et al., 2022). Thus, this command helps minimize bias and variance in the bandwidth. A significant advantage of rdbwselect is that it provides an indication of the bandwidth bias, which can be controlled for and added to the estimator to account for it. Additionally, *rdbwselect* works side by side with the *rdrobust* command (both developed by the same authors), used in this project to obtain the treatment effect of the regressions. Hence, it allows for consistent and robust estimation. Ultimately, after running the command, the final bandwidth chosen was 6.971 on both sides of the cutoff, with a bias of 10.940.

## 5.5. Polynomial selection

As seen in Equation 2 and Equation 3, the running variable is influenced by a function, which is modelled by a polynomial. This polynomial helps to define the relationship between the running variable and the dependent variable. Selecting an appropriate polynomial order is crucial in an RDD to accurately estimate the treatment effect. On the one hand, a high order polynomial can capture complex trends between the running variable and the outcome. However, there is the risk that high order polynomials may overfit the model, which means that the design captures noise instead of the relationship between the variables. On the other hand, a low polynomial might be too simple, and may fail to capture the true relationship. As such, the selection of the most optimal polynomial is necessary to ensure accuracy and prevent overfitting, among other problems.

The polynomial choice has been based off two academic papers, Gelman and Imbens (2019) and Pei et al. (2024).

Gelman and Imbens (2019) argue that high-order polynomials should not be used in sharp or fuzzy RD designs. Instead, a local linear or quadratic polynomial should be sufficient. Their argument lies in three main reasons: noisy weights, high sensitivity to the degree of the polynomial and inferences that do not achieve nominal coverage. Firstly, the academics explain that high-order polynomials attach large weights to individuals with extreme values in the running variable, which may be positive or negative, but often larger than the average weight of one observation. This is not the case in local linear or quadratic estimators, which give a zero weight to extreme observations. Secondly, the authors argue that estimates in RDD are highly sensitive to the choice of high-order polynomials. They demonstrate this by comparing results from global polynomials of 1st to 6th order with local polynomials of 1st and 2nd order. The estimates from high-order global polynomials (3rd to 6th) varied substantially, ranging from -0.112 to -0.069, while the local polynomial estimates showed less variation, only ranging from -0.078 to -0.064. This sensitivity is concerning because the variation in estimates across different polynomial orders exceeds what the standard errors suggest, indicating that standard errors fail to capture the full uncertainty in the estimates. Therefore, the authors advise against using high-order global polynomials in RDD due to this sensitivity and the failure of standard errors in reflecting the true uncertainty of the estimates. Thirdly, Gelman and Imbens (2019) argue that conventional inference methods using high-order polynomials in RDD can be misleading. They demonstrate that confidence intervals derived from high-order polynomial regressions often have lower than nominal coverage, meaning they fail to include the true value as often as they should. For instance, they displayed examples in which high polynomials showed rejection rates substantially above 5% level, while local linear and quadratic estimators maintained rejection rates close to 5%. Also, high-order polynomials displayed larger standard errors with confidence intervals that were too narrow, resulting in over-rejection of the null hypothesis. This combination of large standard errors and under-coverage of confidence intervals makes high-order polynomials less reliable in fuzzy or sharp RD designs. In sum, Gelman and Imbens (2019) suggest for the use of low-order polynomials that are linear or quadratic due to undesirable weights, a high sensitivity of the estimate and the over-rejection of the null hypothesis, in the case of high order polynomials.

Gelman and Imbens clarify that a linear or quadratic polynomial is typically the preferred choice for modeling in regression discontinuity designs. However, deciding which polynomial order is more appropriate can be challenging. To address this issue, Pei et al. (2024) developed a package called rdmse, which uses asymptotic mean squared error (AMSE) as a criterion for polynomial selection. As explained in the bandwidth selection section, AMSE accounts for both the bias and variance of an estimator. The rdmse package calculates two types of AMSE: conventional AMSE and bias-corrected AMSE. The conventional AMSE measures the combined variance and squared bias of the estimator, while the bias-corrected AMSE adjusts for the bias introduced by the bandwidth choice itself. This approach allows researchers to compare AMSE values across different polynomial orders and select the one with the lowest AMSE, which minimizes both bias and variance and thus provides the most reliable estimate of the treatment effect.

| | (1) Polynomial order 1 | (2) Polynomial order 2 | (3) Polynomial order 3 | (4) Polynomial order 4 |
|---|---|---|---|---|
| AMSE_conventional | 2.49 | 6.29 | 17.00 | 29.46 |
| AMSE_bias-corrected | 3.64 | 7.49 | 18.82 | 31.20 |

**Table 6:** *rdmse* check for different polynomial orders

Table 6 provides the AMSE values for the model from a polynomial of order 1 until order 4. Certainly, the lowest AMSE values are in the polynomial order 1. Therefore, polynomial order 1 would minimize variance and bias, which makes it the ideal polynomial for the model. This selection is in line with the literature explored by Pei et al. (2024) and Gelman and Imbens (2019).

# 6. Results

## 6.1. The main regression without controls

The main model employs a fuzzy regression discontinuity design to assess the impact of YEI funding on NEET rates in NUTS2 regions. To investigate the treatment effect, it is essential to analyze the second-stage estimates of the model, which reveal the effect on NEET rates.

This analysis is conducted using the *rdrobust* command, developed by Calonico et al. (2017). When using the command, the user must choose a kernel, which functions as weights in the model. Among the various types of kernels available, the uniform and triangular kernels were carefully considered. In the end, the triangular kernel was selected as the optimal choice. This decision is based on the reasoning that observations further from the cutoff tend to differ more from those closer to the cutoff. Triangular weights assign higher weights to observations near the cutoff and lower weights to those further away. In contrast, uniform weights treat all observations equally, regardless of their distance from the cutoff. Therefore, the use of triangular weights helps to decrease the bias of the model.

| | (1)<br>Main regression without controls |
|---|---|
| Conventional | -2.14 |
| | (1.58) |
| Bias-corrected | -2.10 |
| | (1.58) |
| Robust | -2.10 |
| | (1.91) |
| Kernel Type | Triangular |
| Observations | 185 |
| Effective number of observations (left of cutoff) | 13 |
| Effective number of observations (right of cutoff) | 38 |
| Conventional p-value | 0.18 |
| Robust p-value | 0.27 |
| Bandwidth | 6.97 |

Standard errors in parentheses

**Table 7:** regression results without controls

Table 7 displays the results of the model without any control variables. The results suggest that regions just above the 25% youth unemployment threshold have on average a 2.14 percentage point lower NEET rate compared to regions just below the threshold. However, this effect is not statistically significant even at a 10% level, with a p-value above 0.10 in both of its forms, conventional and robust. Moreover, the conventional and robust estimates are very similar, with a difference of only 0.04 p.p more in the conventional estimate and a larger standard error in the robust estimate. The similarity in estimates suggests that bias correction didn't substantially change the results, while the larger standard error in the robust estimate portrays increased uncertainty. Remarkably, the analysis only included 13 effective observations to the left of the cutoff and 38 to the right. This can be a cause of concern due to a small sample size, which may make the treatment effect less likely to be close to the true effect. It may not be representative of the population mean. In sum, the model without any controls does not display any statistically significant results. Therefore, the results suggest that YEI did not play a significant role to decrease NEET rates in NUTS2 regions that benefitted from the program.

### 6.2. The main regression with controls

The initial RDD analysis without controls did not yield statistically significant results. However, after adding key economic indicators as controls, one can observe significant changes in both the magnitude and statistical significance of the estimates. This subsection presents these results and discusses their implications.

| | (1)<br>No covariates | (2)<br>4 additional covariates |
|---|---|---|
| Conventional | -2.14 | -4.55* |
| | (1.58) | (2.35) |
| Bias-corrected | -2.10 | -5.29** |
| | (1.58) | (2.35) |
| Robust | -2.10 | -5.29* |
| | (1.91) | (2.70) |
| Conventional p-value | 0.18 | 0.05 |
| Robust p-value | 0.27 | 0.05 |
| Bandwidth | 6.97 | 6.752 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Table 8:** regression results comparison between no controls and with controls

Table 8 presents the RDD estimates, both with and without controls. The controls included are real labor productivity in 2012, average real labor productivity (2014-2020), real growth rate in 2012, and average real growth rate (2014-2020) which come from Eurostat (2024b, 2024c). The controls introduced capture both the initial economic conditions at the time of YEI fund eligibility (2012) and the ongoing economic trends during the policy implementation period (2014-2020). They aim to isolate the effect of YEI funds from broader economic factors that might influence NEET rates. As can be seen from Table 8, the inclusion of controls led to a substantial change in the estimates. The coefficient for the effect of YEI funds on NEET rates increased two percentage points and reached the 10% level of statistical significance in the conventional and robust estimates. The emergence of significant results after controlling for economic factors indicates that the effectiveness of YEI funds may be dependent on broader economic conditions. From a methodological perspective, the substantial changes observed after the inclusion of controls stress the critical importance of adding appropriate controls in RDD analyses. While one might assume that controls in an RDD design should only affect the standard errors, the significant impact of adding these controls reveals that key economic indicators play an essential role in understanding the treatment effect. Therefore, the addition of controls not only refines the estimates but also unveils that YEI funds may have indeed had a statistically significant impact on NEET rates in NUTS2 regions when economic conditions are taken into account. In summary, the inclusion of controls introduces a layer of complexity to the analysis but also reveals that, when adjusting for economic conditions, YEI funds had a statistically significant effect on NEET rates.

## 6.3. Across time

Although examining the average effect of the Youth Employment Initiative (YEI) over the entire treatment period (2014-2019) gives an overview of the program's impact, it is also important to analyze the effects of the program in individual years. A year-by-year analysis takes into consideration potential temporal variations in the program's effectiveness, which may be hidden when looking at the average effect. Additionally, looking at individual years helps to identify whether YEI's impact was immediate or delayed, whether the effect strengthened or weakened over time, and if there were any particular years when the program was notably more or less effective. This approach can help policymakers and researchers understand the full trajectory of YEI's impact.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Avg. NEETS 2014-2019 | NEETS 2014 | NEETS 2015 | NEETS 2016 | NEETS 2017 | NEETS 2018 | NEETS 2019 |
| Conventional | -2.14 | -4.42* | -3.20** | -2.23 | -0.20 | -1.03 | -1.42 |
| | (1.58) | (2.32) | (1.53) | (1.44) | (1.82) | (1.92) | (1.93) |
| Bias-corrected | -2.10 | -5.04** | -2.94* | -2.41* | 0.00 | -0.87 | -1.39 |
| | (1.58) | (2.32) | (1.53) | (1.44) | (1.82) | (1.92) | (1.93) |
| Robust | -2.10 | -5.04* | -2.94 | -2.41 | 0.00 | -0.87 | -1.39 |
| | (1.91) | (2.69) | (1.87) | (1.75) | (2.19) | (2.30) | (2.32) |
| Observations | 185 | 185 | 185 | 185 | 181 | 181 | 180 |
| Conventional p-value | 0.18 | 0.06 | 0.037 | 0.12 | 0.91 | 0.59 | 0.46 |
| Robust p-value | 0.27 | 0.06 | 0.12 | 0.17 | 0.99 | 0.70 | 0.55 |
| Bandwidth | 6.97 | 7.35 | 6.91 | 7.15 | 6.09 | 6.55 | 7.83 |
| Effective number of observations (left of cutoff) | 13 | 15 | 13 | 14 | 11 | 13 | 16 |
| Effective number of observations (right of cutoff) | 38 | 40 | 38 | 40 | 35 | 36 | 39 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Table 9:** regression results across time

Table 9 presents the results of the regression discontinuity analysis assessing the impact of the Youth Employment Initiative on NEET rates across different years, without including control variables. When examining individual years, we observe that the YEI had significant effects during the early years of the program. In 2014, the conventional estimator indicates a significant reduction of 4.42 percentage points in NEET rates, with the robust estimator showing a larger reduction of 5.04 percentage points. Similarly, in 2015, the conventional estimator reveals a significant reduction of 3.20 percentage points, while the bias-corrected estimator shows a reduction of 2.94 percentage points. Interestingly, the robust estimator for 2015 does not achieve statistical significance at the 10% level, even though both the conventional and bias-corrected estimators do. Additionally, from 2016 to 2019, the overall effects are generally not statistically significant. Moreover, the strength of the YEI's impact appears to diminish over time. The program's effect on NEET rates decreases from an approximate reduction of 5 percentage points in 2014 to about -1.40 percentage points in 2019. This temporal pattern suggests that the YEI had a more substantial initial impact, which decreased in subsequent years. This diminishing effect is consistent with the idea that, over time, the impacts of the Sovereign Debt Crisis and the Great Recession gradually wear off. Lastly, it is worth noting that just as in Table 7, individual years also do not have many observations on the left-hand side. Hence, the same limitation applies to Table 9 across time.

| | Avg. NEETS 2014-2019 | NEETS 2014 | NEETS 2015 | NEETS 2016 | NEETS 2017 | NEETS 2018 | NEETS 2019 |
|---|---|---|---|---|---|---|---|
| Conventional | -4.55* | -7.50* | -1.99 | -4.75* | -2.53* | -2.93*** | -4.02 |
| | (2.35) | (4.47) | (1.75) | (2.55) | (1.52) | (1.06) | (2.64) |
| Bias-corrected | -5.29** | -8.17* | -2.46 | -5.26** | -2.92* | -3.29*** | -4.77* |
| | (2.35) | (4.47) | (1.75) | (2.55) | (1.52) | (1.06) | (2.64) |
| Robust | -5.29* | -8.17 | -2.46 | -5.26* | -2.92 | -3.29*** | -4.77 |
| | (2.70) | (5.12) | (2.12) | (2.97) | (1.82) | (1.25) | (3.09) |
| Observations | 176 | 176 | 176 | 176 | 172 | 172 | 171 |
| Conventional p-value | 0.05 | 0.09 | 0.26 | 0.06 | 0.10 | 0.01 | 0.13 |
| Robust p-value | 0.05 | 0.11 | 0.25 | 0.08 | 0.11 | 0.01 | 0.12 |
| Bandwidth | 6.75 | 6.07 | 7.35 | 6.21 | 6.96 | 7.48 | 7.23 |
| Effective number of observations (left of cutoff) | 10 | 8 | 12 | 9 | 10 | 13 | 12 |
| Effective number of observations (right of cutoff) | 34 | 32 | 37 | 33 | 35 | 37 | 36 |

**Table 10:** regression results across time with controls

Table 10 shows the results when control variables (real growth rate in 2012, real growth rate 2014-2019, real labor productivity 2012 and real labor productivity 2014-2019) are included in the regression. Looking at individual years, we observe significant effects in multiple years: 2014, 2016, 2017, and 2018. Notably, the effect in 2018 is highly significant across all estimation methods. Moreover, the magnitude of these effects is generally larger compared to Table 9, ranging from -2.53 to -8.17 percentage points. Even though the estimates for 2014 show a significant initial drop of around 8 percentage points, subsequent years suggest a more persistent and substantial impact of the YEI when controlling for economic factors. Lastly, similarly to Table 9 and Table 7, the effective number of observations at the left of the cutoff remains quite low.

Comparing the two tables reveals several important differences. Firstly, the inclusion of control variables in Table 10 leads to larger estimated effects of the YEI on NEET rates. The average effect for 2014-2019 becomes statistically significant and more than doubles in magnitude (from -2.14 to -4.55). Secondly, while Table 9 shows significant effects only in the early years (2014-2015), Table 10 demonstrates significant impacts in later years as well (2016-2018). This suggests that controlling for economic factors unveils a more enduring effect of the YEI. Thirdly, the statistical significance of the results generally improves in Table 10, with more estimates reaching significance levels of $p<0.05$ or $p<0.01$. Lastly, the pattern of effects over time differs between the tables. In Table 9, the effect appears to diminish over time, while in Table 10, the impact seems more consistent across years, with a particularly strong effect in 2018.

Overall, Tables 9 and 10 suggest that the YEI has had a significant effect on decreasing NEET rates in NUTS2 regions. Notably, while the initial findings indicate that the effect diminishes over time when economic conditions are not controlled for, incorporating these controls reveals that the treatment effect becomes more persistent over time.

# 7. Robustness checks

## 7.1. Robustness check: McCrary test

| | (0) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| | | +1 | -1 | +5 | -5 |
| Cutoff point | 25 | 26 | 24 | 30 | 20 |
| McCrary estimate | -0.470 | -0.24 | 0.06 | 0.06 | 0.40 |
| p-value | 0.24 | 0.64 | 0.98 | 0.89 | 0.41 |
| T-statistic | -1.19 | -0.47 | 0.03 | 0.14 | 0.82 |
| Bandwidth | 6.97 | 5.49 | 13.80 | 10.15 | 4.90 |

**Table 11:** McCrary test under different cutoff points

Running McCrary tests at different thresholds is advisable to help to verify the robustness of the initial findings at the true cutoff point. Additionally, it can also provide evidence for other points of manipulation in the distribution which could undermine the design. Taking a close look at Table 11, there is further evidence to believe that there is not a significant manipulation at any other points. For the thresholds at 26 (%) and 24 (%) youth unemployment rate, the McCrary estimates are smaller in magnitude (-0.24 and 0.06 respectively) than at the cutoff point, and have larger p-values (0.64 and 0.98), indicating no significant discontinuity. Then, at the more distant thresholds at 30 and 20, the estimates vary more (0.06 and 0.40), but remain statistically insignificant with high p-values. Hence, these results provide evidence that the there is no manipulation at the threshold or any other points, strengthening the validity of the design.

## 7.2. Sensitivity check: are results sensitive to different bandwidths?

Conducting a sensitivity check with different bandwidths helps to determine whether the results are stable across various bandwidth choices, or if they are sensitive to a specific bandwidth selection. Most notably, it provides an insight into the trade-off between bias and variance in the estimates when choosing a bandwidth.

| | (1)<br>Original<br>BW | (2)<br>Three times the<br>BW | (3)<br>Double the<br>BW | (4)<br>Half the<br>BW |
|---|---|---|---|---|
| Conventional | -4.49* | -0.42 | -2.63** | -4.21* |
| | (2.31) | (1.05) | (1.32) | (2.43) |
| Bias-corrected | -4.95** | -4.18*** | -5.29*** | 6.26*** |
| | (2.31) | (1.05) | (1.32) | (2.43) |
| Robust | -4.95* | -4.18** | -5.29** | 6.26 |
| | (2.82) | (1.84) | (2.30) | (16.27) |
| Observations | 176 | 176 | 176 | 176 |
| Effective number of observations (left of cutoff) | 10 | 71 | 36 | 4 |
| Effective number of observations (right of cutoff) | 35 | 71 | 53 | 18 |
| Conventional p-value | 0.05 | 0.69 | 0.05 | 0.08 |
| Robust p-value | 0.08 | 0.02 | 0.02 | 0.70 |
| Bandwidth | 6.97 | 20.91 | 13.94 | 3.49 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Table 12:** bandwidth sensitivity analysis with controls

Table 12 presents the results of the sensitivity analysis across four different bandwidths with controls: the original bandwidth, three times the original bandwidth, double the original bandwidth and half the original bandwidth. As a reminder, with the original bandwidth of 6.97, the YEI shows a statistically significant effect on NEET rates, reducing them by 4.95 percentage points. When the bandwidth is doubled to 13.94, the estimated effect changes slightly to -5.29 percentage points—a difference of only 0.34 percentage points—but remains statistically significant. Increasing the bandwidth further to 20.91 (triple) results in an estimate of -4.18 percentage points. While this change is larger than the previous estimate, it still represents a modest difference of 0.77 percentage points and maintains statistical significance. This suggests that expanding the bandwidth to include observations farther from the cutoff introduces some bias, possibly due to including less comparable regions. On the other hand, halving the bandwidth to 3.49 leads to a larger estimate of 6.26, but with increased standard errors and no statistical significance. This is likely due to the small sample size close to the cutoff, including only 4 effective observations on the left of the cutoff and 18 on the right of the cutoff. In sum, these results suggest that the main findings are reasonably robust to bandwidth choice, especially when considering the bandwidths that are double or triple the original size. In these two cases, the direction of the effect remains consistent, and the magnitude of the effect stays relatively close. However, when examining the estimates generated with a bandwidth half the original size, concerns arise due to the low effective sample size, which causes the estimate to drastically change. This highlights the importance of careful bandwidth selection.

# 8. Discussion

## 8.1. General findings

The analysis of the Youth Employment Initiative's (YEI) impact on NEET rates in NUTS2 regions reveals several key findings. Firstly, without controls, the effect appeared insignificant, suggesting that YEI funds do not influence NEET rates. However, after adding economic indicators as controls, the magnitude of the estimated effect increased, and it also revealed statistically significant impacts. This suggested that it is important to take into consideration economic conditions. Secondly, the year-by-year analysis without controls showed that YEI had stronger effects in its first years, with the impact decreasing over time. Yet, when economic controls were added, YEI's effect became more consistent across years. The vast differences in results between models with and without controls highlights the importance of considering economic context in this case as well. Overall, even though the YEI's impact changes depending on the analytical approach, there is sufficient evidence to suggest that it contributed to reducing NEET rates between 2014 and 2019, especially when accounting for economic factors.

Furthermore, the validity of the regression discontinuity design for evaluating the Youth Employment Initiative's impact on NEET rates is largely supported by the methodology section and further supported by the sensitivity tests. Firstly, the McCrary test and RDdensity analysis found no significant evidence of manipulation at the 25% youth unemployment threshold, indicating that regions did not sort around the cutoff. Secondly, sensitivity checks across different bandwidths demonstrated that the main findings are reasonably robust, particularly for bandwidths double or triple the original size. Thirdly, balance checks on covariates also did not reveal any significant jumps at the threshold, despite showing initial signs of discontinuities in the visual analysis. Fourthly, the instrumental variable used in the RD design satisfied its key assumptions: a meaningful first stage was confirmed with a high F-statistic, the monotonicity assumption was theoretically justified, the independence assumption was supported by the exogenous nature of the threshold, and the exclusion restriction was theoretically explained through met the policy's design. These results strengthen the credibility of the empirical strategy and the causal interpretation of the YEI's effect on NEET rates.

As a whole, this investigation provided significant results to establish a negative effect of YEI funds on NEET rates in NUTS2 regions, with a credible empirical strategy to establish a causal relationship.

## 8.2. Limitations

Despite the findings, this study has several limitations that should be considered when interpreting the results.

Firstly, the limited number of effective observations within the optimal bandwidth may weaken the robustness of the findings. A larger sample size in the bandwidth could have provided more definitive results.

Secondly, the lack of year-specific control variables for YEI, specifically the absence of data on annual fund allocation per region, affects the precision of the instrumental variable across different years, possibly hiding more significant temporal variations in the impact of YEI funding. Furthermore, it hinders the estimation of the marginal effect of YEI funds, limiting the policy implications on the impact of funding. This calls for further research with to fully understand the impact of YEI funds on NEET rates.

Thirdly, a balance table would have helped determine whether the regions across the threshold were equal on average. However, the *balancetable* command could not select the same number of effective observations across the threshold used in *rdrobust*, and it lacked options to provide weights, such as triangular kernels or local polynomials, as in *rdrobust* or *rddensity*. Therefore, there are still some minor concerns regarding the comparability of the treatment and control groups.

Fourthly, in the sensitivity check across different bandwidths, the bandwidth bias was equal to the bandwidth itself. This is because when a bandwidth is not calculated through *rdbwselect* or *rdrobust*, *rdrobust* is unable to calculate the bandwidth bias itself. Therefore, there could be inaccuracies in the non-optimal bandwidths, which introduce additional uncertainty to the sensitivity check.

Fifthly, a notable limitation of the study is the inability to cluster standard errors. Clustering is necessary to account for potential correlation of errors within groups. Especially under the context of the study, NUTS2 regions within the same country are likely to share unobserved characteristics. Ultimately, the lack of clustered standard errors may have led to inflated t-statistics or narrow confidence intervals.

In sixth place, the study may have had underestimated YEI's impact due to the age-restricted definition of NEETs. As individuals who benefit from the intervention age out of the 15-24 year category, they are no longer captured in the NEET statistics, even if they have successfully transitioned to employment. At the same time, new cohorts entering this age range may temporarily inflate the NEET numbers before they can benefit from the program. This dynamic could lead to an underestimation of the policy's effectiveness, as the methodology doesn't track individual trajectories. Future research could address this by incorporating longitudinal data to capture longer-term outcomes of YEI beneficiaries.

Lastly and most importantly, this investigation does not conduct a heterogeneity analysis. Such analysis is crucial to fully understand the depth of the YEI program. For instance, YEI might be more effective in reducing NEET rates in ALMPs that subsidize employment than in job search programs, or YEI funds might be more effective towards women than men. Future research should include a heterogeneity analysis to capture the varying impacts of different YEI interventions, through the use of micro-level data.

# 9. Conclusion

This thesis has provided a comprehensive evaluation of YEI, a major ALMP implemented by the European Union from 2014 to 2020. Using a fuzzy regression discontinuity design, the results delivered evidence of a statistical significant effect of YEI in reducing NEET rates across eligible EU regions.

Primary results indicate that the YEI led to a statistically significant reduction in NEET rates by 4.95 percentage points. This effect was particularly pronounced when controlling for economic factors such as real growth rate and labor productivity. The impact of the YEI, however, varied over time, suggesting that the program's effectiveness may be influenced by broader economic conditions.

The validity of the findings is strengthened by a series of rigorous tests. The McCrary test (and RDdensity analysis) found no significant evidence of manipulation at the 25% youth unemployment threshold, supporting the assumption that regions did not sort around the cutoff. Sensitivity checks across different bandwidths demonstrated the robustness of the main findings, particularly for bandwidths double or triple the original size. Discontinuity checks on covariates, while showing some visual discontinuities, did not reveal statistically significant jumps at the threshold, preserving the continuity assumption. Furthermore, the instrumental variable approach satisfied key assumptions, including a strong first stage, theoretically justified monotonicity, and likely adherence to the exclusion restriction.

These results collectively strengthen the credibility of the empirical strategy and support a causal interpretation of the YEI's effect on NEET rates. They suggest that targeted YEI can indeed make a difference in addressing youth unemployment and youth education.

However, it is important to acknowledge the limitations of the study. The analyses, through the use of the optimal bandwidth, deliver an estimate with a low sample size. A larger sample size could have provided stronger results. Furthermore, while the analysis was at a macro-level, and it provided valuable results; it does not take into consideration heterogeneity in the YEI's impact across different subgroups. Future research could benefit from a heterogeneity analysis, examining how the program's effectiveness varies based on factors such as gender, age, education level, and Youth Guarantee programs.

Despite these limitations, the findings make a significant contribution to the understanding of large-scale youth employment interventions. They provide empirical evidence supporting YEI in combating youth unemployment and youth education. However, it is important to establish that this thesis does not serve as a final policy recommendation to keep or end the YEI. Further research, with the use of alternative methodological approaches and heterogeneity analyses, are necessary to reach a definitive conclusion about the program's overall effect.

As the EU continues to prioritize youth employment and engagement, the insights from this study and subsequent research will be crucial in shaping evidence-based policies. The ultimate goal remains to ensure that European youth have access to meaningful employment and educational opportunities, contributing to both individual well-being and broader economic prosperity. Future policy design should still consider the vulnerability of the youth, and this thesis provides evidence to establish that the youth can be helped through the use of programs like YEI.

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# Appendices

## Appendix 1: Explanation of different types of reclassification

There are five potential changes regions might experience in their NUTS reclassification: name change, recoding, discontinuation, creation of new region, and boundary shift or splitting (Eurostat, 2024a). A name change involves only a change in how a region is referred to. Recoding entails a change in the 2-digit code of NUTS2 regions; for example, the Polish NUTS2 region Podlaskie was recoded in 2016 from PL84 to PL34. Discontinuation of a region results in the region ceasing to exist. The creation of a new region, boundary shift, and splitting lead to the most disruptive changes in NUTS classification. Creating a new region may involve discontinuing other regions and forming a new united region, developing a new area without certain zones, or a mix of both. A boundary shift changes the area's size, potentially affecting other existing regions. Lastly, if a region splits, it causes the formation of at least two new regions with unique codes, names, and boundaries.

# Appendix 2: NUTS changes (from 2010 to 2013)

| Region | NUTS2010 code | New region, if applicable | NUTS2013 code (change experienced) |
|---|---|---|---|
| Inner London | UK11 | Inner London West and Inner London East | UK13 and UK14 (split) |
| Outer London | UK12 | Outer London East and North East, Outer London East and North East and Outer London West and North West | UK15, UK16 and UK17 (split) |
| Vzhodna Slovenija | SI01 | - | SI03 (boundary change) |
| Zahodna Slovenija | SI02 | - | SI04 (boundary change) |
| Guadeloupe | FR91 | - | FRA1 (boundary shift) |

# Appendix 3: NUTS changes (from 2013 to 2016)

| Region | NUTS2013 code | New region (if applicable) | NUTS2016 code (change experienced) |
|---|---|---|---|
| Eastern Scotland | UKM2 | - | UKM7 (boundary shift) |
| South Western Scotland | UKM3 | West Central Scotland and Southern Scotland | UKM8 and UKM9 (split) |

| | | | |
|---|---|---|---|
| Border, Midland and Western (Ireland) | IE01 | - | (Discontinued) |
| - | - | Northern and Western (Ireland) | IE04 (new region) |
| Southern and Eastern (Ireland) | IE02 | - | (discontinued) |
| - | - | Southern (Ireland) | IE05 (new region) |
| - | - | Eastern and Midland (Ireland) | IE06 (new region) |
| Lithuania | LT00 | Sostinés regionas and Vidurio ir vakaru Lietuvos regionas | LT01 and LT00 (split) |
| Kozep-Magyarorszag | HU10 | Budapest and Pest | HU11 and HU12 (split) |
| Mazowieckie | PL12 | Warszawski stoleczny and Mazowiecki regionalny | PL91 and PL92 (split) |

# Appendix 4: NUTS changes (from 2016 to 2021)

| **Region** | **NUTS2016 code** | **New region, if applicable** | **NUTS2021 code (change experienced)** |
|---|---|---|---|
| Kontinentalna Hrvastska | HR04 | Panonska Hrvatska, Grad Zagreb and Sjeverna Hrvatska | HR02, HR05 and HR06 (split) |

## Appendix 5: RDdensity analysis

Even though the McCrary test provided strong evidence that there is no manipulation at the threshold, RDdensity offers additional insights to support this claim. Developed by Cattaneo and Jansson (2018), RDdensity is an ideal complement to the McCrary test because it incorporates the same weights used in the *rdrobust* analysis, including the kernel and linear polynomial methods. Therefore, RDdensity serves as a valuable tool for further addressing concerns about manipulation at the threshold.

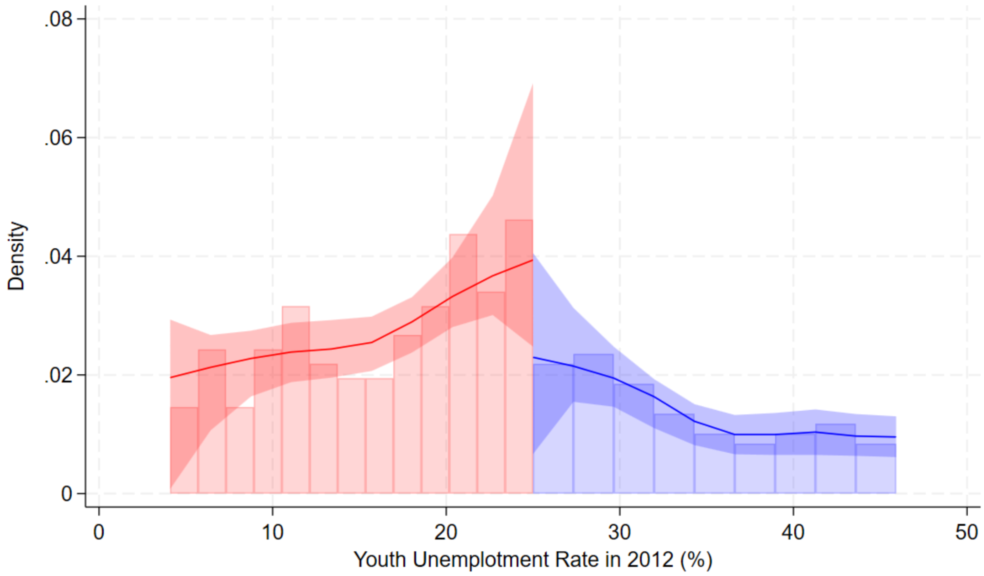


**Figure 5:** RDdensity visual display

Similarly as in the McCrary test, Figure 5 presents a visible discontinuity at the 25% cutoff point. The density appears to be higher just below the cutoff (red area) compared to just above it (blue area). This suggests a potential clustering of observations just below the 25% threshold. This discontinuity at the cutoff point raises concerns about potential manipulation of the running variable. However, the results slightly change compared to the estimation results in the McCrary test. In this case, the estimation provides a t-statistic of -2.31 and a p-value of 0.02 which suggests significant manipulation at the cutoff. Nonetheless, the robust t-statistic and p-value of RDdensity test provide insignificant values, these being -1.57 and 0.12 respectively. Therefore, the manipulation concerns can also be alleviated when considering the robust values of the t-statistic and p-value of the RDdensity test.

| | (0) | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| | | +1 | -1 | +5 | -5 |
| Cutoff point | 25 | 26 | 24 | 30 | 20 |
| p-value | 0.02** | 0.03* | 0.10* | 0.00*** | 0.16 |
| Bias corrected p-value | 0.12 | 0.29 | 0.40 | 0.91 | 0.75 |
| T-statistic | -2.31 | -2.11 | -1.66 | -3.96 | 1.39 |
| Bias corrected t-statistic | -1.57 | -1.07 | -0.84 | 0.11 | 0.33 |
| Effective sample size right of cutoff | 38 | 30 | 67 | 31 | 51 |
| Effective sample size left of cutoff | 64 | 54 | 100 | 80 | 33 |
| Bandwidth | 6.97 | 5.49 | 13.80 | 10.15 | 4.90 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

**Table 13:** RDdensity, different cutoffs

Furthermore, a robustness check was also developed for the RDdensity test. Focusing primarily on the bias-corrected (robust) statistics, we observe that none of the cutoff points show statistically significant manipulation at conventional levels when using the bias-corrected p-values. The 26 and 24 thresholds show bias-corrected p-values of 0.29 and 0.40 respectively, with correspondingly lower t-statistics in absolute terms. The more distant thresholds at 30 and 20 also fail to show significant discontinuities after bias correction, with p-values of 0.91 and 0.75. It is worth noting that before bias correction, some of these thresholds appeared significant at the cutoffs 26 and 30. However, the robust p-value and t-statistics suggest these initial findings may have been influenced by bias in the original estimates. Overall, these results suggest that even though there might be some initial indications of discontinuities, these effects become statistically insignificant with robust statistics. This provides some reassurance against manipulation of the running variable, in line with the findings of the McCrary test.

## Appendix 6: differences in RDrobust estimators

RDrobust uses a total of three different estimators: conventional, bias-corrected and robust. The conventional estimator does not adjust for bandwidth choices, and it simply estimates the effect of the treatment using triangular and polynomial weights, as well as the difference between the treatment and control group within the bandwidth. Then, the bias-corrected estimator adjusts for estimation bias by using two bandwidths: one for the initial estimate and another for the bias correction (Calonico et al., 2012). This method subtracts an estimated bias term to improve the accuracy of treatment effect estimates and adjusts the standard error to reflect this correction. Lastly, the robust estimator uses a new standard error formula to account for the additional variability introduced by bias correction, offering more accurate confidence intervals in practical scenarios with limited sample sizes and non-ideal bandwidth choices (Calonico et al., 2020).